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The political economy of media production and consumption in France

Moritz Jonas Hengel

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Institut d'études politiques de Paris
ÉCOLE DOCTORALE DE SCIENCES PO
Programme doctoral en économie
Département d'économie
Doctorat en sciences économiques

The Political Economy of Media Production and Consumption in France

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submitted on July 28, 2023

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¹The now former open office of the PhD students in the attic of Sciences Po

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Paris

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Note to the Reader

The four chapters of this dissertation are self-contained research articles and can be read separately. They are preceded by an introduction which summarizes the research presented in this dissertation. The terms “paper” or “article” are used to refer to chapters. Chapter 2, 3 and 4 are co-authored, which explains the use of the “we” pronoun.

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Introduction

THE patterns of media consumption and production are changing at a breathtaking place. On the production side, journalists as traditional gatekeepers of high-quality information see their business model threatened by social media, where any consumer can be producer of information as well. On the consumption side, voters can now strongly personalise the media they consume: Where a newspapers and (public) broadcasters bundle politics with entertainment, social media algorithms target all content to the individual preferences of consumers maximise interaction of users.

Politicians and business owners are adapting quickly to these changes to maximise profits and vote shares respectively. For new populist movements, social media presents an opportunity to sway voters beyond the traditional media that is dominated by established parties. Similarly, media owners are pushing the boundaries between profit maximisation and unbiased, fair reporting at the same time as big social media platforms navigate a fine line between content moderation, fact checking and maximising user interactions.

These tectonic shifts in the economic foundations of the media industry have important consequences for voters decision-making and preference formation that this thesis sets out to study in four chapter.

The first chapter focuses on media consumption and populism. It studies a historic natural experiment in Germany to understand how right-wing populist movements exploit media consumption patterns to sway voters. During the division of Germany, West German TV was a key source of uncensored information in Socialist East Germany (1949-1990) - so important that places without it became known as the “valley of the clueless” (*Tal der Ahnungslosen*). I show that, to date, these places consume less TV and rely more on social media to inform their voting decisions. Yet, these differences only become politically relevant with the entry of the right-wing populist party *Alternative for Germany* (AfD) in 2013: Comparing close and similar municipalities, places without historic exposure to West TV robustly have a 1.7 – 2% percentage point higher vote shares today for the AfD, which corresponds to 12%

of its mean. Using Facebook user data, I show that the AfD’s entry strategy of dominant social media campaigning pays off twofold: First, people are engaging more with the AfD’s populist narratives on social media. Second, differential access to uncensored media in the past impacts present-day media literacy, as recent survey data from the Covid pandemic suggests. This can explain greater persuasiveness of the AfD’s populist narrative.

The second chapter, which is based on a paper co-authored with Julia Cagé, Nicolas Hervé and Camille Urvoy, focuses on media pluralism and the tension between media owners, journalistic independence and unbiased reporting. Democracies need informed voters – voters who are exposed to a diverse range of views. News media take an active role in the process of informing voters; yet, they vary in their coverage of political parties. In this paper, we exploit an exhaustive, rich dataset on the production of French TV and radio broadcast from 2002 to 2020. Using the invitation patterns of politicians and other guests with outspoken political views, we shed light on changing power dynamics between profit-maximising media owners and journalists that are negotiating their editorial independence. In particular, we explore whether differences in political coverage are mainly driven by the editorial choices of (a few) owners, or by the preferences of diverse journalists, provided that they have some agency. To do so, we build a novel dataset on millions of French television and radio shows over 20 years, with information on the identity of hosts, guests, and guests’ political leaning. We estimate a two-way fixed effects model identified thanks to the many hosts working on multiple channels. We show that hosts largely comply with outlet-level decisions, which account for 85% of cross-channel differences in political representation. Complementing these results, we study how hosts adapted to a major ownership-driven change in editorial line, and find that the hosts who stayed after the takeover complied with the new owner’s preferences.

The third chapter is based on a paper co-authored with Julia Cagé and Yuchen Huang and studies how political preferences of far-right voters interact with their attitudes towards charitable donations. It first documents a widespread decline in the share of donors to charities in Western countries over the past decade, and show that this can be in part explained by a lower propensity to donate among far-right voters. Focusing on France, we first conduct a large-scale survey ($N = 12,600$) and show that far-right voters are significantly less likely to report a charitable donation than the rest of the population, conditional on a rich set of controls. Second, using administrative tax data for the universe of French municipalities ($N \simeq 33,000$) combined with electoral results, we find that the negative relationship between vote shares for the far right and charitable donations holds in a broad range of specifications, at both the extensive and the intensive margin, and controlling for municipality fixed effects.

Third, we exploit unique geo-localized donation data from several charities and document similar patterns. All evidence points towards a drop in the propensity to donate driven by a shift in social norms that threatens the general acceptance of the charitable sector.

The fourth chapter is ongoing work with Julia Cagé, Emeric Henry and Nathan Gallo to investigate how big social media platforms tackle misinformation. We address this question by building a unique partnership with the “*Agence France Presse*” (AFP), the largest fact-checking organization in the world. For 18 months, we collected the stories proposed by fact-checkers during the daily editorial meetings, some of which are ultimately fact-checked while others, despite being ex ante “similar”, are left aside. Using a Difference-in-Differences approach, we show that Facebook posts related to stories that are fact-checked receive 26 – 30% fewer shares compared to stories that were considered but not ultimately fact-checked. Moreover, we document that journalists, due to a time constraint, do not rate all posts associated to a story, despite them being mostly identical in content. We leverage this for a second with-story identification strategy on the post level, where we find important spillovers of ratings on unrated posts which are deleted pre-emptively by users. We draw on our unique data on the production of fact checks to formulate policy recommendations to improve the efficiency of fact-checking.

Chapter 1: “The Valley of the Clueless?” Media Consumption and Populist Persuasion

In this chapter I investigate the role of media consumption in the rise of populism. Amidst the rise of right-wing populist parties in many Western democracies in the 2010s, understanding the role of media consumption of voters has become crucial to explain how new populist competitors vie for votes with established parties. Several studies have addressed the short and medium run impact of media consumption on populism. However, few can exploit long-lasting exogenous variation media consumption patterns to explain how populist persuade voters through their media diet.

In this paper, I leverage a historic natural experiment in East Germany to address this question. During the time of division (1945-1990), most East Germans had access to West German TV via terrestrial signal. Technical feasibility and the absence of language barriers allowed East Germans to tune in easily who did so extensively for entertainment and uncensored information. However, some 15% of East Germany were cut off from West TV, becoming known as the “Valley of the clueless” in East German popular culture (“*Tal der Ahnunglosen*”). As the name suggests, the exact local TV availability was determined by

geographic features like elevation and forests and varied between neighbouring municipalities which made TV reception impossible around a signal strength cut-off. In addition, there were municipalities in various regions without access to West TV, and overall areas with and without access to West TV were very similar to each other. When Germany reunified in 1990 under West German institutions, most of East Germans had thus been exposed to the media of a political system they would then unexpectedly and immediately become part of. Twenty-three years later, since 2013, Germany's first successful right-wing populist party, the *Alternative für Deutschland* (Alternative for Germany, AfD), celebrates its largest electoral successes in East Germany more generally and even more so in areas without historic access to West TV.

As a first step, I show that differences in media consumption patterns persist until today and continue to shape the type of media that voters consume to inform their voting decisions. Voters in areas without access to West TV pre-1990 are to this day less likely to watch public TV to inform their voting decisions. Instead, these individuals turn more often to social media for information on politics. Differences in TV news consumption exist only for the two public broadcasters that were available during the time of division, but there are no differences for private TV stations that entered the market just before or after reunification, which indicates that these patterns are directly linked to the historic access to West TV. The increased propensity to use social media instead is still noticeable today, for example in terms of the sources that individuals consulted about the Covid pandemic.

Second, I show that the lack of historic exposure to West German TV is associated with an increase in the vote share for the right-wing populist AfD since its foundation in 2013. In terms of identification and methods, I improve on the previous literature by exploiting fine-grained municipality-level signal data, controlling for state fixed effects and the distance to the former border with West Germany, thus comparing only close municipalities with different historic West TV exposure. The results are highly robust across a broad range of specifications, in particular to using a robust regression discontinuity design (RDD) around a signal reception cut-off and using two different individual-level surveys. They are also robust to controlling for internet or IT signal technologies that could use similar antennas as West TV and affect voting (Zhuravskaya et al., 2020; Falck et al., 2014; Gavazza et al., 2019). Using novel data from (Cantoni et al., 2019), I further improve on the existing literature by showing that municipalities (rather than districts) were mostly well balanced pre-treatment and that any controlling for pre-treatment differences does not affect the estimation.

An event-study design shows that this effect is specific to the AfD and did not translate into differences in voting for other right-wing populist parties in the pre-internet era. The

effect is also much more pronounced after the AfD’s populist turn in 2015, when it switched from a fiscally conservative, eurosceptic party to an anti-immigration, anti-establishment and anti-media platform with a strongly pronounced populist rhetoric.

Third, I provide evidence that this populist rhetoric is more persuasive because of the AfD’s stronger online presence. During the study period, the AfD is the dominant party on German social media. For example, it generated more shares on Facebook in the 2017 election than *all other parties combined* (Stier et al., 2018). Using data from Müller and Schwarz (2021), I show that the AfD is more present in areas without historic exposure to West German TV. Moreover, this presence is more impactful in these areas after its populist turn, but only for actions that induce a spread of its narratives (posts) and not for mere expressions of approval (likes).

Next, I employ survey data on agreement with conspiratorial statements during the Covid pandemic to show that respondents in areas without historic exposure to West TV are more likely to agree that “the Media manipulate information”, but not with other conspiracy theories. While the AfD generally pushes conspiratorial narratives, its (Germany-specific) narrative about the “lying press”, a Nazi propaganda term, seems particularly effective in these areas that could not access the uncensored West German TV as an information source under the social East German regime.

Finally, I turn to a rich household survey to further disentangle the mechanism and study other potential differences in attitudes. Despite a large sample size and various variables covering a long time horizon, I am unable to detect persistent differences in right-wing attitudes, which is in line with the event study finding that the effect is specific to the AfD and the internet and not right-wing extremism in general ¹. Instead, I find evidence that voters are less knowledgeable about politics, which supports the notion that they are less critical in their media consumption. I further leverage my improved identification strategy to revisit earlier findings from the literature. Amongst other, I find that, while Kern and Hainmueller (2009) were right about the positive well-being effects of TV access (see also Chadi and Hoffmann, 2021), it did not translate into increased support for the Socialist East German regime as proxied by satisfaction with life and democracy under the regime in a 1990 survey before reunification.

The findings underline the importance of media consumption patterns in the rise of right-wing populism. First, similar to many challengers to the system (Durante et al., 2019), the AfD leverages social media as a campaigning ground. This strategy is far more effective if

¹Note that this is in contrast to Hornuf et al. (2023), probably due to the less precise treatment assignment and the specific sample and selective questions used. I will return to this below.

faced with an electorate that relies more on social media as an information source to begin with. Moreover, the populist narrative that attacks the established media is thus much more attractive to the electorate. The demonstrated effects are sizeable and, if scaled up to the entire population, play an important role in explaining the rise of right-wing populism in East Germany.

Chapter 2: Hosting Media Bias - Evidence from the Universe of French Broadcasts, 2002-2020

In this chapter, co-authored with Julia Cagé, Nicolas Hervé and Camille Urvoy, we investigate how, for democracies to function, voters need to be exposed to a plurality of views (Pariser, 2011). For this reason, regulators in many countries have sought to preserve pluralism in news media. With the idea that media ownership may influence editorial lines, they have promoted ownership diffusion across competing outlets (external pluralism).² They have also created rules requiring that each outlet features a balanced representation of political forces, thereby setting bounds to channel editorial policies (internal pluralism).³ While today people can access a virtually infinite number of opinions, reach and attention patterns are such that they are actually exposed to a reduce set of news sources, themselves controlled by a small number of conglomerates (Prat, 2018; Kennedy and Prat, 2019). It has raised concerns that some media tycoons may disproportionately influence the political process, and renewed discussions on media concentration and polarization.⁴

Contrasting with the small number of owners, there are many journalists and hosts in charge

²In the Unites States, the Federal Communications Commission (FCC), designed regulations in line with its mission to ensure “*the diversity of viewpoints from antagonistic forces.*” The US Supreme Court has supported the “*assumption that diversity of ownership would enhance the possibility of diversity of viewpoints*” (Fisch, 2010). The European Commission writes that: “*independent media, and in particular news media, provide access to a plurality of views and are reliable sources of information to citizens and businesses alike. They contribute to shaping public opinion and [...] are essential for the functioning of our democratic societies and economies.*” In case of mergers or acquisitions, the Commission recommends to assess “*the impact of the concentration on media pluralism, including its effects on the formation of public opinion*” (COM/2022/457).

³In the US, the 1949 FCC fairness doctrine required that media with a broadcast license give the public “*a reasonable opportunity to hear different opposing positions on the public issues of importance and interest in the community*” (Fisch, 2010). In France, the Regulatory Authority for Audiovisual and Digital Communication (ARCOM) monitors the equity and diversity of political expression on broadcast media. Most European country have some kind of internal pluralism rules (see “*Internal Media Plurality in Audiovisual Media Services in the EU: Rules and Practices,*” ERGA Report, 2018).

⁴The literature provides evidence that media content can be impacted by ownership (Durante and Knight, 2012b; Martin and McCrain, 2019; Mastrorocco and Ornaghi, 2020, for instance), and that media content impacts voters’ behaviors (DellaVigna and Kaplan, 2007; Bursztyn et al., 2020; Moreno-Medina et al., 2022, among others).

of the daily production of media content. Their diversity – in terms of specialization, views or backgrounds – is a potential source of pluralism, provided that they have some agency vis-à-vis their employers’ editorial policies. In today’s world, engaging directly with their audience online may for example give them leverage and independence,⁵ while employment insecurity may be a disciplining force, pushing them to conform to the editorial policy of their outlet. Furthermore, journalists may chose their employers based on political affinity, which may amplify each outlet’s tendency to prioritize certain views.

In this paper, we study how much agency hosts have regarding opinion representation in their shows. We examine an important choice they have to make on a recurrent basis: who to invite. To do so, we use novel show-level data on French broadcast between 2002 and 2020 and track hosts as they work for distinct outlets over time. We estimate to what extent differences in representation of political views across channels are driven by host-level decisions on the one hand, and hosts adapting to the channel they work for on the other hand. We complement this quantification exercise with a case study. We track how hosts reacted to a major owner-induced change in editorial line around the 2015 takeover of three television channels by the Vivendi conglomerate, owned by the so-called “French Murdoch,” Vincent Bolloré.

As in many countries, media power in France is concentrated in a relatively small number of news outlets, with television and radio being at the center stage of the news ecosystem (Kennedy and Prat, 2019).⁶ Outlets topping the lists of main news source among French respondents are television channels, ahead of social media like Facebook (4%). In 2019, 71% and 53% of the respondents reported that they got their daily news from television and radio respectively, compared to only 47% online (Sumida et al., 2019). Our data includes all the major news sources: it comprises all the most consumed television and radio outlets from 2002 to 2020, with detailed show-level information compiled and enriched by the National Audiovisual Institute (INA) archives. The 2.1 million shows in our data are not restricted to newscasts, but also include talk and entertainment shows.⁷ They feature 39,322 distinct hosts and more than 260,413 distinct guests. With the ample time frame covered, we can track hosts as they move from an outlet to the other and observe how they adapt to their new work environment upon move. Data granularity ensures we can finely control for viewership composition and news events at the time each show airs.

⁵Respectively, 21% of US and 29% of French respondents report paying more attention to the journalist than to the news brand when consuming news online (Reuters Institute, Digital News Report, 2022).

⁶See also Newman et al. (2022).

⁷We include all the shows with at least one host and one guest. We do not include fictions and sport games.

We first document that political forces are unevenly represented across channels.⁸ For instance, on average during our time period, left-wing parties account for 40% of the speaking time on LCI, but 60% on France 4. To show this, we classify guests by political leaning in six groups (radical left, green, left, liberal, right, radical right). We use lists of candidates running in elections and lists of government appointees to identify politicians. Given the increasing coverage they receive in talk shows, we also classify guests who are not politicians in a strict sense, but are politically vocal (activists, think tank commentators, public intellectuals, etc.).⁹ To do so, we rely on think tank contribution or affiliation, endorsements, and party-event participation. Overall, we classify 16,380 distinct individuals, accounting for 661,295 appearances (of course, we allow the political leaning of the guests to vary over time). From there, we can compute the screen time share of each political group at the show- or channel-level.

What explains the differences in political coverage across channels? One explanation is that channels have distinct editorial policies, to which hosts comply by adapting their invitations to the channel they work for (*contextual* factors).¹⁰ Another is that channels employ distinct hosts on average, who invite distinct guests, potentially due to the hosts’ preferences or specialization (*individual* factors). We estimate the relative role of contextual and individual factors in a two-way fixed effects model that allows channel effects to vary over time (Lachowska et al., 2022). We regress the political time share of a given host at the show level on host fixed effects, channel-times-period fixed effects¹¹, and media platform (radio or television), date, and hour fixed effects. Time fixed effects capture news shocks, potentially making one party more news-worthy than the others at a given moment of time (e.g. because there is a change in the leadership of the party), as well as viewership by controlling for the characteristics of potential viewers or listeners for each hour of each day, by media type. Among the 14,492 hosts in our data who invited politically-classified guests, 9,810

⁸The diversity in coverage is clearly visible despite the regulatory agency’s guidelines requiring channels to represent political forces ‘equitably,’ which here means in proportion to their contribution to the political debate (see Section 2 for more details on the institutional background). The differences that prevail nonetheless partly reflect the ambiguity and weak enforcement of this rule.

⁹We call “public intellectuals” here all the intellectuals that are publicly “engaged”, in the sense of the French expression “*intellectuels engagés*”. As will appear clearly from our empirical results, in recent years, media owners have increasingly substituted talk shows to news programs, both to reduce costs (Cagé, 2015), but also as a way to escape broadcast regulation on pluralism.

¹⁰Distinct editorial policies can be driven either or both by supply-driven or demand-driven factors. We come back to this point below.

¹¹Here, each period corresponds to two ‘seasons’, where seasons are one-year periods from September to August, so as to match the time frame media outlets use to plan their shows or to adjust their programs. In the spirit of the rolling AKM approach (R-AKM) proposed by Lachowska et al. (2022) – and to allow for time-varying channel effects – we indeed estimate the model separately for successive two-season time intervals.

are observed working on at least two of the 20 channels in our sample. Changes in who they invite as they move from one channel to the other reveals to what extent they adapt the content of their shows to their employer. In other words, if hosts moving from channel C to channel C' systematically invite more left-wing guests upon move, everything else equal, we interpret it as a sign that channel C' prioritizes left-wing guests with respect to C. We can also estimate the extent to which hosts have agency with respect to their outlet's editorial policy. If hosts keep inviting an above average share of right- or left-wing guests as they move from one outlet to the other, it implies that they also partly contribute to slanting shows, potentially based on their own preferences or specialization. We investigate whether the agency hosts have varies with their observable characteristics.

We show that hosts largely adapt who they invite based on which channel they work for. According to our estimations, when moving to a channel that grants 1 extra percentage point of screen time to a political group than their origin channel, they increase their coverage of this group by 0.63 percentage points on average. We decompose differences in political representation across channel-period pairs using our two-way fixed effects model. Based on the linear decomposition, channel-level decisions are crucial and explain 87% (respectively 90%) of the differences in left-wing (respectively right-wing) parties time share. Host characteristics account for the remaining 13% (10%). A variance-decomposition exercise leads to similar conclusions – channels account for around 82% (85%) of the difference for the left (right) – while highlighting host sorting: covariance between channel and host effects account for 16% (13%) of the variance. Host effects only explain the remaining 2.2% (2.1%). Hosts therefore largely comply to channel-level editorial policies. This finding sheds new light on the mechanisms through which media slant happens, by quantifying the relative role played by owners and hosts.

Analyzing trends over time, we find that the dispersion of channel effects increased over the sample period, which can be seen as reflecting polarization in editorial policies. One reason for this may be that profit-maximizing owners specialize each channel ideologically; another is that owners want their channels to prioritize certain views ([Gentzkow and Shapiro, 2010](#)). We find that, within owner, channels often tend to prioritize the same political forces, suggesting that the latter explanation might be at play.

We then explore the factor that predicts hosts over- or under-representing certain political groups. Female hosts and hosts who are more central to the political guest-host network tend to deviate more to the left relative to their channel, but the effect is small. Interestingly, when looking at absolute deviations from the channel line, we find that hosts tend to deviate more if they are more famous as proxied by their total screen time, the existence of a Wikipedia

entry, or the number of times they interview the ruling President.¹² At the same time, hosts who work as journalists on channels, who are more central to the political host-guest network and who have more political screen time tend to deviate less in absolute terms from the channel line. This suggests that journalists specialized in politics follow more closely the outlet’s editorial line, while more famous hosts are allowed to deviate more from it.

In the second part of the paper, we focus on a large owner-induced change in editorial policy, and study two hosts’ response margins: complying or leaving. In 2015, Vincent Bolloré – a French billionaire often compared to Rupert Murdoch – became the main shareholder of the Vivendi conglomerate, the parent company of the Canal Plus group, which owns several television channels. Journalistic accounts of the event have highlighted the proximity of Vincent Bolloré with conservative figures, and noted shows swiftly moved rightwards (see also [Capozzi, 2016](#); [Cagé, 2022](#)). We compare Vivendi channels to others in our sample before and after the takeover. Our event-study specification includes host-channel fixed effects, meaning that we exploit within host-channel variations. After the takeover, we show that right-wing parties’ screen time share increased by 5.5 percentage points, and that the one of left-wing parties decreased by 6.8 percentage points. We find no evidence of diverging pre-trends. Hosts who remained on the acquired channels adapted the content of their show to the new editorial policy implemented after the takeover.

We further analyse whether hosts left the channel in response to the change in editorial policy. We find that the probability that a host stays decreases by 15 percentage points following the takeover, from a 38% baseline. The effect is driven by hosts who invite political guests, who have above median political screen time, who are credited as ‘journalists’ and whose shows are newscasts. It suggests that hosts who were the most exposed to the change in editorial policy were precisely the ones most likely to leave. Male hosts, famous hosts, and hosts with higher ratings are more likely to stay in the medium run. Regarding hosts who leave, a majority of them is no longer observed on one of the channels in our sample following the takeover, suggesting their career has been negatively impacted.¹³ Those who work on another channel are more likely to work on a channel that represents the right relatively less, hinting at potential sorting on editorial policy.

¹²In France, the President tends to grant very few interviews, contrary to the US for example where the President holds regular press conferences. Interviewing the President can thus be seen as a form of “consecration” for a French journalist.

¹³This is consistent with existing anecdotal evidence documenting that a large share of the former journalists working for the news channel acquired by Bolloré have quit journalism (which is unfortunately not a surprise in a context where the overall number of journalists in France is declining).

Chapter 3: The Far-Right Donation Gap

In the third chapter, which is joint work with Julia Cagé and Yuchen Huang, we document the importance of a far-right ideology for the propensity to donate to charities.

Although the 21st century is often being presented as the “age of philanthropy”¹⁴ with an unprecedented increase in the amount of charitable giving, we show that the *share* of the population donating to charities is declining in many Western democracies. This drop – concomitant with the electoral rise of far-right parties in many of these countries – poses a threat to the charitable business, as giving increasingly relies on a small number of individuals. In this paper, we provide novel evidence on the relationship between political ideology and charitable donations. Specifically, drawing on insights from rich survey data, geo-localized tax data, and charity records, we show a significant and persistent donation gap among individuals who align themselves with far-right political ideologies. We investigate whether this gap may lead to a further reduction in charities’ donor base in the years to come.

To document what we call the “far-right donation gap” – the fact that far-right voters are significantly less likely to donate to charities than other citizens, even relative to people who abstain – we proceed in three steps. First, we run a large-scale pre-registered survey ($N = 12,600$) one week before the 2022 presidential elections in France, where we ask respondents about their past and future donations. According to our findings, Marine Le Pen’s (far-right) voters are 6 percentage points less likely to make a charitable donation than citizens who abstain, and Eric Zemmour’s (far-right) voters are 4 percentage points less likely. On the contrary, both Jean-Luc Mélenchon (left) and Emmanuel Macron’s (center) voters as well as supporters of all the other parties on the left and right of the political spectrum are more likely than abstainers – by 6 to 20 percentage points – to contribute money to a charity. Thus, while voting is generally associated with a higher propensity to donate relative to abstention, the reverse is true for far-right voters (Yen and Zampelli, 2014).¹⁵

On top of income, these findings are robust to controlling for a large number of demographic observables, such as the age of the surveyed individuals, their gender, marital status, religion, life satisfaction, trust, pessimism, as well as the size of the city where they live. It is also

¹⁴See e.g. <https://www.theguardian.com/world/2021/jun/18/a-million-dollars-a-minute-the-rise-and-rise-of-philanthropy>.

¹⁵Unfortunately, we do not have information on whether far-right voters devote more or less time (e.g. through volunteering) to charities compared to other voters. We indeed only have information on monetary contributions. However, in the last section of the paper, when dealing with external validity, we show that the probability of making a blood donation is also lower for far-right voters in Germany.

robust to using the surveyed individuals’ self-placement on a left-right scale, furthermore showing that the negative relationship between far-right voting and donations we document is specific to right-wing extremism and not to political extremism in general. More importantly, the size of the far-right effect does not vary when we control for additional observables, suggesting that the far-right donors gap is structural. We are also able to reproduce the same finding in similar survey data for Germany, which shows that the far-right donation gap is not specific to France.

Survey data may suffer from a number of concerns, in particular regarding social desirability bias in reporting. To address these concerns, we leverage detailed administrative data on tax-deducted charitable contributions (Cagé and Guillot, 2021) for 33,037 French municipalities¹⁶ between 2013 and 2019, and compare them with the vote shares obtained by each of the candidates in these municipalities in the first round of the presidential elections, controlling for a large set of city-level socio-demographic variables, including the local supply of charities. We find that a 10% increase in the vote share obtained by Le Pen in a municipality compared to abstention is associated with a 1.9% decrease in the share of households declaring a charitable donation on their tax return. Importantly, the magnitude of the estimated effect is consistent with the one we obtain when using the survey data; furthermore, we show that this effect happens at both the intensive and the extensive margin. We also find that this finding is robust to using the panel dimension of the administrative tax data – with two presidential elections that took place during our period of interest (in 2012 and 2017) – and thus to controlling for municipality fixed effects.

While most individuals declare their donations on their tax form to benefit from tax deductions, not all of them do so. To overcome this limitation of the administrative data, we finally obtain detailed information on donations received (with the precise date of the donation and the location of the donor) by three large charities in France: “Action Contre la Faim” (ACF), SOS Méditerranée (SOSM), and Oxfam. Using these data – similarly merged with the electoral results at the local level – we find that a 10% increase in Le Pen’s vote share in a municipality compared to abstention is associated with a 1.9% (ACF) to 0.1%5 (Oxfam) percent decrease in the amount donated to these charities per household. In other words, even if the magnitude of the elasticity of the donations with respect to Le Pen’s vote is smaller for smaller charities like Oxfam and SOSM, the far-right donation gap remains both economically and statistically significant independently of the data source or of the estimation methods used.

¹⁶This represents nearly the universe of French municipalities ($\simeq 36,000$), except the very small ones due to statistical secrecy.

What explains the negative relationship between far-right ideology and charitable giving? As highlighted above, our results are robust to controlling for pessimism, unhappiness, and (the lack of) trust at the individual level. Thus, while these factors have been associated with far-right voting (see e.g. [Algan et al., 2017, 2019](#); [Giuliano and Wacziarg, 2020](#); [Guriev and Papaioannou, 2022](#)), they cannot drive our findings. This suggests that there may be some unobservable characteristics that are simultaneously associated with far-right voting and a lower probability of making a charitable donation.

Inspired by the far-right criticism of charities as “*universalism without borders*”, we hypothesize – in the spirit of [Enke \(2020\)](#) – that far-right voting is associated with a lower propensity to donate to charities through the underlying moral values of far-right voters, specifically a sense of “communal” morality, which allocates more altruism to the in-group members than to the out-group members of society.¹⁷ On the one hand, an individual displaying communal morality would be less likely to donate to “distant” charities, since the identity of the recipient is by definition unknown and likely to be out-group; on the other hand, the individual would also be more likely to identify with the far-right parties.¹⁸

We provide evidence in support of the communal morality hypothesis using information from the supply side of charity. Using the National Directory of Associations, we locate all the existing charities in France. We separate the charities into “global” and “non-global” using their stated purpose: a charity is categorized “global” if its purpose explicitly mentions places in a foreign country or contains keywords such as “global” or “universal”. We show that the interaction between the far-right effect and the percentage of global charities in a municipality is statistically significant and negative: in municipalities where charities have a globalist outlook, the far-right donors gap is wider. On the contrary, in municipalities that contain more non-global (local) charities, far-right voters are less hesitant to donate, with the reverse being true for more centrist voters.

In addition, we discuss anecdotal evidence that far-right politicians have become increasingly critical of the charitable sector and its “universalism without borders,” in particular compared to other parties. We next show for a sub-sample of our tax data that the elasticity of charitable donations with respect to political donations is negatively associated with the far-right vote. In other words, the higher the far-right vote, the more political and charitable donations are negatively associated, suggesting that far-right voters perceive them more as substitutes than the rest of the population. This is in line with the reading that far-right

¹⁷Moral values correspond to people’s deep beliefs about what is right and wrong. See also ?.

¹⁸In the context of multi-party election systems such as the ones we observe in the majority of the Western democracies – and contrarily to the US, we think that it is the far right that appeals to people holding communal morality rather than the traditional right. We come back to this point below.

voters substitute charitable donations with financing far-right politicians that push their communal values through policy.

We also show that the far-right donation effect is not driven by social desirability. It is indeed stronger in municipalities where the share of far-right voters is smaller (i.e. in municipalities where voters should theoretically suffer from more stigma if they do not donate).

Finally, we discuss the decomposition and implications of the far-right donation gap. While the far right donation gap persists when we exploit the panel dimension of our tax data and control for municipality fixed effects, it is smaller in magnitude. Hence, we show that the donation gap is not only driven by people who newly converted to the far-right ideology but also by those whose affiliation with the far-right is more deep-rooted. In particular, we show that cities that voted more for the far right in 2012 experienced a much sharper drop between 2013 and 2019 in the share of households donating unexplained by changes in city-level characteristics. We provide additional evidence using survey data and show that the 2022 far-right donation gap is largely driven by people who were already voting for Le Pen in the previous 2017 presidential elections. These results point toward the fact that the decreasing trend in charitable contributors is driven by both the *intensification* of the existing preferences of people with communal morality, as well as by the *adoption* of people who did not possess communal morality before. Given the increasingly persistent electoral success of far-right parties, this poses a threat to the charitable sector in two ways. First, the stronger and more persistent the electoral success of far-right parties, the greater the chance that donations will decline. Second, a shrinking donor base at the extensive margin undermines the democratic legitimacy of large public subsidies that benefit charities in most countries.

Chapter 4: Fact-Checking and Misinformation: Evidence from the Market Leader

In this last, ongoing chapter, which is based on a paper co-authored with Julia Cagé, Emeric Henry and Nathan Gallo, we study if fact-checking is efficient at reducing the spread of misinformation and how it does affect the behavior of users who circulate false information.

While the fact-checking industry has been rising in recent years due to global concerns about fake news (Allcott and Gentzkow, 2017; Allcott et al., 2019), the impact of fact-checking is still under intense scrutiny. The literature provides strong evidence that, while fact-checking is unable to correct beliefs or voting intentions, it is effective in reducing circulation

(Pennycook et al., 2020a,b; Henry et al., 2022). However, most of the papers in the literature use controlled lab in the field experiment, that cannot document dynamic effects on the behavior of participants.

To address these questions, we rely on a unique partnership with the “*Agence France Presse*” (AFP), the third largest news agency in the world and the world’s largest fact-checking organization. A journalist was hired for 18 months to attend the daily editorial meetings of “*AFP Factuel*”, the AFP’s unit working on fact-checking the news in French language. She collected information on all the stories that were discussed during the daily meetings, those that were approved and fact-checked and those that were left aside. She also recorded the reasons for rejections (lack of resources, lack of virality, etc.) based on regular meetings with the *AFP Factuel*’s chief editors.

The *AFP* is a member of the “Third-party fact-checking program” set up by Facebook.¹⁹ This gives journalists direct access to the Facebook tool where they can rate posts directly once a fact-check is produced. It also gives access to the so-called “Facebook claim”, which contains a list of suspicious posts automatically detected by Facebook using algorithms. Importantly, the agreement with Facebook does not provide incentives to systematically rate all posts that relate to the same fact-checked misinformation. For each of the stories, fact-checked or not, the journalist we hired also collected information on the associated posts rated and non-rated by the “*AFP Factuel*”’s journalists.

This unique data collection effort allows us to build an original identification strategy to identify the causal effect of fact-checking on the circulation of misinformation. We use two approaches, one at the story level (controlling for story and time fixed effects) and the other one at the post level (controlling for story-time and post fixed effects). These two distinct approaches address different questions. At the story level, we use a Difference-in-Differences (DiD) approach, comparing stories that were fact-checked to “similar” stories that were initially considered but left aside, in particular due to a lack of resources. Our preferred outcome variable is the (logarithm of the) sum of posts related to the stories on Facebook. The key identifying assumption is that the two types of stories would have had similar trajectories in terms of circulation absent the fact-checking intervention. To ensure the validity of this identifying assumption, we impose two restrictions on the data exploiting the details of the editorial process. First, we exclude the stories that were not fact-checked because of lack of virality.²⁰ Second, we exclude stories that were fact-checked even though

¹⁹There are 123 accredited organisations worldwide, for example Reuters and *The Washington Post* in the US, or *Le Monde* and *Libération* in France.

²⁰There are 5 main reasons for not fact-checking a story: (i) a lack of resources, (ii) a lack of virality of the story, (iii) the fact that the story is probably true, (iv) the fact that the fact-check would be infeasible,

the journalist proposing the story was already working on another fact-check at the time, since for those the cut-off to accept the story is higher.²¹ Importantly, consistent with our identification assumption, we show that both the fact-checked and the non fact-checked stories were facing similar popularity trend before being first considered by the AFP.

The second identification strategy uses only fact-checked stories and, for each story, compares the posts that were rated to those that were not. As explained above, the agreement with Facebook does not provide incentives to systematically rate all posts. Rating additional posts linked to the story is up to the willingness of the journalist who fact-checked the story. Working together with the *AFP Factuel* team allowed us to understand that journalists rate as many posts as they can but often not all of them due to a lack of time. Our identification assumption here – based on extensive discussions with AFP staff – is that the last post that is rated is “similar” to the first one that is not, i.e. that the fact-checker decides to stop rating posts at some point for random reasons. Consistently with this assumption we show that the posts that are and are not rated were following parallel trends in terms of number of shares on Facebook before the AFP Factuel team first considered the story.

Our preliminary results show that fact-checking reduces the circulation of the posts related to fact-checked stories. The story-level identification strategy shows that this reduction is significant – both from a statistical and from an economical point of view. On the story level, a fact-check reduces the circulation of associated posts by 26 – 30% relative to the control group. We find that this is driven by fact checks that are published fast for misinformation that was discovered quickly, but is only half as efficient if the process takes long. On the post level, we show that rating posts has important spillover effects on unrated posts, with accounts deleting their posts before they are rated to avoid downranking penalties by Facebook. Hence, our result reflect the combination of an enforcement effect by Facebook reducing circulation and a behavioral response by users.

Together with the descriptive evidence that we gather, we find several policy-relevant margins to improve the efficiency of fact-checking. First, we argue that, since speed matters, fact-checkers should be equipped with better tools to find misinformation, since speeding up the writing process itself may come with a trade-off with respect to the fact check quality. Second, we document that – in lack of properly working tools provided – the currently dominant way to find misinformation relies heavily on screening sub-communities on Facebook, leading to imperfect and path-dependent monitoring efforts. Third, we document that, as

and (v) the fact that the story has already been fact-checked.

²¹Journalists indeed usually only work on one fact-check at a given moment of time. Note however that we show that our main results are robust even absent these two restrictions.

discussed, due to misaligned incentives, posts that share identical misinformation are not flagged. Improving on these three points appear to be low-hanging fruits, which would require little additional resources and significantly disburden the fact-checkers.

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The Valley of the Clueless? Media Consumption and Populist Persuasion

Abstract

This paper underlines the importance of online media consumption for the rise of a new populist political entrant by exploiting latent differences in media consumption from a historic natural experiment: During the division of Germany, West German TV was a key source of uncensored information in Socialist East Germany (1949-1990) - so important that places without it became known as the “valley of the clueless” (Tal der Ahnungslosen). I show that, to date, these places consume less TV and instead have adopted social media to inform their voting decisions. Yet, these differences only become politically relevant with the entry of the right-wing populist party Alternative for Germany (AfD) in 2013: Comparing close and similar municipalities, places without historic exposure to West TV robustly have a 1.7 – 2% percentage point higher vote shares today for the AfD, which corresponds to 12% of its mean. Using Facebook user data, I show that the AfD’s entry strategy of dominant social media campaigning pays off twofold: First, people are engaging more with the AfD’s populist narratives on social media. Second, differential access to uncensored media in the past impacts present-day media literacy, as recent survey data from the Covid pandemic suggests. This can explain greater persuasiveness of the AfD’s populist narrative.

Keywords: Populism, social media, West German TV, East Germany, persuasion.

JEL No: L82, L86, D72.

Introduction

What is the role of media consumption in the rise of populism? Amidst the rise of right-wing populist parties in many Western democracies in the 2010s, understanding the role of media consumption of voters has become crucial to explain how new populist competitors vie for votes with established parties. Several studies have addressed the short and medium run impact of media consumption on populism. However, few can exploit long-lasting exogenous variation media consumption patters to explain how populist persuade voters through their media diet.

In this paper, I leverage a historic natural experiment in East Germany to address this question. During the time of division (1945-1990), most East Germans had access to West German TV via terrestrial signal. Technical feasibility and the absence of language barriers allowed East Germans to tune in easily who did so extensively for entertainment and uncensored information. However, some 15% of East Germany were cut off from West TV, becoming know as the “Valley of the clueless” in East German popular culture (“*Tal der Ahnunglosen*”). As the name suggests, the exact local TV availability was determined by geographic features like elevation and forests and varied between neighbouring municipalities which made TV reception impossible around a signal strength cut-off. In addition, there were municipalities in various regions without access to West TV, and overall areas with and without access to West TV were very similar to each other. When Germany reunified in 1990 under West German institutions, most of East Germans had thus been exposed to the media of a political system they would then unexpectedly and immediately become part of. Twenty-three years later, since 2013, Germany’s first successful right-wing populist party, the *Alternative für Deutschland* (Alternative for Germany, AfD), celebrates its largest electoral successes in East Germany more generally and even more so in areas without historic access to West TV.

As a first step, I show that differences in media consumption patterns persist until today and continue to shape the type of media that voters consume to inform their voting decisions. Voters in areas without access to West TV pre-1990 are to this day less likely to watch public TV to inform their voting decisions. Instead, these individuals turn more often to social media for information on politics. Differences in TV news consumption exist only for the two public broadcasters that were available during the time of division, but there are no differences for private TV stations that entered the market just before or after reunification, which indicates that these patterns are directly linked to the historic access to West TV. The increased propensity to use social media instead is still noticeable today, for example in terms of the sources that individuals consulted about the Covid pandemic.

Second, I show that the lack of historic exposure to West German TV is associated with

an increase in the vote share for the right-wing populist AfD since its foundation in 2013. In terms of identification and methods, I improve on the previous literature by exploiting fine-grained municipality-level signal data, controlling for state fixed effects and the distance to the former border with West Germany, thus comparing only close municipalities with different historic West TV exposure. The results are highly robust across a broad range of specifications, in particular to using a robust RDD approach around a signal reception cut-off and using two different individual-level surveys. They are also robust to controlling for internet or IT signal technologies that could use similar antennas as West TV and affect voting (e.g. Zhuravskaya et al., 2020; Falck et al., 2014; Gavazza et al., 2019). Using novel data from Cantoni et al. (2019), I further improve on the existing literature by showing that municipalities (rather than districts) were mostly well balanced pre-treatment and that any controlling for pre-treatment differences does not affect the estimation.

An event-study design shows that this effect is specific to the AfD and did not translate into differences in voting for other right-wing populist parties in the pre-internet era. The effect is also much more pronounced after the AfD's populist turn in 2015, when it switched from a fiscally conservative, eurosceptic party to an anti-immigration, anti-establishment and anti-media platform with a strongly pronounced populist rhetoric.

Third, I provide evidence that this populist rhetoric is more persuasive because of the AfD's stronger online presence. During the study period, the AfD is the dominant party on German social media. For example, it generated more shares on Facebook in the 2017 election than *all other parties combined* (Stier et al., 2018). Using data from Müller and Schwarz (2021), I show that the AfD is more present in areas without historic exposure to West German TV. Moreover, this presence is more impactful in these areas after its populist turn, but only for actions that induce a spread of its narratives (posts) and not for mere expressions of approval (likes).

Next, I employ survey data on agreement with conspiratorial statements during the Covid pandemic to show that respondents in areas without historic exposure to West TV are more likely to agree that "the Media manipulate information", but not with other conspiracy theories. While the AfD generally pushes conspiratorial narratives, its (Germany-specific) narrative about the "lying press", a Nazi propaganda term, seems particularly effective in these areas that could not access the uncensored West German TV as an information source under the social East German regime.

Finally, I turn to a rich household survey to further disentangle the mechanism and study other potential differences in attitudes. Despite a large sample size and various variables

covering a long time horizon, I am unable to detect persistent differences in right-wing attitudes, which is in line with the event study finding that the effect is specific to the AfD and the internet and not right-wing extremism in general ¹. Instead, I find evidence that voters are less knowledgeable about politics, which supports the notion that they are less critical in their media consumption. I further leverage my improved identification strategy to revisit earlier findings from the literature. Amongst other, I find that, while Kern and Hainmueller (2009) were right about the positive well-being effects of TV access (see also Chadi and Hoffmann, 2021), it did not translate into increased support for the Socialist East German regime as proxied by satisfaction with life and democracy under the regime in a 1990 survey before reunification.

The findings underline the importance of media consumption patterns in the rise of right-wing populism. First, similar to many challengers to the system (Durante et al., 2019), the AfD leverages social media as a campaigning ground. This strategy is far more effective if faced with an electorate that relies more on social media as an information source to begin with. Moreover, the populist narrative that attacks the established media is thus much more attractive to the electorate. The demonstrated effects are sizeable and, if scaled up to the entire population, play an important role in explaining the rise of right-wing populism in East Germany.

Literature. I contribute to a growing literature that has used this natural experiment to study questions of material aspirations and attitudes (Kern and Hainmueller, 2009; Hyll and Schneider, 2013; Hennighausen, 2015; Bursztyrn and Cantoni, 2016; Crabtree et al., 2015; Friehe et al., 2018, 2020; Laudenbach et al., 2020; Hornuf et al., 2023; Slavtchev and Wyrwich, 2023). Bursztyrn and Cantoni (2016) used detailed signal availability based of the Irregular Terrain Model (ITM) to study the impact of West TV ads on consumption patterns in survey data. I add additional evidence that areas with and without West TV were indeed comparable along a broad range of socio-economic outcomes. Crucially, the municipality-level analysis allows for a fine-grained analysis that makes identification more credible, an aspect so far absent from the literature that has relied mostly on survey outcomes aggregated at the regional or district level (Hennighausen, 2015; Bursztyrn and Cantoni, 2016; Laudenbach et al., 2020; Friehe et al., 2018; Hornuf et al., 2023; Slavtchev and Wyrwich, 2023) or from unrepresentative surveys with small samples (Hesse, 1990; Kern and Hainmueller, 2009; Hyll and Schneider, 2013). The closest to this paper are Friehe et al. (2020), who show that electoral results at the municipal level differed for a short period in the 1990s.

¹Note that this is in contrast to Hornuf et al. (2023), probably due to the less precise treatment assignment and the specific sample and selective questions used. I will return to this below.

I add to this a long-term perspective and a framework to understand the circumstances under which media consumption patterns matter for populism. Related to this literature, I re-evaluate findings by [Kern and Hainmueller \(2009\)](#) on the effect on regime support and by [Hyll and Schneider \(2013\)](#) on material aspirations, both which I am unable to reproduce, with the improved identification strategy, in more reliable survey data from 1990 directly before reunification. Second, I contribute to a literature that uses natural experiments in media exposure to study voting outcomes. [Enikolopov et al. \(2011\)](#); [DellaVigna and Kaplan \(2007\)](#) and [DellaVigna et al. \(2014\)](#) show how media affects election outcomes in the short or medium run. Among those, [Enikolopov et al. \(2011\)](#) and [DellaVigna et al. \(2014\)](#) also employ the ITM to define exposure based on signal data. Other related papers study more long-run norm diffusion through media ([Gentzkow and Shapiro, 2004](#); [Jensen and Oster, 2009](#); [La Ferrara et al., 2012](#)) and the impact of the Socialist East German regime on political attitudes ([Alesina and Fuchs-Schündeln, 2007](#); [Burchardi and Hassan, 2013](#); [Friehe and Mechtel, 2014](#)). Finally, I also touch upon the recently burgeoning literature that studies the role of internet consumption on political outcomes and populism (see [Zhuravskaya et al. \(2020\)](#) for a review). The natural experiment studied here is unique as it created exogenous differences in media consumption that are persistent and politically relevant. As I will discuss, the treatment it had a very high take-up, ran over a long period of time and involved media of a political system that the population would later become part of without knowing it at the time of treatment. Moreover, the strong recent surge in populism in East Germany permits the analysis of the impact of historic exposure to West TV on populism.

The remainder of the paper is structured as follows. Section 2 outlines the natural experiment and its historic and present-day setting. Section 3 describes the data sources used. Section 4 addresses identification and estimation. Section 5 gives the main results on media consumption and populism. Section 6 discusses online populist persuasion and other potential mechanisms before I conclude.

1 Background

Division of Germany and reunification. Following World War II and the occupation by the Allies, Germany was divided and two German states were founded in 1949. The Western Federal Republic of Germany (FRG) adopted a free-market economy, Western democratic constitution with civic and political freedoms, free media and a firm integration into the West. Conversely, the Eastern German Democratic Republic (GDR) was tightly controlled by the Soviet Union with a planned economy, centralised political power by the Socialist

Unity Party (SED) and highly restricted media. Berlin as the former capital was similarly divided, with West Berlin as *de facto* part of the FRG and East Berlin as capital of the GDR. From 1961 onwards, Berlin and Germany were additionally divided by a wall to prevent East Germans from fleeing to the West. In addition, movement within the GDR was very restricted, and it was almost impossible to leave the GDR and difficult to move within it.

Media landscape in East and West Germany. Between 1949 and 1990, East and West Germany exhibited drastically different media landscapes. Television in the Eastern German Democratic Republic (GDR) started in 1951 but was first constrained by a lack of TV sets in the centralised planned economy which persisted until the early 1960s. In the late 1960s, the GDR Politbureau explicitly declared universal ownership of TV sets as a national target, which was achieved by the beginning of the 1970s. Throughout the GDR's existence, there was only one GDR TV channel available, which was under direct control of the Politbureau and followed closely its official propaganda. However, most regions had access to West TV and actively tuned in when they could. Just when TV sets became universally available in the early 1970s, the GDR regime also became less hostile towards the prevalent consumption of West TV by its citizens: The new leader, Erich Honecker, proclaimed in 1971 that “everyone can tune in or out as they please”, ending a short phase of occasional crack-downs. [Kern and Hainmueller \(2009\)](#) even suggest that, by the end of the GDR, the regime saw West TV as a measure to entertain and distract its people. As a result, since 1970s, there were no political, technical or cultural barriers for East Germans to watch West German TV.

Because of its much higher entertainment value and its uncensored information, West German TV played an enormous role for East Germans who received it. Yet, there were two regions comprising 15% of the GDR population that could not receive West German TV because of their geography: First, the region around Dresden situated in valleys around the Elbe river and some parts in the hilly area of East Saxony and second, the area around Greifswald in Mecklenburg-Vorpommern, where forests blocked off the signal. The importance of West TV in everyday life is reflected by condescending references - prevalent to date - to the Elbe valley as *Tal der Ahnungslosen*, which can translate to both “valley of the clueless” or “innocent”.

In the Western Federal Republic of Germany (FRG), television was introduced in 1952 with the establishment of the ARD ² channel as a public broadcaster akin to the BBC in

²“*Arbeitsgemeinschaft der öffentlich-rechtlichen Rundfunkanstalten der Bundesrepublik Deutschland*”, i.e.

the UK. In 1963, a second public channel (ZDF³) was added while the quasi-monopoly of public television in West Germany persisted until the mid 80s. Importantly, both ARD and ZDF were directed at the West German population and generally did not target East Germans directly⁴. They are financed by public fees that are collected independently of the government and have the stated mission to provide information as a public good and hold the executive accountable. Moreover, public broadcasters are tasked with both political education and entertainment. ARD and ZDF had almost identical coverage in terms of content and geography, which is why, following [Bursztyn and Cantoni \(2016\)](#), the analysis below limits itself to the ARD signal.⁵ The two public channels enjoyed a monopoly until the late 80s when private channels entered the market, but those were only available in selected areas very close to Berlin and remained marginal ([Stiehler, 2001](#)). After 1990, cable television reduced the importance of terrestrial TV signal and lowered the entry bar for new TV channels. Recently, the public broadcasting system has become under criticism from the right-wing politicians, with some demanding its abolition, despite the fact that the German Supreme Court considers public broadcasters an integral part of the German constitution.

East Germany 1990-2021. In the wake of the unforeseen fall of the Iron Curtain, the Berlin Wall fell in November 1989 as East Germany lifted the travel ban on its citizens, marking the end of Germany’s division. In October 1990, the GDR was formally incorporated into the FRG under the preexisting West German constitution such that its institutions and the media system were imposed on East Germany with immediate effect. The economic integration, most importantly the privatisation of all East German state companies organised by an independent trust (the *Treuhand*), followed quickly and was finalised by 1994. Despite continuous, large transfers and investments of 2.5% of GDP annually, East Germany has lagged behind West Germany ever since, with East GDP per capita stagnating at 70% net or 90% disposable real income in PPP terms after transfers ([Ragnitz, 2019](#)). Unemployment has been significantly higher at roughly twice the West German level throughout, despite large recent absolute improvements in both parts of Germany. By 2013, the former GDR population has shrunk by one sixth in population due to a very low birth rate and mass emigration of mostly young, educated women to the West ([Ragnitz et al., 2015](#)).

“Working Group of the Public Broadcasters of the Federal Republic of Germany”.

³“*Zweites Deutsches Fernsehen*”, i.e. “Second German Television”

⁴The ARD did feature one news show that analysed East German propaganda from 1958 to 1960, which provoked the GDR television to produce targeted shows to criticize West German TV. One ZDF show, the “Cries for help from across” (1978-1988), did regularly focus on GDR topics, but was still directed mostly at the West German TV audience.

⁵The ZDF signal was sometimes interfered by Soviet military frequencies and regionally by Czech television ([Stiehler, 2001](#)).

East German Politics 1990-2021. In the political arena, East Germany politics has been largely shaped by West German parties since the collapse of the one-party Socialist rule. However, fringe parties like the far-left DIE LINKE or the extreme right NPD fared marginally better in the East, eating into the more volatile vote shares of mainstream parties. This has become more prevalent during Angela Merkel’s term as Chancellor from 2005 to 2021, governing mostly in a mainstream coalition with the centre-left social democrats.

In 2013, the *Alternative für Deutschland* (Alternative for Germany, AfD) was founded as a eurosceptic ultra-conservative party and turned into an anti-immigration far-right platform in 2014 which fares particularly well in East Germany. Around the same time, the far-right PEGIDA movement started out in Dresden (located in the “valley of the innocent”), protesting against a perceived “islamisation of the West” and explicitly criticising the FRG’s political system, including the media which were decried as “lying press”, a term borrowed from Nazi terminology. In 2017, the AfD came third in the federal election, clearing the 5% threshold which it had narrowly missed in 2013, only months after its foundation. In East Germany, it received 22% in 2017, by far overtaking the traditionally second-placed Social Democrats (SPD) and only marginally short of the Christian Democrats (CDU). The AfD is under surveillance by the Federal Office for the Protection of the Constitution for being suspected to threaten the democratic order and constitutional freedoms.

2 Data

2.1 Signal Data

The signal data at the municipality level is taken from [Bursztyn and Cantoni \(2016\)](#) who measure the signal strength on the municipality level in 1992 for East Germany. They impute the signal strength using the Longley-Rice Irregular Terrain model ([Longley and Rice, 1968](#)), an engineering model which calculates the terrestrial signal strength based on the antenna height, strength and the topography of receiving municipalities. I then track municipalities in 1992 to present-day municipal borders across multiple administrative reforms that reduced the total number of municipalities from 7,526 in 1992 to 2,652 in 2017 ⁶. To keep track of the signal strength within a municipality, I calculate both the population-weighted and the area-weighted signal strength at each merge, split or redistricting. The results are identical using either measure. From the same data source, I take the driving distance of municipalities to

⁶These reforms mostly affected the control group areas in Sachsen-Anhalt and Brandenburg, which were reduced to half the number of administrative units and touched the treated areas in Saxony and Mecklenburg-Vorpommern less

West Germany to measure the overall proximity of a place to West Germany, since remoteness presents an important potential confounder that is correlated with access to West German TV.

2.2 Voting and media in East German Municipalities

I collect voting data on the municipality level for the Federal elections in East Germany in 1998, 2002, 2005, 2009, 2013 and 2017 and for European elections in 2009, 2014 and 2019 from the Federal Returning Officer. In addition, I collect State election results from the respective State Returning Officer for the two states - Saxony and Mecklenburg-Vorpommern - in which 95% of the treated municipalities lie.

As controls, I collect all information on municipalities from the Federal Office for Statistics, including a rich set of geographic variables (share of forest, rivers, land use, area size), economic measures (firms, taxes, jobs, work force, commuting, expenditure) and demographic controls (average age, share of women, foreigners, youth). The summary statistics and balance of these variables is reported in section A.1 in the Appendix and discussed further below.

In addition, I rely on three data sources to proxy media consumption on the local level. From Falck et al. (2014), I take the share of household that have access to DSL internet in East Germany in 2005-2009. From Müller and Schwarz (2021) I use the number of AfD Facebook users per municipality and the number of posts on AfD Facebook groups. From the Federal Ministry of Transport and Digitisation, I take mobile signal data in German municipalities in 2013 and 2019. Finally, I rely on Cantoni et al. (2019) who shared their data on pre-treatment voting and socio-economic measures matched to modern-day municipalities.

2.3 Survey Data

The main survey data I use is the German Longitudinal Electoral Survey (GLES), which I accessed through the Secure Data Centre of the GESIS in Cologne. The GLES is an electoral survey that records voting intentions and media consumption. As it is a Germany-wide survey, I pool the cross-sections for the Federal Elections in 2013 and 2017 as well as two state elections on Saxonia (2014) and Mecklenburg-Vorpommern (2016) to reach enough power for the analysis on the East German sub-sample⁷. These cross-sections contain information on voting behaviour, TV news consumption as well as information on which sources people

⁷East Germany accounts for only 16% of Germany's population.

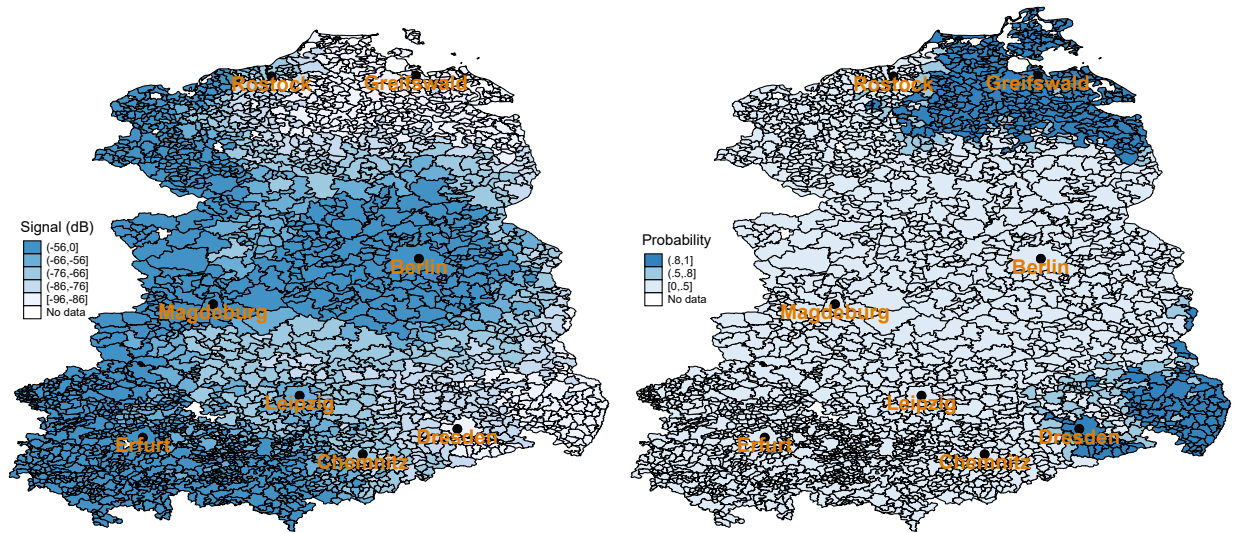
rely on to form their voting decision. Table 1.17 provides the summary statistics. I link the respondents' zip code to their municipalities' treatment status.

To explore media consumption and beliefs in conspiracy theories during the Covid pandemic 2020-2021 as well as a broad range of alternative outcomes, I rely on the the German Socio-Economic Panel (SOEP), which is an annual household panel running since 1984 with a large battery of questions about politics, well-being and attitudes with different frequencies. Since 1990, the SOEP features a large sub-sample of about 6000 East German individuals, which lends it sufficient power for the analysis.

3 Identification

3.1 Treatment definition

East Germany could receive West German TV via terrestrial signal from FRG antennas in West-Berlin, Bavaria (south-west), Hesse (west-south-west) and Lower Saxony (west), with the signal strength declining due to distance and geographic barriers towards the Baltic Sea in the north, Poland in the West and the Czechoslovakia in the South. The left panel of Figure 1.1 plots the raw West German TV signal strength in dB in East Germany from Bursztyn and Cantoni (2016) as described in section 2.1.



(a) TV Signal Strength

(b) Lack of Signal Reception

Notes: The left-hand side Figure plots signal data from [Bursztyn and Cantoni \(2016\)](#) mapped to 2017 municipality borders. The right-hand side Figure plots the probability of not having received West German TV in a municipality. Signal reception declines exponentially around a threshold, which is calibrated for the city of Dresden (signal strength: $db = -86.7$) to have had a 0.8 probability of not having had the signal, since anecdotal and survey evidence shows that Dresden’s population did not have access to West German TV for the most part.

Figure 1.1: Historic Exposure to West TV in East Germany

I define the treatment as the probability that a municipality *had no reception of West German TV before 1990*. I choose to define the *lack* of reception as a treatment, as the norm in East Germany was to have access to it. Public discourse and parts of the literature have hence conceptualised the treatment as *lack of* West German TV, rather than the flip side of *exposure to* West German TV ([Bursztyn and Cantoni, 2016](#); [Stiehler, 2001](#)). I follow this convention here, as I am interested in how the historic lack of access affects media consumption and voting today in a setting where everyone has access to the same media.

The right panel of Figure [figure 1.1](#) maps the treatment assignment of municipalities. To assign the probability to have lacked West German TV reception, I exploit the fact that TV reception deteriorates exponentially around a signal strength threshold. For example, the radio reception when driving around in a car deteriorates discontinuously for small decreases in signal strength. As in [Bursztyn and Cantoni \(2016\)](#), I calibrate this threshold to the city of Dresden, for which anecdotal and survey evidence confirms that West TV was not available most of the time: In survey evidence from the *Zentralarchiv für Empirische Sozialforschung*, about 70% of respondents in Dresden report to never watch West German TV, and about 8% report to have watched it daily (see [Appendix Figure 1.7](#)). Thus, all municipalities with a signal strength of $dB \leq -86.7$ are considered to not have had access to the West German television with a probability of 0.8 or higher. [Figure 1.5](#) plots signal strength against signal

reception for all municipality, with the reception sharply declining in an s-shaped manner around the threshold following the calibration from [Bursztyn and Cantoni \(2016\)](#).

I prefer the reception probability - which is essentially a measure of treatment intensity - over a binary indicator, since the (few) municipalities with a reception probability between 0.2 and 0.8 probably could watch West German TV depending on the exact local weather and geography conditions of a household⁸. As Appendix Figure 1.6 shows, the resulting treatment assignment is still very binary, the results are robust to using a binary indicator. When presenting balance checks, I classify East German municipalities as treated (i.e. lacking West TV) if their reception probability was 0.5 or lower. Moreover, I will further show that the results are robust to using a robust RDD design around the signal threshold of Dresden. In total, about 15% of the GDR population lived in treated municipalities around Dresden in the south-east of Saxony and Greifswald in the north-east of Mecklenburg-Vorpommern.

3.2 Treatment compliance

All survey and anecdotal evidence suggests that West TV was watched enthusiastically wherever possible ([Hesse, 1990](#); [Kern and Hainmueller, 2009](#); [Stiehler, 2001](#)). First, there were no cultural or linguistic barriers between East and West Germans. Second, as discussed, the East German regime publicly encouraged its citizens to watch whatever they preferred to increase their satisfaction with life and reduce their propensity to protest ([Kern and Hainmueller, 2009](#)). Third, ownership of TV sets did not differ between treated and non-treated regions⁹. In the reality of a planned economy, individuals would buy what was centrally made available, and the GDR leadership had declared TV sets a strategic priority, initially hoping to spread propaganda more effectively ([Stiehler, 2001](#)). Finally, and most importantly, West TV was considered much more interesting and entertaining than the propaganda-guided GDR TV, because of the high journalistic quality and the more liberal entertainment programme that also features American movies and series. In surveys, still conducted under the GDR regime, virtually all respondents that could receive West German TV admitted to watching it (see Appendix Figure 1.7), with a sizeable share of young respondents reporting watching it more than four hours per day ([Stiehler, 2001](#)). A survey conducted by [Hesse \(1990\)](#) amongst East German emigrants suggests that East Germans who could watch West German TV almost never watched East German TV instead.

⁸For example, for the city of Dresden, it is reported that signal reception was impossible in the city centre, but sometimes possible under ideal weather condition for households located on the hills north of the city ([Stiehler, 2001](#))

⁹In fact, TV set ownership did not differ between regions and was, if anything, slightly higher in the treated regions ([Bursztyn and Cantoni, 2016](#)).

If anything, West TV played such an important part that there might have been a considerable share of always-takers that bias the estimates downwards: [Stiehler \(2001\)](#) documents that some people started building higher antennas in the 1980s to receive West TV. Moreover, some went to relatives or chose vacation locations specifically with the goal to enjoy West TV, and respondents in qualitative and quantitative surveys did report to have access to (much less popular) West German radio stations like the West German *Deutschlandfunk* and occasionally used it to circumvent GDR censorship for unbiased information ([Stiehler, 2001](#)). Still, while key information about important events appears to have circulated in treated areas as well, it only did so as rumours rather than through direct consumption of West German TV. As a result, there seems to have been a general lack of background information about the FRG political system and the appreciation of West German TV as daily source of reliable information and valued entertainment ([Stiehler, 2001](#)).

3.3 Pre-treatment balance

There are, by now, several papers that have argued that treatment and control areas have been well balanced before treatment. This has mostly been done using district-level data from the GDR in the 1960s ([Bursztyn and Cantoni, 2016](#); [Kern and Hainmueller, 2009](#); [Hyll and Schneider, 2013](#); [Friehe et al., 2020](#); [Crabtree et al., 2015](#)), which are reported in the appendix. In terms of political outcomes that we are interested here, [Friehe et al. \(2020\)](#) show that in the last partly free elections in 1932, treated and untreated areas voted similarly at the district level, and [Kern and Hainmueller \(2009\)](#) further report that there was no significant difference in the partially free elections in 1946.

Since the identification strategy uses variation at the much finer municipality level, it is useful to revisit the balance of pre-treatment outcomes. [Table 1.1](#) reports the balance of votes for the NSDAP, the party of Hitler whose vocabulary and resentments resonates with the AfD today, where No West TV is a binary indicator if the probability to have received it is lower than 0.5. The vote shares are mapped to 2017 municipality borders thanks to data from [Cantoni et al. \(2019\)](#). While we can detect some differences in historic vote shares of the NSDAP in earlier elections, the direction of the persistence in today's AfD vote would bias estimates downwards. Moreover, according to persistence estimates of [Cantoni et al. \(2019\)](#), even a one standard deviation (9.87%) increase in the 1933 NSDAP vote only translates into an increase by 0.06 standard deviations in the 2017 AfD vote, which would be $6.16 * 0.06 = 0.36$ percentage points. Since the observed differences are smaller, ranging from 0.5 of a standard deviation in 1928 to an insignificant 0.27 in 1993, the pre-treatment differences are unlikely to matter economically for the estimation. Additional data from [Voigtländer](#)

and Voth (2012) shows no differences with two proxies for the ideological alignment with the NSDAP. There were no significant differences in the number of letters written to the Nazi propagandist journal “*Der Stürmer*”¹⁰ or the number of Jews deported during the Nazi regime; if anything the point estimates point towards a lower historic prevalence of Nazi ideology in the areas without historic access to West TV. Together with differences in socio-economic characteristics reported in Appendix Table 1.5, these pre-treatment differences can further be controlled for to gauge their impact on the estimation.

Table 1.1: Balance Checks on Pre-treatment Political Outcomes

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
% NSDAP 05/1928	397	0.981 (0.052)	2050	2.241 (0.153)	2442	-1.260***
% NSDAP 09/1930	397	16.723 (0.916)	2060	18.000 (0.933)	2452	-1.277**
% NSDAP 03/1933	537	45.548 (1.152)	2060	42.862 (2.037)	2591	2.687
turnout 05/1928	397	81.884 (1.744)	2050	78.079 (1.195)	2442	3.805
turnout 09/1930	397	86.381 (1.244)	2060	83.119 (2.054)	2452	3.262
turnout 03/1933	537	90.893 (0.989)	2060	89.000 (1.370)	2591	1.893
Deported Jews	550	2.070 (1.323)	2101	4.121 (1.764)	2645	-2.051
Stürmer letters	550	1.048 (0.718)	2101	1.916 (1.022)	2645	-0.868

Notes: Data from Cantoni et al. (2019) and Voigtländer and Voth (2012) matched to present-day municipalities. The NSDAP represents Hitler’s party, with the month of the election indicated. The number of Jews deported is reported in logs as in Voigtländer and Voth (2012). Robust standard errors controlling for distance to West Germany regressed on a binary indicator if the probability to lack West German television is larger than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

¹⁰“The Stormer”

Furthermore, as Figure 1.1 shows, the treatment status varies locally and is driven by distance and geographic factors, e.g. small mountains blocking off the signal in Pomerania and valleys in the south east. Appendix Table 1.7 shows that while treated municipalities tend to be more distant from West Germany, exhibit more wasteland and water and are somewhat more agricultural, they are overall comparable. In addition, there is no reason to believe that West Germany targeted specific regions in East Germany with its antennas, as they made no effort to reach the populous city of Dresden, which was only a few decibel short of receiving the signal. This also aligns with the fact that West German TV was directed first and foremost at West Germans and its availability in the GDR was merely a by-product of that. Finally, it is reassuring that all studies have shown that at the time of reunification, treatment and control municipalities were still well-balanced (Hyll and Schneider, 2013; Kern and Hainmueller, 2009). Further below, we add to these findings by presenting micro evidence that treatment and control municipalities were also very well balanced even in terms of values and attitudes towards life (see Appendix Table 1.25).

3.4 Comparability today

The effect of West TV availability on election outcomes of municipalities does not require these municipalities to have balanced characteristics today, as controlling for socio-economic characteristics might be conditioning on bad controls that are themselves an outcome of watching West TV. For example, West TV might have changed the way people work or their propensity to emigrate, which in turn affects populism in a village and should thus not be controlled for. Previous studies have demonstrated that no selective migration occurred and the demographic structure of municipalities remained similar. This has been done by Bursztyrn and Cantoni (2016) and Friehe et al. (2020) for the 1990s with a focus on migration patterns (see appendix). I add to this evidence by showing in Table 1.6 that, to date, municipalities in treatment and control areas are well balanced across a broad range of characteristics, including the share of asylum seekers, migration patterns and the demographic structure. In addition, in Table 1.10 in the appendix, I use data by Falck et al. (2014) for the 2000s to confirm that migration patterns and other characteristics (including IT infrastructure) were similar in treated and non-treated municipalities during the 2000s. As with any observed differences pre-treatment, we can further include these post-treatment outcomes to assess their importance in the estimation.

3.5 Estimation

The identifying assumption of the estimation strategy is that, absent any differences in access to West German TV, areas with exposure to it are a good counterfactual for what areas without exposure would have been today. This assumes that a treated (unexposed) and an untreated (exposed) municipality in the same region with the same distance to West Germany would have differed only in their historic exposure to West German TV. As the previous balance checks show, this seems a reasonable assumption. To increase the credibility of the identification strategy, I control, in all specifications, for the driving distance to West Germany, which proxies for the remoteness of municipalities. I include state fixed effects as the relevant political unit, since German parties are organised on the state level. This specification is a very demanding, as remoteness is also driving the signal reception, and thus I compare only similarly remote municipalities in the same state with varying treatment status. Standard errors control for spatial correlation with a 50 km cut-off (Conley, 1999), although the results are robust to clustering at the 51 East German districts as well.

This main specification is applied to both the survey and municipality data to keep estimates comparable. Formally:

$$y_{ise} = \alpha_e + \delta_s + \eta * distance_{is} + \beta * NoTV_{is} + \mathbf{X}_{ise} * \boldsymbol{\gamma} + \epsilon_{ise} \quad (1.1)$$

$NoTV_{is}$ is the probability that a municipality i in state s at election e did not receive West German TV, ranging from $[0; 1]$, with β being the coefficient of interest. As outcome y_{ise} , I study media consumption and political attitudes as well as voting outcomes. When pooling elections, α_e controls for election fixed effects. As mentioned, all specifications controls for δ_s , a state fixed effect and $distance_{is}$ as the driving distance to West Germany to proxy remoteness. To address potential remaining identification concerns, I flexibly control for a varying set of municipality-level covariates \mathbf{X}_{ise} depending on the data source: In the survey data, it controls for individual characteristics like employment, education, gender and age and religion. In the municipality panel, it controls for geographic controls since geography is driving identification, historic controls from Cantoni et al. (2019) for potential pre-treatment differences, IT controls for 3G signal and other infrastructure that might be correlated with historic TV signal (Guriev et al., 2021) as well as any post-treatment socioeconomic differences.

4 Results

The presentation of the results proceed in three steps. First, I show that different media consumption patterns in municipalities without historic exposure to West German TV persist until today. Second, I show that this translates into an increased vote share for the AfD in the 2017 Federal election. Third, I provide evidence that this is channelled through the online presence of the AfD in those areas.

4.1 Media consumption

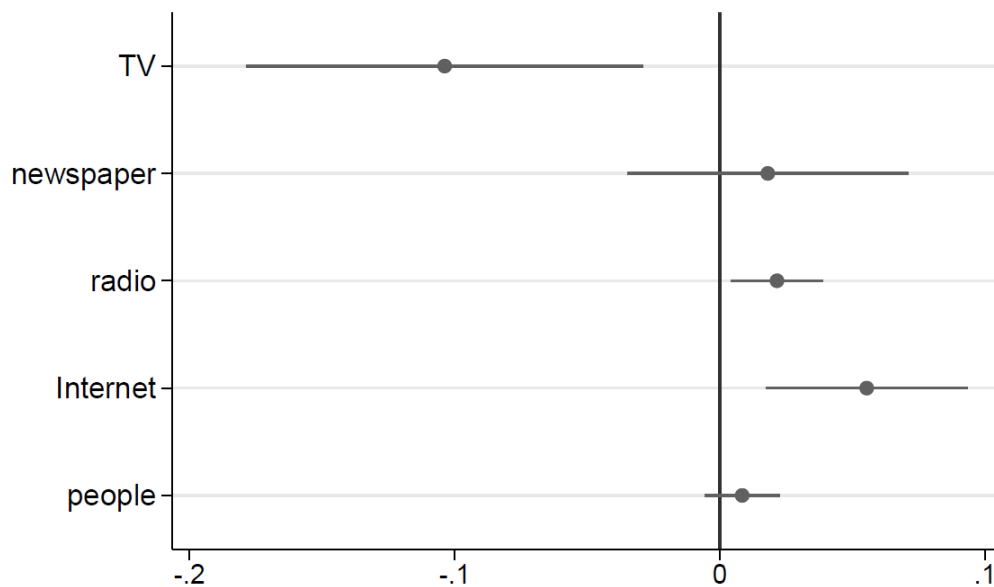
Figure 1.2 shows persistent differences in information sources of respondents in municipalities without historic exposure to West German TV. Each line represents a regression on a dummy if the source was mentioned by respondents in the pooled cross-sections of the GLES survey summarised in Table 1.17. Overall, respondents in treated areas are to date 10.1 % less likely to rely on information from television when they form their voting decision. This seems partly substituted by the Internet (5 % more likely) and the radio (1.6 % more likely). Furthermore, survey evidence from the SOEP Covid waves shows that East German survey respondents are 8 % more likely to rely on social media as information source on the Covid pandemic if they live in areas without historic exposure to West German Television (see Appendix tables 1.23 and 1.20). This finding in two independent data sources supports the hypothesis that respondents in treated areas persistently substitute TV for social media in their new diet.

In terms of affected TV stations, Figure 1.19 in the Appendix shows that only the public TV stations that were part of the treatment are affected. When asked about which TV news they consume, respondents in treated municipalities mention less often ARD (-24 %) and ZDF (-41 %), the two public channels that were available to other East German municipalities during the division of Germany. There might also be a marginal effect for RTL, the first private channel established in 1984. However, there are no difference with respect to TV channels that entered the market just before or around reunification. This supports the assumption that present differences in media consumption are indeed linked to the differential exposure to West German television before reunification.

Importantly, Appendix Table 1.9 shows that access to mobile signal is balanced in treated and control areas and is thus unlikely to drive differences in media consumption today. This speaks to the fact that West German TV signal was broadcast via antennas across the border in West Germany, whereas overall TV access relied on East German antennas and infrastructure that served later in the spread of internet and mobile signal. In terms of

internet roll-out, data from Falck et al. (2014) shows that if anything, the internet adoption rate was somewhat lower in 2005 and 2006, which is no longer significant by 2008 and in any case would bias the estimates on social media usage downwards.

Overall, this paints a coherent picture of substitution of public television in favour of social media as a source of information in areas without historic exposure to West German television. Voters in areas without historic exposure to West TV appear to have embraced social media more than others as an information source when it emerged. This happened at the expense of public TV channels that constituted West German TV during the treatment period.



Notes: Data from the pooled GLES survey for the 2013 and 2017 federal election and the 2014 and 2016 State election in Saxony and Pomerania (N=3787). Each line estimates equation (1.1) on a dummy if the source was mentioned, controlling for individual characteristics. All variables are described in Appendix Table 1.17. The plotted confidence intervals are constructed with Conley standard errors at the 95 % confidence interval with a 50 km cut-off.

Figure 1.2: Main source to inform the respondent's voting decision

These differences in media consumption appear to be reflected in how voters are in contact with parties before elections. Figure 1.18 suggests that voters without historic access to West TV are somewhat more likely to be contacted by parties via social media and flyers, suggesting that parties shift from traditionally dominant TV advertising to other media. Moreover, Figure 1.17 indicates that voters are somewhat more often contacted by the AfD. However, these differences are not significant at conventional levels.

4.2 AfD Vote (2013-2019)

Table 1.2 reports the results for estimating equation (1.1) with a varying set of controls for the pooled election panel of East German municipalities between 2013 and 2019 for all elections in which the right-wing populist AfD party. Throughout, standard errors allow for spatial correlation with a cut-off of 50 kilometre (Conley, 1999).

Column (1) reports the raw correlation between former exposure to West German television with the AfD vote without any controls. It alone can explain about 2.3% of the AfD's performance in elections between 2013 and 2019, but also associates more remote areas with the AfD vote that might have voted more for it absent any differences in West TV exposure.

Column (2) reports the preferred specification that controls for the distance to West Germany of a municipality as well as state and election fixed effects, which correspond to the level at which parties form their electoral lists. As discussed, this specification only compares municipalities in the same state (each about the size of the state of New Jersey) with a identical remoteness from West Germany that differ in their historic exposure to West TV, hence relying on local differences in West TV exposure due to topography.

Column (3) incorporates all pre-treatment differences observed on the municipality levels as discussed in Table 1.1 and Appendix Table 1.5 to address potentially persistent differences in pre-treatment imbalances. As expected, these economically small difference do not affect the estimation significantly and represent, if anything, an upwards corrections.

Column (4) further adds the geographic controls reported in Table 1.7, thus controlling, amongst others for the land use of a municipality, the elevation levels and presence of water or woods in a municipality as well as its longitude and latitude. Reassuringly, while geography matters for the treatment assignment as the signal decays both with distance and because of geographic barriers like hills and forests, these differences do not significantly affect the estimation either.

Similarly, column (5) controls for post-treatment access to IT technologies. As shown by Guriev et al. (2021), access to internet through mobile signal matters for government accountability and potentially for voting. However, as argued in Table 1.9, access to IT technologies was balanced across municipalities post treatment, since it was only the location of West German TV antennas that mattered for access, while East German antennas - that matter ultimately for present-day access to mobile signal - covered all of East Germany equally well. Column (5) also controls for the roll-out of DSL internet in the late 2000s as studied by Falck et al. (2014), which is orthogonal to the treatment effect.

Finally, column (6) conditions on any post-treatment differences in socio-economic characteristics of municipalities as discussed in tables 1.6 and 1.8. As it also controls for levels and changes in unemployment and demographics, it addresses potential alternative drivers of right-wing populism, such as the feeling of being left out by economic development. While these might themselves be affected by the treatment, even this rich set of controls does not affect the estimation much. This also suggests that the variety of short-term socio-economic effects that the literature has studied do not matters much in this context (e.g. Hyll and Schneider, 2013; Bursztyn and Cantoni, 2016).

The magnitude of the effect is sizeable. In the preferred specification of column (2), going from a probability of not having received West German TV municipalities of 0 to 1 leads to a 1.88 percentage point higher vote share for the AfD from 2013 to 2019, corresponding to 12% of the mean or 18% of a standard deviation. This is twice the magnitude of moving from a densely populated area to a sparsely populated area.¹¹ In comparison, a one-standard deviation increase in the distance to West Germany (about a 45 minute drive) is associated with an increase in the AfD vote share of 0.25%.

¹¹Densely populated corresponds to more than 1000 inhabitants per square kilometre, sparsely populated to less than 60 inhabitants per square kilometre. The classification is taken from Falck et al. (2014)

Table 1.2: AfD Vote shares (2013-2019) and West TV

	Vote Share of the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
No West TV	3.991** (1.724)	1.884** (0.839)	1.799** (0.783)	1.800** (0.777)	1.691** (0.733)	1.631** (0.717)
Distance to West	No	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes	Yes
Election FE	No	Yes	Yes	Yes	Yes	Yes
Hist. controls	No	No	Yes	Yes	Yes	Yes
Geog. controls	No	No	No	Yes	Yes	Yes
Mobile signal	No	No	No	No	Yes	Yes
Socio-economics	No	No	No	No	No	Yes
Observations	12015	12015	12015	12015	12015	12015
R-squared	0.0233	0.756	0.764	0.765	0.767	0.773
Dep. mean	15.8	15.8	15.8	15.8	15.8	15.8
Dep. SD	(10.0)	(10.0)	(10.0)	(10.0)	(10.0)	(10.0)

Notes: P-value significance: * 0.10 ** 0.05 *** 0.01. Conley standard errors with 50 km cut-off in parenthesis. The data are pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). No West TV is the probability that a municipality could not access West German Television, see section 3.1. The distance to West Germany is measured in hours (see Appendix Table 1.7). State fixed effects control for the state-specific party lists in elections. Historic controls consist of party vote shares and turnout in the last free elections (Appendix Table 1.1) and socio-economic characteristics (Appendix Table 1.5) in municipalities pre treatment (1928-1933). Geographic controls include the area, land use and geology of municipalities (see Appendix Table 1.7). Mobile signal controls for LTE and 3G signal access as well as early-stage DSL access (Appendix Table 1.9). Socio-economic controls include demographic, and economics variables, including the number of refugee and the structure of the economy (see Appendix Table 1.6, 1.8).

This finding is highly robust to different specifications. First of all, it is robust to employing a binary measure of No West TV access instead of the probability (Appendix Table 1.11) and to dropping municipalities with an ambiguous treatment assignment, e.g. a probability of lacking West TV between 0.2 and 0.8 (Appendix Table 1.12).

Moreover, Appendix section A.2 exploits the fact that - in contrast to the survey data - the municipality panel is sufficiently dense around the signal threshold to employ a more data-intensive robust regression discontinuity design (RDD), where the window around the cut-off is chosen optimally (Calonico et al., 2022). Appendix Table 1.14 reports this estimates, with

standard errors clustered amongst the nearest five neighbours. The associated graph is reported in Appendix graph 1.14. Appendix Table 1.15 further suggests that the effect is local to the Dresden signal cut-off as described in section 3.1, suggesting that the threshold is chosen adequately. Appendix Table 1.16 further shows that the finding is robust to taking logs, thus limiting the influence of potential outliers. In this specification, crossing the signal threshold is associated with a 8 – 14% increase in the AfD vote share, in line with the main result as discussed above.

Finally, the same qualitative finding extends to the GLES survey data (Appendix Table 1.18) and the SOEP survey data (Appendix Table 1.21). In these individual-level specifications, it also holds in a logit specification. While the power in the survey results is overall too low to apply the robust RDD or perform a detailed heterogeneity analysis, Appendix Table 1.22 shows for a subset of respondents with detailed information on the place of birth that the effect comes from individuals born before 1980 in places that did not have access to West TV.

4.3 Far-right vote since 1998

There have been several surges other right-wing populists in East Germany, most notably of the Republicans and the NPD parties in the late 1990s or the DVU party in the mid-2000s, which made it into regional parliaments in East Germany by clearing the 5% threshold on several occasion. However, as shown in the parallel trend in Appendix Figure 1.13, areas without historic exposure to West TV followed very closely the voting pattern of the control group for these right-wing populist parties. This only changed with the arrival of the AfD that differences emerge, which are most pronounced after 2014. To estimate the treatment effect over time, I employ a modified version of equation (1.1):

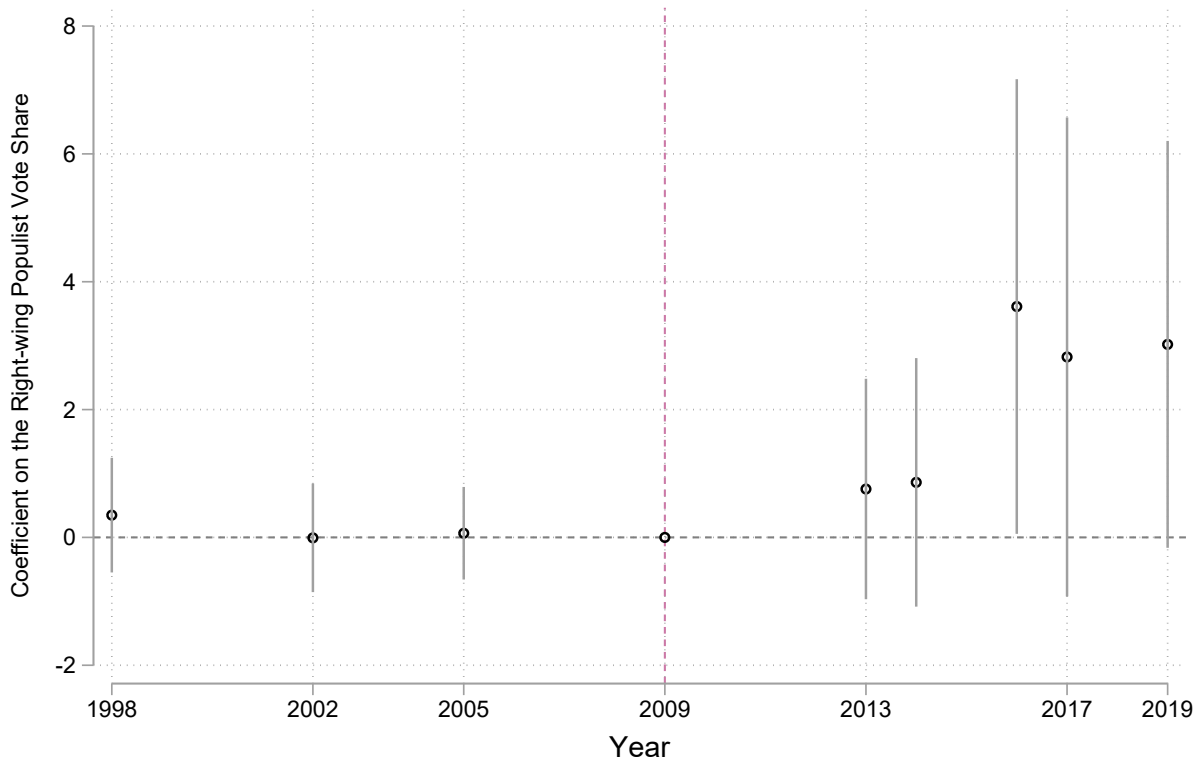
$$y_{isey} = \alpha_e + \delta_s + \eta * distance_{is} + \sum_{y=1998}^{2019} \beta_y * NoTV_{is} + \epsilon_{ise} \quad (1.2)$$

Where β_y is the year fixed effect with 2009, the last election before the AfD was founded, as omitted category. In years with two elections, the election fixed effect controls for election specific effects. Figure 1.3 plots the year fixed effect coefficients for the accumulated vote shares for right-wing populist parties in East Germany over time, otherwise using the preferred specification of column 2 from Table 1.2 and standard errors conservatively clustered at the regional level ¹². The baseline year is 2009, which is the last year with an election

¹²For earlier elections, there is no detailed information on the longitude and latitude of municipalities.

before the foundation of the AfD in February 2013. The flat pre-trends and the change following the arrival of the AfD suggests that historic exposure to West TV matters for the rise of the AfD, but not for right-wing populism in general.

While the disaggregated estimates are noisy, there is a clear break following AfD’s Essen party congress in July 2015, which marked the AfD party’s anti-elite and anti-immigration shift¹³. This also coincides with a stark increase in the online presence of the AfD: For East Germany, data by Müller and Schwarz (2021) suggests that the number of AfD Facebook users per 100,000 inhabitants increased from 1.5 before 2015 to 4.5 after (see section 5.1).



Notes: The Figure plots the interaction of year fixed effects with No West TV estimated in Table 1.13. 95 % confidence intervals with standard errors clustered at the region level. The data are pooled election data for Federal elections (1998, 2002, 2005, 2009, 2013, 2017), EU elections (2009, 2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). No West TV is the probability that a municipality could not access West German Television, see section 3.1. The estimated specification is the preferred one from column (2) of Table 1.2 and controls for the distance to West Germany is measured in hours as well as state and election fixed effects to control for the state-specific party lists in elections. Appendix Figure 1.14 shows the parallel pre-trends.

Figure 1.3: Evolution of the Aggregated Right-Wing Populist Vote Share over time

There are 51 regions in East Germany, which makes the clustering rather conservative.

¹³Column (2) in Table 1.4 shows that this shift is significant at the 10% level

4.4 Turnout and other parties

Table 1.13 reports the estimation of equation (1.2) as well as identical estimates for turnout, far left parties, mainstream parties, left parties and right parties which are grouped together to account for changing party dynamics over time. Following the early 2000s, there are no significant differences in the vote share of extreme left parties, underlining that the result is not about political extremism in itself and different from the effect that Friehe et al. (2020) find. Figure 1.14 further shows that following 2015, other far-right parties become very marginal and account for less than 2% of all votes.

Column 3 of Table 1.13 further shows that the increase in votes for the AfD is driven by a decrease in the vote shares for centre-right parties, who are also the slowest in adopting an effective social media campaigning strategy, especially given their overall size (Serrano et al., 2019). There is also a positive impact on the vote share of left parties (SPD and Greens) after they invested heavily in their social media campaign during the 2017 Federal election (Stier et al., 2018). Yet, in comparison to the mean and standard deviation, the impact of the AfD is by far the largest. For example, in the 2019 European election, the AfD's increase of 3 percentage points over the far-right baseline in 2009 corresponds to one third of the mean, whereas the positive effect of 2% for left wing parties corresponds to merely ten percent of the mean.

5 Populist persuasion online

The preceding section suggests that the differences in the AfD vote share are not specific to its far-right policies or political extremism, but rather a function of party-specific characteristics that interact with latent differences in media consumption of the electorate. This section presents evidence for two possible mechanism - online persuasion and media literacy, before discussing alternative explanations.

5.1 AfD online presence

The AfD has established a dominant presence on social media in the political German online landscape. By one estimate for the 2017 Federal election, the AfD received more shares on Facebook for its posts than all other parties *combined* (Stier et al., 2018). It also posted more than any other party and generated by far the most likes during the 2017 Federal election (Serrano et al., 2019).

To test if the AfD shows a differential online presence in areas without historic exposure to

West TV, we can rely on data from Müller and Schwarz (2021). They geolocate users of the AfD Facebook page by municipality through the interaction of users with it. Users can post, comment or like the page. Of 93,806 users, they identify 34,396 users, out of which 11,906 are in East Germany ¹⁴. The provided variables are already winsorised at the 99.9th percentile to account for the extreme tails common to social media data. For the overall number of users, there is an additional estimate on the number of AfD Facebook page users before 2015.

There are several caveats to this data. First, while there exists a pre and post number for AfD Facebook users around 2015, there is no temporal breakdown of posts and likes by year. Second, the interaction with the AfD Facebook page could itself be a function of the local AfD vote share. Third, the process of geolocating users might be unbalanced across treatment and control as it relies on users sharing their location.

I first study if the AfD exhibits a stronger online presence in municipalities without historic exposure to West German television. Table 1.3 reports balance across the variables provided by Müller and Schwarz (2021) in raw numbers, and Figure 1.16 plots the distribution of AfD Facebook users in treated and untreated areas normalised by population in terms of the median municipality. Already before 2015, more users interacted with the AfD in treated versus untreated areas. This difference further intensifies after the AfD's populist turn in 2015. Moreover, treated municipalities are overrepresented in the top of the distribution and 10.6% more likely to have any user interacting with the AfD Facebook page, although the difference is not statistically significant. In the median municipality, the AfD has about 1,2 more users in 2017. Interestingly, conditional on having one AfD Facebook user, the number of posts, likes or comments per user is similar across treatment and control municipalities.

¹⁴Unsurprisingly, East Germany is thus strongly overrepresented relative to its population share of about 17%.

Table 1.3: Historic TV access and User Interactions with the AfD Facebook page

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
# users (post 2017)	550	4.647 (0.849)	2102	4.448 (0.381)	2646	0.199*
# users (pre 2015)	550	1.575 (0.294)	2102	1.495 (0.130)	2646	0.079***
=1 if top 1 percent	550	0.013 (0.005)	2102	0.008 (0.002)	2646	0.005***
=1 if any user	550	0.933 (0.011)	2102	0.827 (0.008)	2646	0.106
likes / user	513	0.504 (0.079)	1738	0.800 (0.105)	2246	-0.296
posts / user	513	0.226 (0.025)	1738	0.248 (0.028)	2246	-0.022
comments / user	513	0.375 (0.057)	1738	0.477 (0.047)	2246	-0.102

Notes: P-value significance: * 0.10 ** 0.05 *** 0.01. East German municipalities in 2017 merged with data from Müller and Schwarz (2021), who geolocate Facebook users that interact with the AfD Facebook page through likes, posts or comments before 2015 and in 2017. The provided data is winsorised at the 99.9th percentile. Each t-test controls for distance to West Germany, state fixed effects and population. No West TV is a binary indicator if the probability to have received it is lower than 0.5. Robust standard errors in parenthesis.

These differences in the online presence of the AfD matter for its vote share in particular following its populist turn in 2015. Table 1.4 consecutively interacts the AfD’s online presence in a municipality with the treatment and an indicator for elections post 2015. Column (1) is identical with column (2) of Table 1.2 but controls for potential differences in internet access through mobile signal or DSL as well as the population size of a municipality. Column (2) shows that the AfD is performing significantly better in areas without historic exposure to West German TV after its populist turn in 2015 and its ensuing electoral success.

In line with the argument above, columns (3) and (4) show that the electoral payoffs to the AfD’s Facebook presence are different in areas that had not been exposed to West German

TV. Column (3) suggests that one additional AfD Facebook user - which corresponds to the difference between treatment and control - is associated with an additional 0.03 percentage point increase in the AfD vote share in treated areas versus untreated areas in for the post-2015 period.

Column (4) distinguishes between the association of the AfD's vote share and the type of interaction by users with its Facebook page ¹⁵. While no breakdown before and after 2015 is available, it allows to differentiate between the number of posts and the number of likes per user, which are fundamentally different on Facebook: Likes are a costless expression that is unambiguously positive for the liked page. Likes are also an unambiguously positive and strongly predictive of the AfD, but not differentially for treatment and control municipalities. This suggests that the differences in the AfD vote is unlikely to reflect profound differences in political preferences, as will be further discussed in section 5.3.

On the other hand, posts, appear on the users' timeline and potentially on the timeline of their friends. Assuming that friends are more likely to be local, posts are a better measure of the importance of exposure to the AfD's narrative online. Strikingly, the association between number of posts per user and the AfD vote share is much stronger in areas without historic exposure to West TV. One additional post per user is associated with an increase in the AfD's vote share by 1.7 percentage points, over the control group. This is very large compared to the initial estimate of 1.76 percentage points reported in column (2) of Table 1.4.

Lastly, there is further evidence from the survey data to believe that the AfD's messaging has a stronger impact in areas without historic exposure to West TV. Figure 1.20 shows that, in the GLES survey data, respondents in treated areas that rely on social media to inform their voting decision are 11% more likely to vote for the AfD compared to the baseling of respondents that rely on television.

¹⁵By definition, this is conditional on having at least one AfD Facebook user, thus the sample size differs from columns (1)-(3)

Table 1.4: Interaction of Historic TV access and media usage

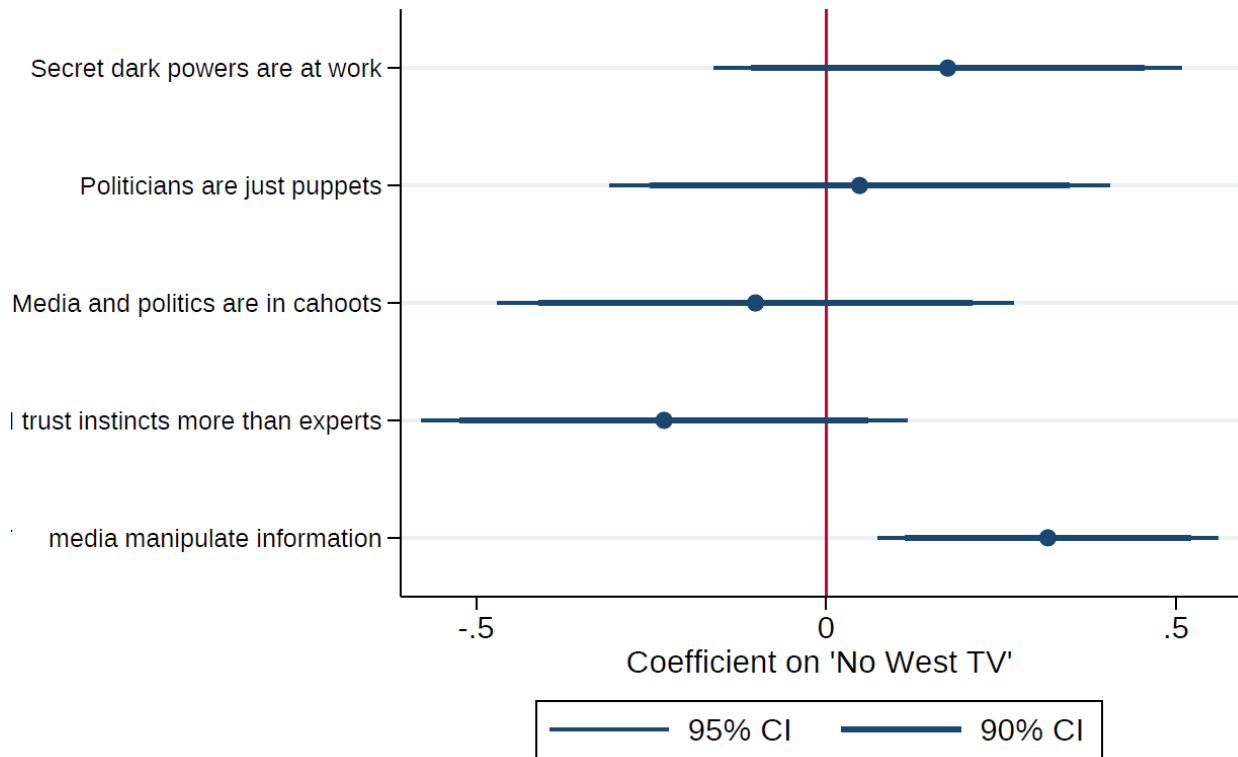
	Vote Share of the AfD Party							
	(1)		(2)		(3)		(4)	
No West TV	1.763***	(0.655)	0.433	(0.905)	0.478	(0.914)	1.407**	(0.680)
Post 2015			20.764***	(0.667)	20.888***	(0.670)		
No West TV × Post 2015			2.422*	(1.430)	2.338	(1.450)		
AFD FB users					0.007	(0.016)	-0.022***	(0.004)
No West TV × AFD FB users					-0.024*	(0.013)	0.022	(0.021)
AFD FB users × Post 2015					-0.034*	(0.019)		
No West TV × AFD FB users × Post 2015					0.033*	(0.017)		
posts / user							-0.318**	(0.142)
likes / user							0.075**	(0.036)
No West TV × posts / user							1.696***	(0.473)
No West TV × likes / user							-0.300	(0.208)
State FE	Yes		Yes		Yes		Yes	
Election FE	Yes		Yes		Yes		Yes	
Distance to West	Yes		Yes		Yes		Yes	
Internet access	Yes		Yes		Yes		Yes	
Observations	12,015		12,015		12,015		10,289	
R-squared	0.758		0.841		0.842		0.767	
Dep. mean	15.9		15.9		15.9		15.8	
Dep. SD	(9.97)		(9.97)		(9.97)		(9.83)	

Notes: P-value significance: * 0.10 ** 0.05 *** 0.01. Standard errors are conservatively clustered at the region level. The data are pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014) merged with data from Müller and Schwarz (2021) (see Table 1.3). No West TV is the probability that a municipality could not access West German Television, see section 3.1. The distance to West Germany is measured in hours. State fixed effects control for the state-specific party lists in elections. Internet access controls for LTE and 3G signal access as well as early-stage DSL access (table 1.9).

5.2 Narrative of “The lying press”

The AfD is pushing several narratives that are common to many right-wing populist movements. One important narrative describes the mainstream media as “lying press” or “system media”, both directly borrowed from Nazi terminology. Following its resurrection as a political term by the far-right, the corresponding German word, *Lügenpresse*, has even been voted *faux-pas* word of the year by German linguists in 2015. Moreover, the AfD is the only major political party that demands the complete abolition of all public broadcasters and frequently accuses public broadcasters of being biased in favour of a left-wing political agenda. While the AfD’s agenda features a much broader range of anti-establishment positions, areas without historic exposure to West German TV might be more susceptible to the narrative of the “lying press”: In contrast with other East Germans, they could not compare the highly censored East German TV with the uncensored West TV and build the media literacy necessary to tell apart manipulated media from unbiased reporting.

To test this hypothesis, we turn to the SOEP Covid waves that asked respondents about various aspects related to the Covid pandemic during 2020 and 2021. Table 1.20 summarizes the variables and controls used. The 2020 wave features a battery of questions on agreement with statements that proxy support for different types of conspiracy theories. Specifically, they measure the agreement on a 1-5 scale with the following conspiratorial statements: “secret dark powers are at work”, “politicians are just puppets”, “The Media and politics are in cahoots”, “I trust instincts more than experts”, and “The Media manipulate information”. While the AfD is pushing several of these narratives, in particular on corrupt experts, politicians and media, the statements differ in the way the media is affected or portrayed. In particular, one conspiratorial statement addresses the closeness between media and politics, while the last one addresses the manipulation of information specifically.



Notes: Data from the pooled GLES survey for the 2013 and 2017 federal election and the 2014 and 2016 State election in Saxony and Pomerania (N=3787) as described in Appendix Table 1.17. Each line regresses lack of historic TV, defined as the probability of not having had access to West TV on a dummy if the source was mentioned, controlling for the variables described in Appendix Table 1.17. The plotted confidence intervals are constructed with Conley standard errors at the 95 % confidence interval with a 50 km cut-off.

Figure 1.4: Historic TV access and believe in Conspiracies during Covid

Figure 1.4 plots the coefficients for a regressing the treatment on a binary agreement with the statements from Table 1.24, i.e. the respondents that “agree somewhat” or “agree strongly”. Respondents are 31% more likely to agree or strongly agree with the statement that the

Media manipulate information. Given that the median does not agree (see Table 1.20), this effect is sizeable and indicates that the historic exposure to West German TV affects beliefs about manipulation through the media today.

Moreover, there is no significant difference in agreement with the other conspiratorial statements. In particular, respondents are not more likely to agree that “the Media and politics are in cahoots” and are, if anything, less likely to trust their instincts over experts, although the difference is not significant. This lends support to two hypotheses: First, it is unlikely that post-treatment support for the AfD is driving the result, as the AfD is pushing other narratives, e.g. about the corrupt Media or politicians or as well, but does not seem differentially successful in swaying respondents in treated versus untreated areas. Second, it suggests that the effect is about media literacy instead of media trust. If differential trust in media would be important, we would expect respondents to agree more with the conspiracy theory that the Media and politics are in cahoots. Moreover, although the difference is not significant at conventional levels, there is some indication that respondents would trust experts more than their instincts. Taken together, lack of exposure to West TV appears to make respondents less certain about whether information is manipulated or not.

5.3 Alternative Hypotheses

This section exploits the rich survey data from the German Socio-economic panel from 1990-2021 to investigate if any other differences in political preferences could drive the result. Table 1.19 describes the overview over the waves and variables used. As mentioned, the SOEP features a large East German sub-sample of about 5,000 – 6,000 respondents per wave and is apt to reproduce the main result on voting for the AfD and historic exposure to West TV (see section A.4). I use this survey to address whether respondents in treated area have had more right-wing preferences before the AfD and to revisit other findings from the literature with the improved identification strategy.

I find no evidence for an increased preference for right-wing or populist policies due to the treatment that pre-date the AfD. Table 1.27 show that since 1999, there are no significant differences in whether respondents are worried about immigration in treatment vs. control areas, except for one survey wave in 2016 *after* the arrival of the AfD ¹⁶. Similarly, respon-

¹⁶Note that Hornuf et al. (2023) argue in more recent work that West TV has actually reduced xenophobia in East Germany. While this would, if anything, increase the difficulty of the AfD to sway voters and bias the results downwards, I am unable to reproduce these results, possibly due to the fact that their treatment assignment is on the level of the 200 counties instead of the 2700 municipalities (one order of magnitude less precise) and does not control for the distance to West Germany, a key confounder for access to West German TV. Moreover, they rely on specific questionnaires from the 2016 and 2018 survey waves with much

dents are not robustly more worried about law and order in 2006 and 2016, with the exception of the 1996 wave. (see Table 1.28. Table 1.29 confirms that respondents do not consistently place themselves more on the right of the political spectrum. Overall, the absence of higher preferences for populist policies is in line with the event study arguments and the evidence on the impact of Facebook likes in treated areas.

In contrast, Table 1.26 suggests that respondents in areas without historic exposure to West TV are less informed about fundamental elements of the political system. For example, in 2018, 13 years into Angela Merkel’s tenure, they are 19.3% less likely to know that she is a member of the CDU party. This is large - one third of the mean - and economically significant in a party-based political system. Similarly, they are less often able to tell which party was the largest at a time when the CDU was consistently much larger than the second placed SPD in polls and in parliament. Yet, respondents are not less likely to be interested in politics or to claim to follow political debates. This finding is in line with the fact that, to date, treated areas consume less public broadcasters that pursue an educational mandate. It also lends support to the hypothesis that the lack of exposure to West German TV reduced the media literacy of voters and made them less sophisticated in their information acquisition, thus rendering them more susceptible to populist narratives they encounter online.

Finally, the fine-grained, representative and rich SOEP data allows to revisit earlier findings from the literature based on smaller GDR survey data¹⁷. Table 1.25 shows that values of respondents were well balanced in 1993, which includes categories associated with material aspirations for which Hyll and Schneider (2013) and Hennighausen (2015) found a positive association in the GDR survey data. Either these effects were short lived or due to biases in responses¹⁸. Moreover, Table 1.30 uses the large and representative 1990 SOEP wave to shed more light on the hypothesis of West TV as “Opium for the Masses” by Kern and

smaller sub-samples instead of the regular immigration module available annually since 1999 for the full East German sample. In any case, their findings could be interpret as successful persuasion of the AfD since they are measured *after* the arrival of the AfD.

¹⁷Note that Friehe et al. (2020) provide related - and at times contradicting - suggestive evidence from various surveys for the early 1990s on broadly similar variables. I improve on this on three counts: First, in contrast to parts of the data used in Friehe et al. (2020) that come from the GDR Institute *Zentralinstitut für Jugendforschung*, the SOEP is representative. Second, the *ALLBUS* and *Politbarometer* that Friehe et al. (2020) use only gives a breakdown by NUTS2 regions, which is by two orders of magnitude less precise than the zipcode breakdown in the SOEP. Beyond lower precision, this high-level treatment assignment effectively only identifies the *entire* generally more conservative region of east Saxony without including the distance to West Germany as the key confounder (similarly for (Hornuf et al., 2023)). Third, the SOEP sample size for East Germany is large and it asks similar questions in a consistent manner over time, which makes it possible to search robustly and persistently for differences.

¹⁸This further suggests that economic effects found in other papers (Bursztyn and Cantoni, 2016; Hennighausen, 2015; Hyll and Schneider, 2013; Laudenbach et al., 2020) on higher levels of aggregation are unlikely to matter for much for the comparison on the municipality level

Hainmueller (2009): While respondents in areas without access to West TV were indeed less satisfied with their life overall and their evaluation of life 5 years ago, they were actually *more* satisfied with the GDR's democracy and life, suggesting that West TV actually undermined support for the GDR regime.

6 Conclusion

This paper has shown that historic exposure to West TV is echoed by persistent differences in media consumption patterns that translate into support for the right-wing populist AfD in East Germany. It further argues that it is the AfD's specificities in terms of online campaigning methods and political messaging that are at play and rejects the notion that differences in political attitudes are driving the result. There is still progress to be made to better understand the interaction of media consumption patterns and the success of new populist parties. In particular, further efforts could be devoted to study how competing political parties strategically react to the entrance of a populist party that capitalises on different media consumption patterns.

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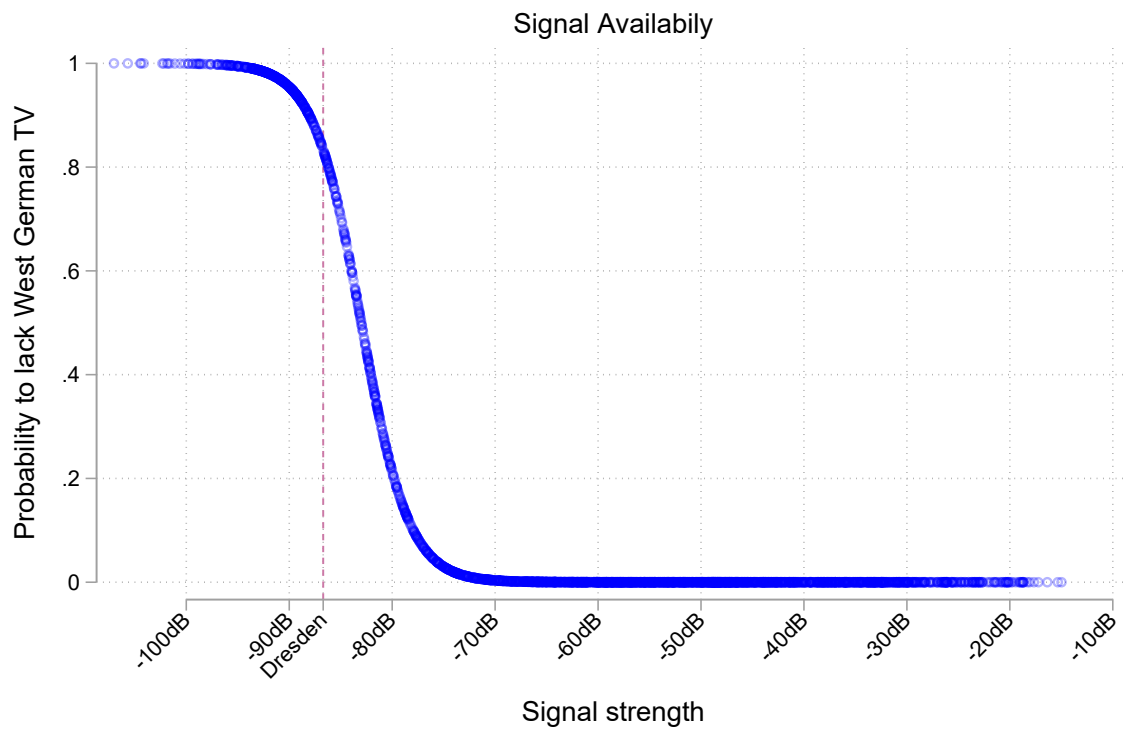
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A Appendices

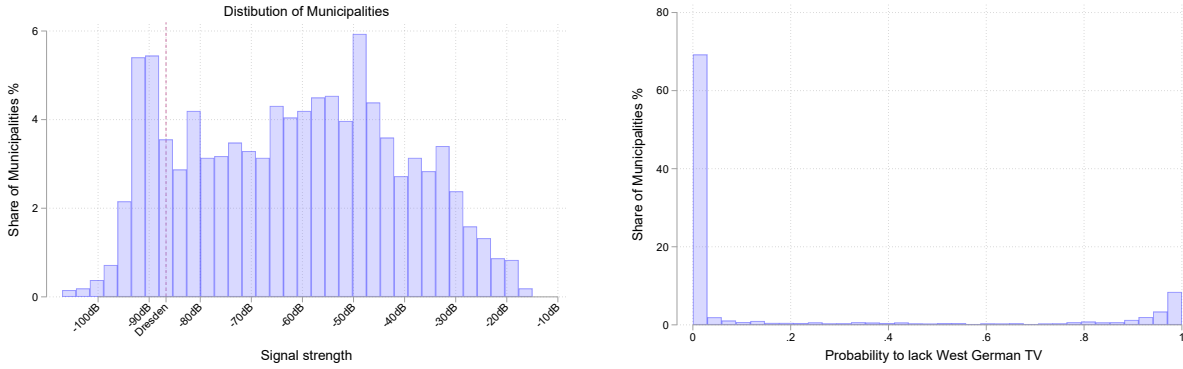
A.1 Identification

Treatment definition



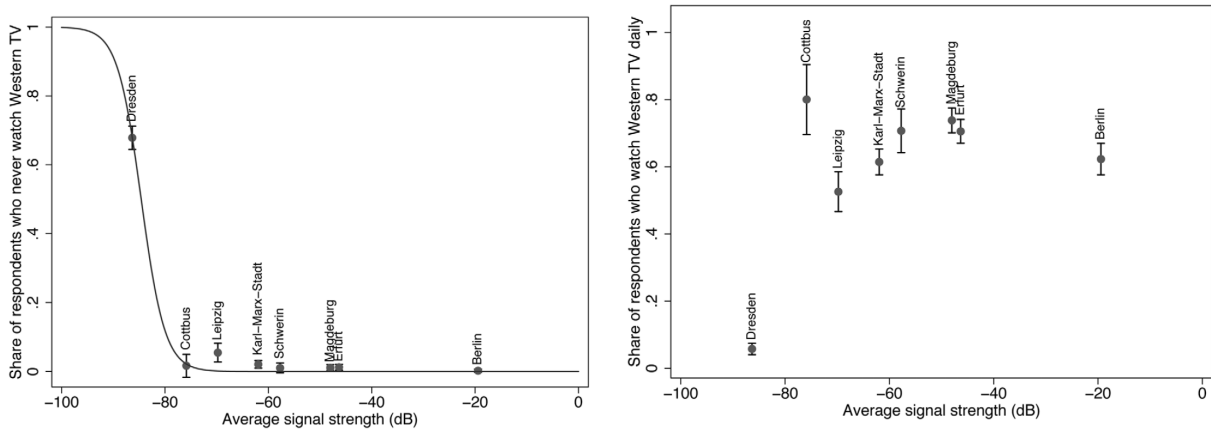
Notes: The figure plots the signal strength from Bursztyn & Cantoni (2016) on the exponentially declining probability to have received West German Television for East German municipalities. Signal reception declines exponentially around a threshold, which is calibrated for the city of Dresden (signal strength: $db = -86.7$) to have had a 0.8 probability of not having had the signal, since anecdotal and survey evidence shows that Dresden's population did not have access to West German TV for the most part. The resulting logistic cdf is fit with $\mu = -84.6$ and $\sigma = 2.3$ as in Bursztyn & Cantoni (2016).

Figure 1.5: Treatment definition following Bursztyn & Cantoni (2016) (part 1)



Notes: The left-hand side figure plots the probability of not having received West German TV in a municipality as a function of TV signal strength from Bursztyn & Cantoni (2016). The right-hand side figure plots the density of municipalities along over the signal strength range. Signal reception declines exponentially around a threshold, which is calibrated for the city of Dresden (signal strength: $db = -86.7$) to have had a 0.8 probability of not having had the signal, since anecdotal and survey evidence shows that Dresden’s population did not have access to West German TV for the most part.

Figure 1.6: Treatment definition following Bursztyn & Cantoni (2016) (part 2)



Notes: Data from the *Zentralarchiv für Empirische Sozialforschung* (ZA 6008) as reported by Bursztyn & Cantoni (2016). They plot the share responding “daily” (left panel) and “never” (right panel), omitting “several times per week”, “once per week”, “less than once per week”. Bars indicate 95% districts were not covered in this survey. The upper panel also displays the best fit of a logistic cdf to the observed data. Source: .

Figure 1.7: Treatment compliance (from Bursztyn & Cantoni, 2016)

Pre-treatment balance

Table 1.5: Economics in 1933

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
Population	523	32340.902 (755.722)	1989	35202.112 (533.609)	2507	-2861.210
labor force	469	631.098 (4.840)	1660	648.129 (2.689)	2124	-17.031**
female workers	469	227.097 (2.126)	1660	240.645 (1.374)	2124	-13.548**
workforce	469	568.293 (4.718)	1660	568.217 (2.701)	2124	0.076***
unemployed	469	62.804 (1.794)	1660	79.912 (0.996)	2124	-17.107***
housewives	469	347.237 (3.019)	1659	338.720 (1.769)	2123	8.516**
agriculture workers	469	346.994 (5.593)	1660	306.830 (3.466)	2124	40.164***
industrial workers	469	115.481 (3.447)	1660	163.610 (1.965)	2124	-48.128***
trade workers	469	61.162 (1.832)	1660	57.502 (0.482)	2124	3.659**
public workers	469	23.887 (0.593)	1660	24.982 (0.316)	2124	-1.095
officials	469	14.678 (0.430)	1660	15.120 (0.218)	2124	-0.443
hired workers	469	30.597 (0.540)	1660	30.140 (0.289)	2124	0.457
manual workers	469	274.871 (2.722)	1660	248.754 (1.572)	2124	26.117

Notes: Data from Falter

(1990) for the year 1933. East German municipalities are matched to their 2017 borders thanks to data from Cantoni et al. (2019). Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

Present-day balance tables

Table 1.6: Demographics and asylum seekers

Variable	(1)		(2)		(1)-(2)	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
population	550	4131.015 (1033.285)	2102	6610.849 (1747.348)	2646	-2479.834
% female population	550	49.492 (0.100)	2102	49.624 (0.048)	2646	-0.132
% Population change (1990-2017)	524	-13.375 (0.976)	1939	-6.539 (0.556)	2463	-6.836
% Population change (1933-1990)	507	-32.580 (29.740)	1891	-31.717 (8.365)	2398	-0.863
% Population change (1933-2017)	523	-38.582 (31.199)	1989	-34.823 (7.286)	2507	-3.759
asylum (full) p. 1000	542	66.067 (15.558)	2081	57.794 (4.948)	2617	8.273
asylum (none) p. 1000	542	28.167 (6.038)	2081	28.024 (2.348)	2617	0.143
asylum (partial) p. 1000	542	4.191 (0.999)	2081	6.944 (0.701)	2617	-2.753
asylum (rejected) p. 1000	542	2.186 (0.576)	1835	1.724 (0.163)	2371	0.462

Notes: Data for

municipalities in 2017 from the Federal Office for Statistics. Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

Table 1.7: Area and geography

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
Longitude	550	13.505 (0.032)	2102	11.963 (0.025)	2652	1.542***
Latitude	550	53.123 (0.055)	2102	51.776 (0.024)	2652	1.347***
Distance to West (h)	544	2.054 (0.022)	2102	0.811 (0.009)	2646	1.243***
area (ha)	550	3550.547 (132.901)	2102	4238.377 (125.634)	2652	-687.830
% agriculture	550	61.562 (0.922)	2102	59.125 (0.465)	2652	2.437*
% buildings	550	6.870 (0.270)	2102	6.840 (0.143)	2652	0.030
% forest	550	21.712 (0.762)	2092	26.036 (0.451)	2642	-4.324
% green	550	85.952 (0.473)	2102	86.906 (0.196)	2652	-0.953
% lakes	540	2.128 (0.213)	1985	1.532 (0.093)	2525	0.596
% river	550	0.848 (0.040)	2084	0.854 (0.022)	2634	-0.006
% traffic	550	3.410 (0.118)	2102	3.951 (0.041)	2652	-0.541
% wasteland	546	1.255 (0.073)	1829	0.982 (0.037)	2375	0.273**
% water	550	3.765 (0.328)	2100	2.303 (0.091)	2650	1.461

Notes: Data for

municipalities in 2017 from the Federal Office for Statistics. Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

Table 1.8: Economy and Employment

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
% unemployed	493	19.662 (0.602)	1813	15.427 (0.269)	2300	4.235
% foreign of unemployed	367	3.846 (0.226)	1395	4.540 (0.149)	1758	-0.695
% longterm of unemployed	547	34.930 (0.530)	2048	34.507 (0.269)	2589	0.423
% old of unemployed	547	33.237 (0.426)	2071	34.156 (0.272)	2612	-0.919
% youth25 of unemployed	528	5.624 (0.170)	1912	5.910 (0.122)	2434	-0.287
jobs p. 1000	550	385.685 (1.838)	2102	414.266 (1.054)	2646	-28.581
outcommuters p. 1000	549	322.892 (3.004)	2095	354.208 (1.649)	2638	-31.316
firms p. 1000	208	1.095 (0.061)	1066	1.316 (0.046)	1273	-0.221
GDP p. capita (€)	550	219.910 (49.750)	2102	202.205 (15.912)	2646	17.705
% agriculture in GDP	550	3.101 (0.043)	2100	2.866 (0.028)	2644	0.235***
% constr in GDP	550	8.191 (0.066)	2102	8.450 (0.040)	2646	-0.259***
% manufacturing in GDP	550	17.862 (0.390)	2102	27.113 (0.181)	2646	-9.251
% public in GDP	550	31.563 (0.212)	2102	25.164 (0.094)	2646	6.399***
% services in GDP	550	21.445 (0.109)	2102	20.085 (0.063)	2646	1.361***
% trade in GDP	550	17.838 (0.122)	2102	16.324 (0.090)	2646	1.514***

Notes: Data for

municipalities in 2017 from the Federal Office for Statistics. Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

Table 1.9: Internet and mobile signal

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
% with 1 Mbits (2013)	549	79.186 (1.091)	2094	85.354 (0.481)	2637	-6.168
% with 2 Mbits (2013)	549	69.000 (1.271)	2094	81.045 (0.529)	2637	-12.044
% with 6 Mbits (2013)	549	41.959 (1.486)	2094	56.067 (0.739)	2637	-14.107
% with 16 Mbits (2013)	549	6.052 (0.918)	2094	3.913 (0.368)	2637	2.139
% with 30 Mbits (2013)	549	0.030 (0.023)	2094	0.066 (0.047)	2637	-0.036
% with 50 Mbits (2013)	549	0.030 (0.023)	2094	0.006 (0.006)	2637	0.024
% with 3G (2019)	432	44.629 (1.567)	1363	44.508 (0.977)	1789	0.120
% with LTE (2019)	432	87.081 (0.792)	1363	88.126 (0.528)	1789	-1.045
% HH with DSL acces (2005)	533	43.574 (1.593)	1994	51.224 (0.765)	2527	-7.650*
% HH with DSL acces (2006)	533	68.789 (1.328)	1994	71.250 (0.635)	2527	-2.461*
% HH with DSL acces (2007)	533	61.587 (1.700)	1994	71.064 (0.751)	2527	-9.477
% HH with DSL acces (2008)	533	76.434 (1.327)	1994	79.206 (0.602)	2527	-2.771

Notes: Data for

municipalities in 2017 from the Federal Office for Statistics and the German Ministry of Traffic and Infrastructure. Data on DSL access from Falck et al. (2014). Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5.

Significance: * 0.10 ** 0.05 *** 0.01.

Table 1.10: Earlier post-treatment periods

Variable	(1) No West TV		(2) West TV		(1)-(2) Pairwise t-test	
	N	Mean/(SE)	N	Mean/(SE)	N	Mean difference
% net migration rate, 94-98	533	-0.744 (0.104)	1994	-0.341 (0.045)	2527	-0.403
% net migration rate, 04-08	533	-0.746 (0.047)	1994	-0.587 (0.031)	2527	-0.160
% women, 94-98	533	49.769 (0.076)	1994	50.149 (0.039)	2527	-0.379
% women, 04-08	533	49.498 (0.079)	1994	49.906 (0.039)	2527	-0.408
% women, 90-94	533	50.661 (0.076)	1994	51.033 (0.035)	2527	-0.372
Population, 94-98	533	4490.700 (954.602)	1994	6665.186 (1741.361)	2527	-2174.486
Population, 04-08	533	4349.690 (969.632)	1994	6551.043 (1746.511)	2527	-2201.353
Population, 90-94	533	4837.503 (1017.114)	1994	6921.745 (1783.655)	2527	-2084.242
% 18-65, 94-98	533	69.593 (0.142)	1994	69.960 (0.064)	2527	-0.367**
% 65y+, 94-98	533	16.833 (0.157)	1994	16.750 (0.075)	2527	0.083
% 18-65, 04-08	533	68.930 (0.160)	1994	68.839 (0.079)	2527	0.090***
% 65y+, 04-08	533	19.792 (0.168)	1994	19.443 (0.082)	2527	0.350
% unemployed, 94-98	533	14.363 (0.167)	1994	11.459 (0.077)	2527	2.905*
% unemployed, 04-08	533	16.442 (0.197)	1994	12.680 (0.086)	2527	3.762*

Notes: Data from Falck

et al. (2014). Robust standard errors controlling for distance to West Germany. The binary indicator for No West German television is equal to one if the probability of having received West German TV is less than 0.5. Significance: * 0.10 ** 0.05 *** 0.01.

Pre-treatment balance from the literature

TABLE 1.—REGIONAL CHARACTERISTICS, 1955 AND 1990 (DISTRICT LEVEL), BY TREATMENT STATUS

	Treatment	Control	Difference	SE	P-Value
<i>A: 1955 (District-Level Data)</i>					
Population density (inhabitants/km ²)	206	202	4	77	0.959
Share of employed in agriculture (%)	23.7	27.8	-4.1	11.1	0.744
Share of employed in industry (%)	34.1	28.7	5.4	10.0	0.635
Retail sales per capita (mark)	1,691	1,694	-3	102	0.979
Savings per capita (mark)	277	297	-20	28	0.544
<i>B: 1990</i>					
Population density (inhabitants/km ²)	181	176	5	62	0.941
Share of employed in agriculture (%)	13.5	11.3	2.2	5.1	0.706
Share of employed in industry (%)	33.2	39.5	-6.3	7.5	0.479
Retail sales per capita (mark)	7,577	7,250	327	188	0.190
Savings per capita (mark)	9,312	9,381	-69	928	0.946
Cars per 1,000 inhabitants	237.4	237.6	-0.2	12.1	0.992
<i>C: Difference 1990–1955</i>					
Population density (inhabitants/km ²)	-18	-26.2	8.2	15.4	0.626
Share of employed in agriculture (%)	-14.5	-12.6	-1.9	6.0	0.778
Share of employed in industry (%)	5	5.5	-0.5	3.0	0.870
Retail sales per capita (mark)	5,862	5,557	305	157	0.142
Savings per capita (mark)	8,946	8,994	-48	770	0.954

Population-weighted averages, excluding the district of East Berlin. P-values based on weighted Welch's t-tests of difference in means (two-sided, allowing for unequal variances). Total number of districts: fourteen (eleven treatment, three control).

Source: Statistisches Amt der DDR (1955, 1990).

Figure 1.8: District-level balance in the GDR, Bursztyn & Cantoni (2016).

Migration rate to West Germany, 1991–1993 in % of original population aged:	Treatment	Control	Diff.	Std. err.	p-value
below 18	1.047	1.035	0.012	0.076	0.870
18–25	4.679	4.704	-0.025	0.235	0.917
25–30	3.636	3.486	0.150	0.184	0.420
30–50	1.731	1.617	0.114	0.11	0.306
50–65	0.447	0.486	-0.039	0.034	0.262
above 65	0.358	0.407	-0.049	0.039	0.227

Figure 1.9: Migration to the West immediately post-treatment, Bursztyn & Cantoni (2016).

Origin	Total	by destination:				Population
		Berlin	East Germany, Treatment	East Germany, Control	West Germany	
East Germany, Treatment	821,873 (6.38%)	43,106 (0.33%)	546,684 (4.25%)	30,981 (0.24%)	201,102 (1.56%)	12,873,985
East Germany, Control	100,934 (6.17%)	4,141 (0.25%)	32,421 (1.98%)	36,640 (2.24%)	27,732 (1.7%)	1,634,665

Figure 1.10: Migration destinations immediately post-treatment, Bursztyn & Cantoni (2016).

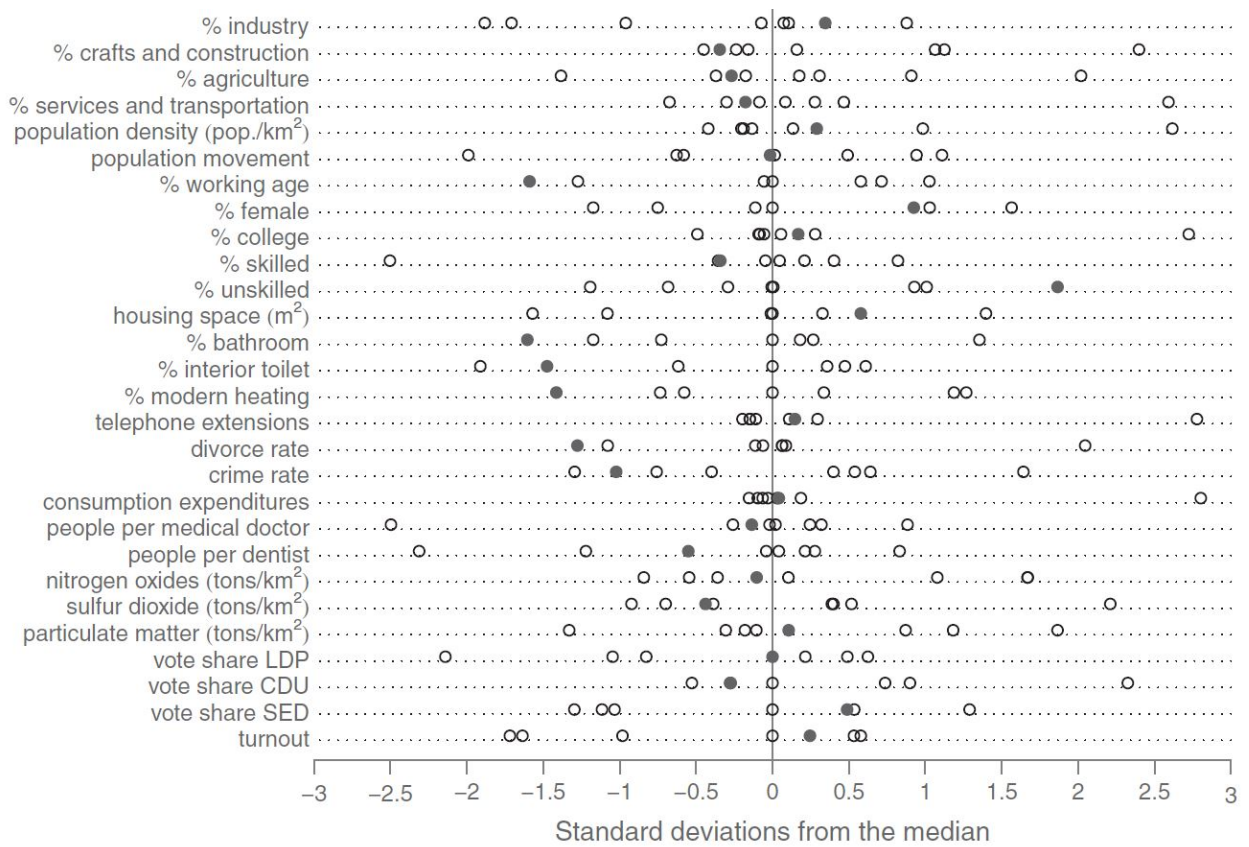


Figure 1.11: Dresden vs other GRD cities, Kern & Hainmüller (2009).

Table 1: Electoral Outcomes for Key Parties in the Reichstag Elections (Districts in Italics Comprise Control Areas).

Electoral district	Party vote share					
	KPD	SPD	Zentrum	DVP	DNVP	NSDAP
	July 1932					
Chemnitz-Zwickau	19.6	22.4	0.7	0.8	3.8	47.1
<i>Dresden-Bautzen</i>	14.3	31.1	2.1	2.9	5.5	39.3
Leipzig	18.7	33.1	1.1	2.2	4.5	36
Mecklenburg	9.4	31.3	1.2	1.7	9.5	44.8
<i>Pomerania</i>	10.8	21	1.5	0.9	15.8	48
	November 1932					
Chemnitz-Zwickau	21.4	22.3	0.6	1.4	5.1	43.4
<i>Dresden-Bautzen</i>	17	29.5	1.9	4.1	8.1	33.9
Leipzig	20.7	32.2	1.1	3.3	7.2	31
Mecklenburg	11.7	30.5	0.9	2.3	15.3	37
<i>Pomerania</i>	12.1	19.8	1.2	1.1	20.7	43.1
	March 1933					
Chemnitz-Zwickau	19	21.3	0.6	0.9	5.4	50
<i>Dresden-Bautzen</i>	13.4	28.4	2.0	2.5	7.7	43.6
Leipzig	17.4	30.1	1	2	6.5	40
Mecklenburg	7.4	26.5	0.8	1.3	14.9	48
<i>Pomerania</i>	7.6	16.2	1.1	0.7	17	56.3

Notes: The electoral district of Dresden-Bautzen is about the same as the district of Dresden during the GDR era. The electoral district Pomerania includes regions without access to West German TV, but also other ones. *Source: Falter et al. (1986).*

Figure 1.12: District-level elections pre-treatment, Friehe et al. (2020).

A.2 Main Results - robustness

Treatment definition

Table 1.11: AfD Vote shares (2013-2019), TV as binary indicator

	Vote Share of the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
No West TV	3.243** (1.463)	1.250** (0.583)	1.207** (0.543)	1.212** (0.540)	1.160** (0.523)	1.122** (0.513)
Distance to West	No	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes	Yes
Election FE	No	Yes	Yes	Yes	Yes	Yes
Hist. controls	No	No	Yes	Yes	Yes	Yes
Geog. controls	No	No	No	Yes	Yes	Yes
Mobile signal	No	No	No	No	Yes	Yes
Socio-economics	No	No	No	No	No	Yes
Observations	12045	12015	12015	12015	12015	12015
R-squared	0.0191	0.756	0.764	0.765	0.766	0.772
Dep. mean	15.8	15.8	15.8	15.8	15.8	15.8
Dep. SD	(10.0)	(10.0)	(10.0)	(10.0)	(10.0)	(10.0)

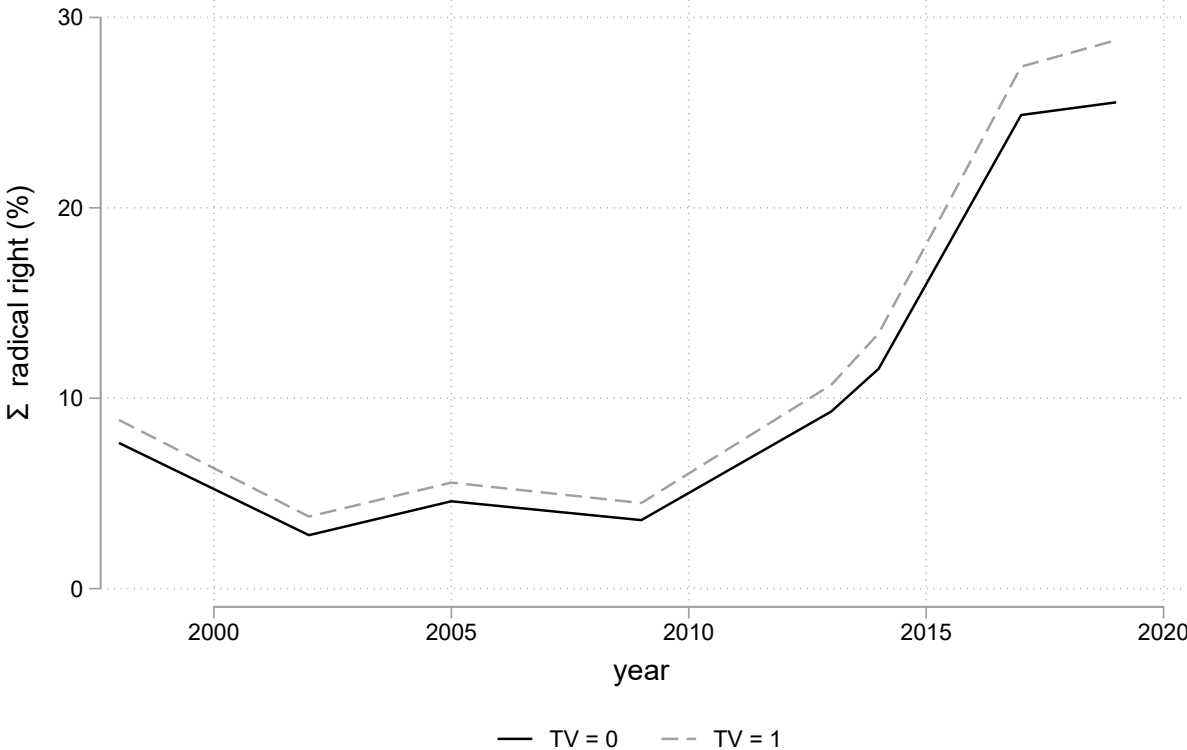
Notes: P-value significance: * 0.10 ** 0.05 *** 0.01. Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Spatial OLS with Conley standard errors with 50 km cut-off. No West TV is defined as binary indicator equal to one if the probability of having received West German TV is less than 0.5.

Table 1.12: AfD Vote shares (2013-2019), dropping municipalities with ambiguous treatment status

	Vote Share of the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
No West TV	3.789** (1.632)	1.635* (0.879)	1.622* (0.834)	1.616* (0.828)	1.528** (0.770)	1.473* (0.759)
Distance to West	No	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes	Yes
Election FE	No	Yes	Yes	Yes	Yes	Yes
Hist. controls	No	No	Yes	Yes	Yes	Yes
Geog. controls	No	No	No	Yes	Yes	Yes
Mobile signal	No	No	No	No	Yes	Yes
Socio-economics	No	No	No	No	No	Yes
Observations	10883	10883	10883	10883	10883	10883
R-squared	0.0226	0.762	0.769	0.770	0.771	0.777
Dep. mean	15.6	15.6	15.6	15.6	15.6	15.6
Dep. SD	(9.8)	(9.8)	(9.8)	(9.8)	(9.8)	(9.8)

Notes: P-value significance: * 0.10 ** 0.05 *** 0.01. Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Spatial OLS with Conley standard errors with 50 km cut-off. No West TV is defined as the predicted lack of West Germany TV calibrated to the signal strength of Dresden (-86.7), for which the signal was not available most of the time. Municipalities with an ambiguous treatment status with a probability between 0.8 and 0.2 are dropped.

Right-wing Populist Parties over time



Notes: The figure plots the mean vote shares of right-wing populist parties over time. The data are pooled election data for Federal elections (1998, 2002, 2005, 2009, 2013, 2017), EU elections (2009, 2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). No West TV is the probability that a municipality could not access West German Television, see section 3.1. Right-wing Populist Parties include the AfD (since 2013), the NPD DVU, Republicans, BfB and ProDM.

Figure 1.13: Trends in Vote Shares for Right-wing Populist Parties

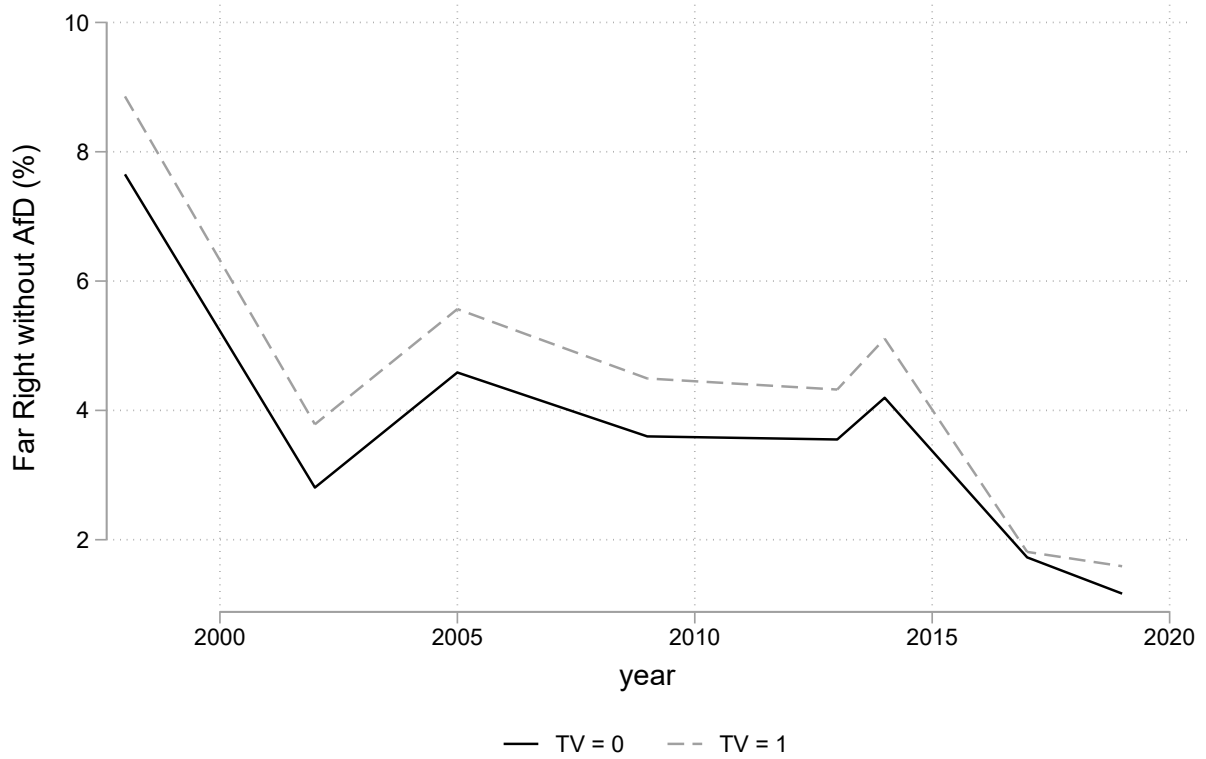


Figure 1.14: Trends in Vote Shares for Right-wing Populist Parties excluding the AfD

Notes: The figure plots the mean vote shares of right-wing populist parties over time. The data are pooled election data for Federal elections (1998, 2002, 2005, 2009, 2013, 2017), EU elections (2009, 2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). No West TV is the probability that a municipality could not access West German Television, see section 3.1. Right-wing Populist Parties include the NPD DVU, Republicans, BfB and ProDM.

Table 1.13: Event study of voting differences over time

	Vote Share of respective parties				
	turnout	far right	right	left	far left
year=1998 × No West TV	0.655 (1.161)	0.348 (0.453)	0.007 (1.057)	-3.661*** (0.813)	3.430*** (0.744)
year=2002 × No West TV	-2.574** (1.162)	-0.007 (0.429)	0.758 (0.840)	-2.823*** (0.883)	2.626*** (0.776)
year=2005 × No West TV	-2.190* (1.114)	0.065 (0.366)	1.482* (0.868)	-2.457*** (0.730)	0.451 (0.657)
year=2009 × No West TV	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
year=2013 × No West TV	-1.674 (1.235)	0.757 (0.871)	-0.684 (0.965)	1.248 (0.833)	-1.019 (0.722)
year=2014 × No West TV	1.609 (1.074)	0.861 (0.982)	-0.879 (1.299)	0.819 (0.868)	-0.738 (0.732)
year=2016 × No West TV	-1.365 (1.391)	3.612** (1.796)	-3.556*** (1.345)	-1.833 (1.734)	0.403 (0.795)
year=2017 × No West TV	-1.583 (1.341)	2.823 (1.892)	-5.728*** (1.871)	2.054** (0.838)	1.215 (0.749)
year=2019 × No West TV	0.748 (1.251)	3.020* (1.607)	-4.876*** (1.371)	2.770*** (1.032)	0.294 (0.712)
State FE	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes
Distance to West	Yes	Yes	Yes	Yes	Yes
Observations	33,280	33,280	33,280	33,280	33,280
R-squared	0.647	0.873	0.477	0.836	0.584
Dep. mean	70.321	9.543	38.598	28.092	18.869
Dep. SD	(12.821)	(8.910)	(9.414)	(11.985)	(7.043)

Notes: Panel of elections for Federal elections (1998, 2002, 2005, 2009, 2013, 2017), EU elections (209, 2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Estimated equation:

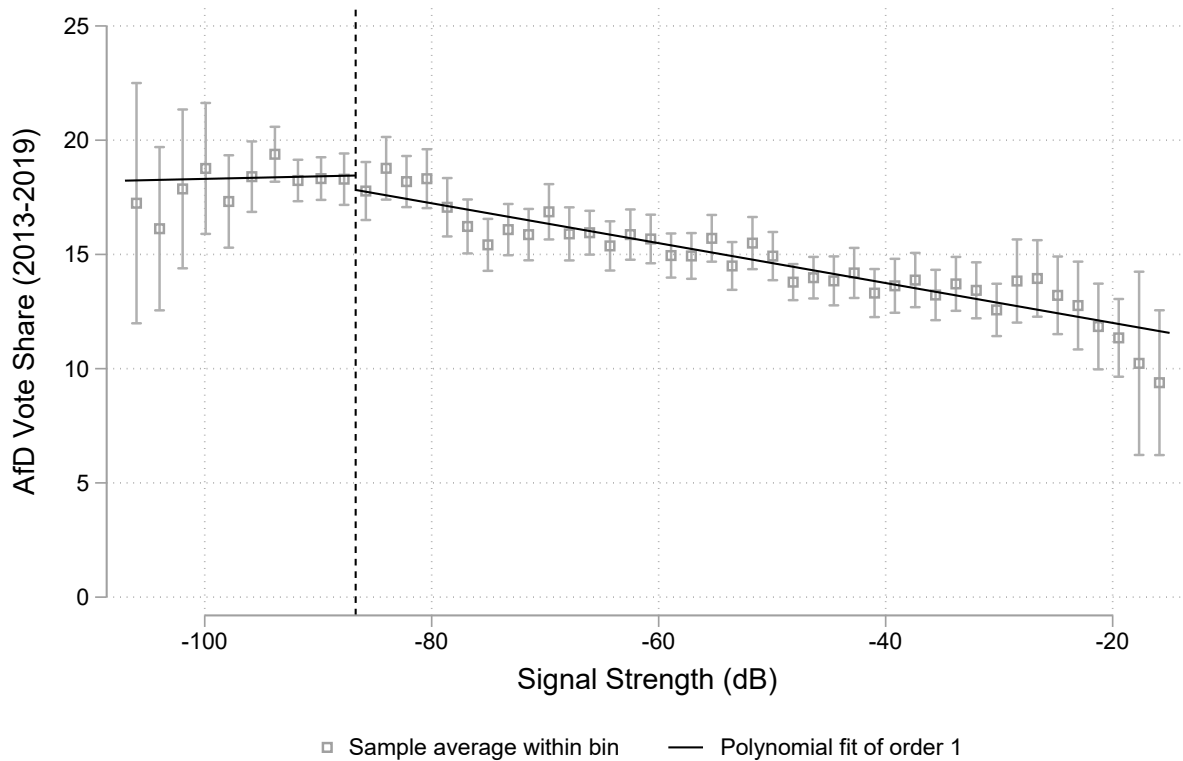
$y_{isey} = \alpha_s + \theta_e + \phi_y + \eta * distance_i + \phi_y * NoTV_i + \epsilon_{isey}$, where y_{isey} is the vote share or turnout in percentage points in municipality i in state s for election e in year y . The omitted year is 2009, the last election before the foundation of the AfD in 2013, shortly before the 2013 Federal election. Far right parties: NPD, DVU, Republicans, BfB and ProDM. Mainstream: CDU, SPD, FDP and Greens. Far left: Linke/PDS, MLPD, PASS, PSG. Right parties: CDU, FDP. Left Parties: SPD, Greens. Standard errors are clustered at the regional level. P-value significance: * 0.10 ** 0.05 *** 0.01.

Robust RDD

The municipality level data in which the data is sufficiently dense around the signal cut-off allows for a regression discontinuity approach with the TV signal as a running variable and TV signal strength in Dresden as the cut-off. The identifying assumption is that the historic exposure is the only change along the running variable at the cut-off. I estimate the following equation :

$$y_{ise} = \alpha_e + \delta_s + \eta * f(signal_{is}) + \beta * \mathbb{1}(signal_{is} < \bar{S}) + \mathbf{X}_{ise} * \boldsymbol{\gamma} + \epsilon_{ise} \quad (1.3)$$

Where $signal_{is}$ is the signal strength in municipality i , $f(\cdot)$ is a polynomial of signal strength and $\bar{S} = -86.7$ is the signal strength of Dresden .



Notes: Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Robust RDD with TV signal strength as running variable and -86.7 dB as cut-off. Municipalities are clustered in 50 bins. P-value significance: * 0.10 ** 0.05 *** 0.01.

Figure 1.15: RDD - pooled elections

Figure 1.15 presents the RDD for voting AfD in elections since its foundation in 2013 until 2019. As can be seen from the plot, the relationship between signal strength and AfD vote

shares is quite smooth up to the threshold at which TV reception was possible. The chosen polynomial here illustrates the fact that the relationship between signal strength seems to be linear and becomes discontinuous at the threshold.

Table 1.14 reports the RDD estimates, regression it on the inverse hyperbolic sine of the AfD vote share to make elections comparable and to account of outliers. The first column is the reduced form effect of not having had access to West German TV before 1990 on the AfD vote share of a village nowadays, controlling of election year fixed effects and geographic features of a municipality. On average, these municipalities vote 8 % more for the AfD.

Column (2) - the preferred specification - addresses the concern that the access to TV signal is correlated with other infrastructure quality that is driving the difference in voting behaviour. There is a large literature showing for Germany Falck et al. (2014) and other countries Gavazza et al. (2019) that the spread of the Internet affects turnout and political engagement. In particular, if municipalities without West TV have geographic features that block off the signal - a key argument to the identification strategy - they might also be less likely to have access to mobile internet technologies like 3G that have been shown to affect confidence in government (Guriev et al., 2020). We control for several dimensions of IT infrastructure: Using data provided by the German Ministry of Infrastructure and Transportation on mobile signal data and internet, we control for the 2013 and 2018 levels of LTE and UMTS coverage and the increase between both periods. Neither when added jointly when added individually (not reported) do these measures change the estimated effect of West TV access. This emphasizes the argument by Stiehler (2001) that it was not TV signal *per se* that was weak, but only West German TV signal as the GDR antennas did cover East Germany rather well. Column (3) adds pre-existent historic difference in voting and socio-economics, which only change the point estimate and SE after the third decimal.

Column (4) includes all controls available on the municipal level that could be themselves affected by the treatment: the universe of municipality -level controls from the German Statistical Office, covering demographics, employment, structure of the local economy, migration patterns, public finances and land use.¹⁹ Most notably, it controls for migration rates since 1990 or the local economic situation. It is remarkable that the inclusion of these controls changes the point estimate only by a little and does not alter its significance. If we expected that West TV affects AfD voting through one of these channels - e.g., the migration

¹⁹The full list includes density, the population change since 2008, the share of women, the average age, the average female age, total emigration and immigration since 2008, female and youth emigration and immigration since 2008, the number of taxpayers, total tax, local corporate and income tax, wages, unemployment, youth and long-term unemployment, labour force participation rate, female participation, land share in manufacturing use and land share in agricultural use.

patterns, local employment, age structure or local taxes - then their inclusion should lower the coefficient or at least partly explain it. The remarkable robustness of the result instead indicates that the effect of West TV on the AfD result is rather direct, i.e. not mediated through a change in other characteristics.

Table 1.14: RDD with threshold at -86.7 Bursztyn & Cantoni (2016)

	Vote Share of the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.870** (0.360)	0.956** (0.455)	1.304*** (0.430)	1.068*** (0.261)	0.896*** (0.249)	1.257*** (0.240)
Election FE		✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Geography + Distance			✓	✓	✓	✓
Mobile signal				✓	✓	✓
History					✓	✓
Soc.-econ.						✓
Observations	12,015	12,015	12,015	12,015	12,015	12,015
Dep. mean	15.85	15.85	15.85	15.85	15.85	15.85
Dep. SD	(9.97)	(9.97)	(9.97)	(9.97)	(9.97)	(9.97)

Notes: Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Robust RDD estimate with TV signal strength as running variable and -86.7 dB as cut-off with MSE-optimal bandwidth selector cluster-robust nearest neighbour standard errors and quadratic polynomial. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.15: RDD with different thresholds

	Vote Share of the AfD Party			
	(1)	(2)	(3)	(4)
	-86.7 dB	-80 dB	-75 dB	-60 dB
RD_Estimate	1.156*** (0.436)	-0.227 (0.245)	-0.014 (0.284)	0.198 (0.318)
Election FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Geography	✓	✓	✓	✓
Observations	11,933	11,933	11,933	11,933
R2	15.85	15.85	15.85	15.85
Dep. mean	(9.98)	(9.98)	(9.98)	(9.98)

Notes: Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Robust RDD estimate with TV signal strength as running variable varying thresholds with MSE-optimal bandwidth selector cluster-robust nearest neighbour standard errors and quadratic polynomial. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.16: RDD with log of AfD vote share

	log of Vote Share of the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
	RD_Estimate	0.083*** (0.020)	0.095*** (0.027)	0.102*** (0.027)	0.084*** (0.018)	0.086*** (0.018)
Election FE		✓	✓	✓	✓	✓
State FE		✓	✓	✓	✓	✓
Geography + Distance			✓	✓	✓	✓
Mobile signal				✓	✓	✓
History					✓	✓
Soc.-econ.						✓
Observations	11,933	11,933	11,933	11,933	11,933	11,933
Dep. mean	2.52	2.52	2.52	2.52	2.52	2.52
Dep. SD	(0.74)	(0.74)	(0.74)	(0.74)	(0.74)	(0.74)

Notes: Pooled election data for Federal elections (2013, 2017), EU elections (2014, 2019) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). Robust RDD estimate with MSE-optimal bandwidth selector cluster-robust nearest neighbour standard errors and quadratic polynomial. P-value significance: * 0.10 ** 0.05 *** 0.01.

Social media

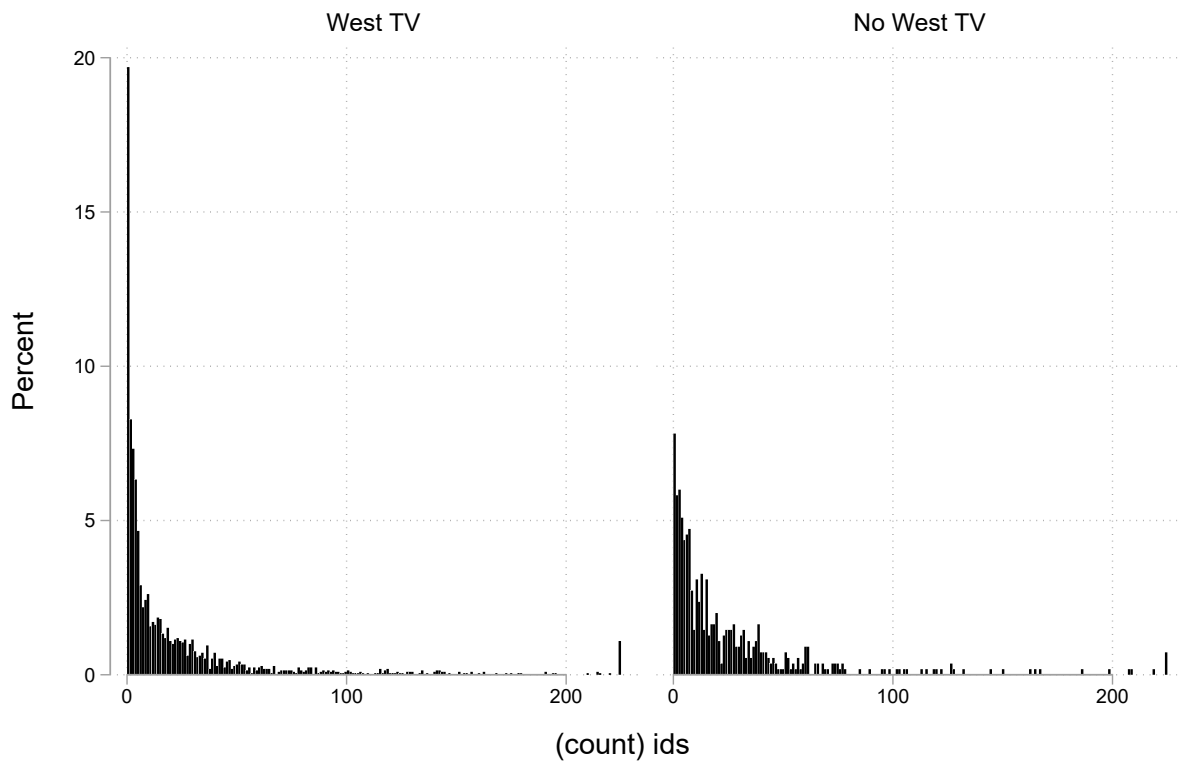


Figure 1.16: Facebook accounts per 6,000 inhabitants

Notes: East German Municipalities in 2017 merged with data from Müller & Schwarz (2021), who geolocate Facebook users that interact with the AfD Facebook page through likes, posts or comments before 2015 and in 2017. 6,000 is chosen as it represents the median size of a municipality in the data, so the values can be interpreted for the typical municipality. Values are capped at the 99th percentile in line with the data provided by Müller & Schwarz (2021). Robust standard errors in parenthesis. P-value significance: * 0.10 ** 0.05 *** 0.01.

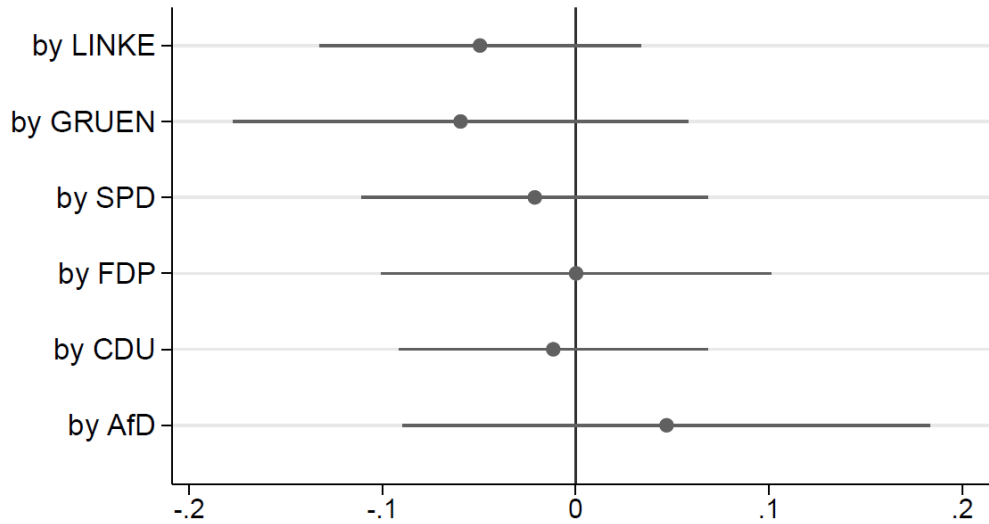
A.3 German longitudinal panel

Campaigning behaviour

Table 1.17: Media outcomes in the German Longitudinal Election Survey

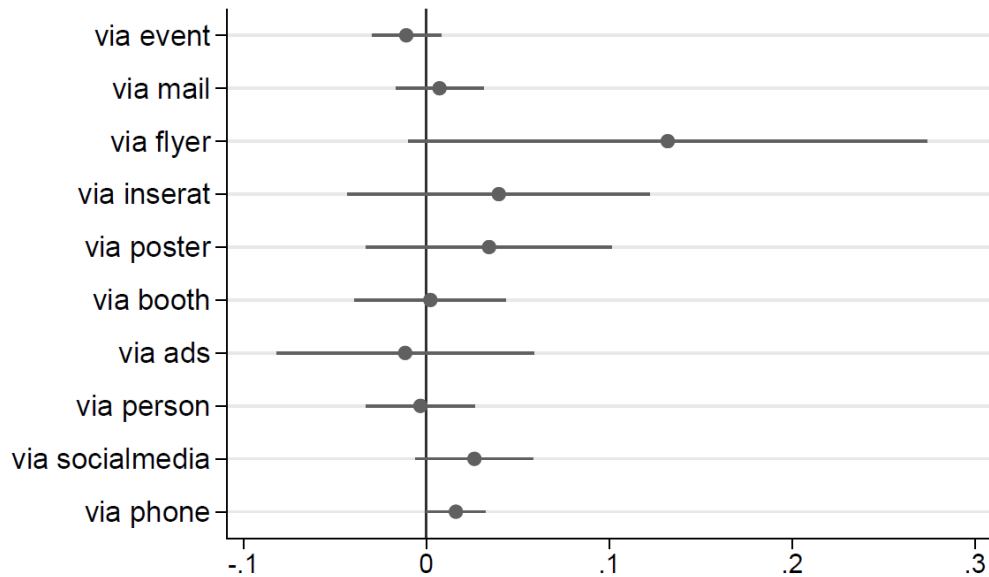
Outcomes					
	count	mean	sd	min	max
Vote AfD	2993	.094	.292	0	1
No West TV	3869	.135	.341	0	1
Main source: TV	3787	.549	.498	0	1
Main source: newspaper	3787	.207	.405	0	1
Main source: radio	3787	.066	.249	0	1
Main source: Interet	3787	.133	.340	0	1
Main source: friends	3787	.032	.176	0	1
days watch ARD news	2381	3.583	2.455	0	7
days watch ZDF news	2359	2.318	2.327	0	7
days watch RTL news	2343	1.177	1.909	0	7
days watch SAT1 news	2333	.672	1.392	0	7
days watch PRO7 news	948	.286	1.035	0	7
days watch public news	3810	4.098	2.577	0	7
days watching private news	3748	1.693	2.291	0	7
Controls					
age	3868	52.660	18.216	16	99
female	3869	.511	.500	0	1
married	3855	.532	.500	0	1
highschool	3864	.226	.418	0	1
income >p50	3456	.382	.486	0	1
urban	1447	.695	.461	0	1
highschool	3864	.226	.418	0	1
religious	3816	.380	.486	0	1

Notes: Data from the pooled GLES survey for the 2013 and 2017 federal election and the 2014 and 2016 State election in Saxony and Pomerania (N=3787).



Notes: Pooled GLES surveys for Federal elections (2013, 2017) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). N = 3588. Each line represents a regression on a dummy if the respondent has been contacted by parties about the election via the stated channel. Spatial OLS with Conley standard errors with 50 km cut-off with 95 % confidence intervals. P-value significance: * 0.10 ** 0.05 *** 0.01.

Figure 1.17: Parties Campaigning contact



Notes: Pooled GLES surveys for Federal elections (2013, 2017) and State elections (Saxony: 2016, Mecklenburg-Vorpommern: 2014). N = 3588. Each line represents a regression on a dummy if the respondent has been contacted by parties about the election via the stated channel. Spatial OLS with Conley standard errors with 50 km cut-off with 95 % confidence intervals. P-value significance: * 0.10 ** 0.05 *** 0.01.

Figure 1.18: Parties Campaigning methods

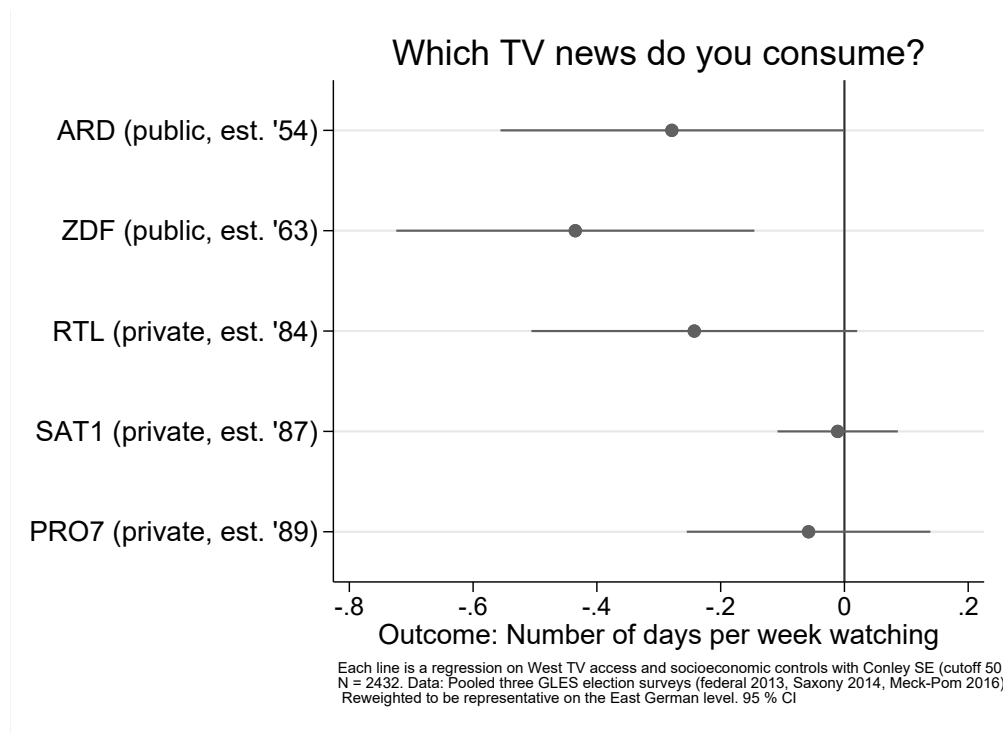


Figure 1.19: Differences in TV News consumption by channel creation date

Reproducing the main result

Table 1.18: AfD vote and TV access in GLES survey data

	Respondent voted for the AfD Party					
	(1)	(2)	(3)	(4)	(5)	(6)
No West TV	0.0657** (0.0271)	0.0366* (0.0222)	0.0294** (0.0130)	0.0294* (0.0172)	0.269** (0.129)	0.0294** (0.0147)
Observations	2993	2993	2977	2977	2969	2977
ymean	0.0942	0.0942	0.0942	0.0939	0.0941	0.0942
ysd	0.292	0.292	0.292	0.292	0.292	0.292
state FE	No	Yes	Yes	Yes	Yes	Yes
election FE	No	Yes	Yes	Yes	Yes	Yes
controls	No	No	Yes	Yes	Yes	Yes
regression	OLS	OLS	OLS	OLS	logit	OLS
SE	Conley (50km)	Conley (50km)	Conley (50km)	cl(zip code)	cl(zip code)	Conley (1000km)

Notes: Data from the pooled GLES survey for the 2013 and 2017 federal election and the 2014 and 2016 State election in Saxony and Pomerania (N=3787) as described in appendix table 1.17. Each line regresses lack of historic TV, defined as the probability of not having had access to West TV on a dummy if the source was mentioned, controlling for the variables described in appendix table 1.17. The plotted confidence intervals are constructed with Conley standard errors at the 95 % confidence interval with a 50 km cut-off.

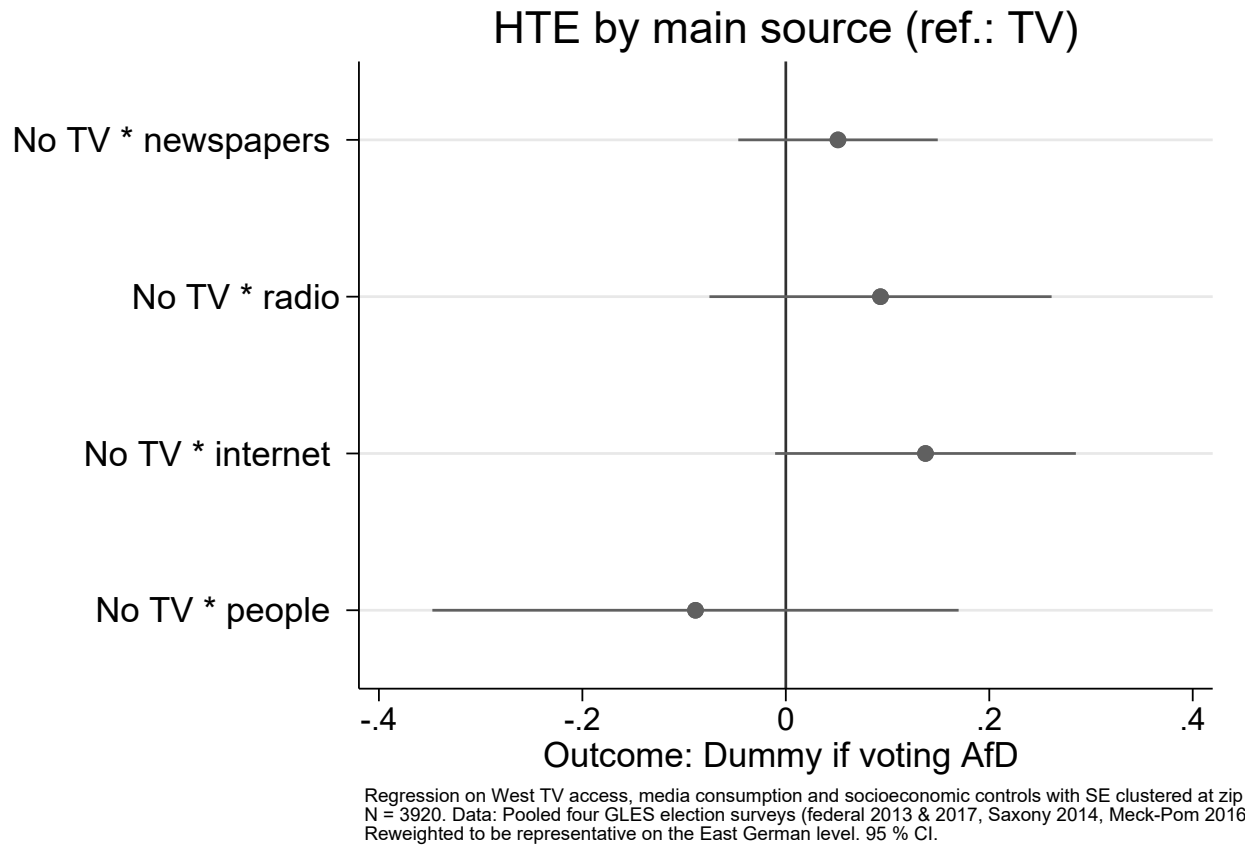


Figure 1.20: Interaction of Treatment with media usage

A.4 German Socio-economic Panel (SOEP)

Summary Statistics

Table 1.19: Summary statistics of SOEP waves

	Mean	Median	SD	Min	Max	N
<i>Vote in Fed. Election (2013, 2017)</i>						
Voted	0.79	1	0.41	0	1	10457
voted==AFD	0.09	0	0.28	0	1	8279
<i>Right-wing attitudes</i>						
Worried about immigration (1999-2020)	1.92	2	0.75	1	3	125187
Left-right placement (05, 09, 14, 19)	4.43	5	1.70	0	10	22700
Importance: Law and order (1996, 2006, 2016)	1.87	2	1.02	1	4	15176
<i>Political knowledge (2018)</i>						
Know: voting age	0.66	1	0.48	0	1	664
Know: Chancellor is CDU	0.56	1	0.50	0	1	664
Know: CDU larger than SPD	0.44	0	0.50	0	1	664
Know: SPD left, CDU right	0.12	0	0.33	0	1	664
Interest in politics	1.80	1	1.22	1	5	619
Understanding of politics	3.96	4	1.13	1	5	632
<i>West TV at residence</i>						
Signal Strength	-55.51	-60	23.25	-107	-15	158945
Absence of West TV	0.07	0	0.26	0	1	158945
Distance to West (h)	0.85	1	0.64	0	3	158945
<i>West TV at place of birth</i>						
West TV Signal Strength	-60.89	-63	17.15	-107	-18	17732
Absence of West TV	0.04	0	0.20	0	1	17732
Distance to West (h)	0.94	1	0.49	0	3	17732
<i>Individual Characteristics</i>						
Age at interview	47.79	47	17.48	15	102	158766
Born Before 1980	0.83	1	0.37	0	1	158945
Female	0.53	1	0.50	0	1	158765
log(net income)	10.20	10	0.60	5	14	156886
Marital Status (cat.)	1.74	1	1.05	1	5	158186
Education years	12.33	12	2.56	7	18	152428
Year	2007.81	2008	7.84	1993	2020	158945

Notes: Data from the SOEP waves merged with treatment.

Table 1.20: Summary statistics SOEP Covid waves

	Mean	Median	SD	Min	Max	N
<i>Information Source on Covid</i>						
source on covid: TV and radio	0.87	1	0.33	0	1	2568
source on covid: Newspapers	0.55	1	0.50	0	1	2568
source on covid: Social media	0.24	0	0.43	0	1	2568
source on covid: Online search	0.49	0	0.50	0	1	2568
source on covid: Friends	0.54	1	0.50	0	1	2568
source on covid: Other	0.07	0	0.26	0	1	2568
source on covid: none	0.00	0	0.07	0	1	2568
<i>Conspiracy Theories</i>						
agreement: Secret dark powers are at work	2.21	2	1.29	1	5	1808
agreement: Politicians are just puppets	2.34	2	1.33	1	5	1817
agreement: Media and politics are in cahoots	2.57	2	1.34	1	5	1827
agreement: I trust instincts more than experts	2.72	3	1.31	1	5	1852
agreement: The media manipulate information	2.93	3	1.37	1	5	1832
<i>Place Characteristics</i>						
Signal Strength	-54.39	-60	23.80	-99	-17	2986
Absence of West TV	0.06	0	0.24	0	1	2986
Distance to West (h)	0.81	1	0.62	0	3	2986
<i>Controls</i>						
Age at interview	54.80	55	16.20	18	97	3215
log(net income)	7.80	8	0.61	5	10	1704
Female	0.59	1	0.49	0	1	3215
Employed	0.53	1	0.50	0	1	3217
Federal state (cat.)	13.43	14	1.71	11	16	1659
Year	2020.48	2020	0.50	2020	2021	3217

Notes: Data from the SOEP Covid waves merged with treatment.

Reproducing the main result

Table 1.21: AfD vote and TV access in the SOEP survey data

	Voted for the AfD		
	(1)	(2)	(3)
No West TV	0.023 (0.017)	0.037** (0.018)	0.038** (0.018)
Distance	✓	✓	✓
state FE		✓	✓
individual controls			✓
Observations	8279	8279	8279
R2	0.008	0.014	0.018
Mean dep. var	0.088	0.088	0.088
Mean no TV	.068	.068	.068

Notes: Data from pooled 2014 and 2018 SOEP waves. Each column represents a spatial OLS regression on a dummy if respondents voted AfD in the 2013 or 2017 federal election. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.22: Main result by TV at residence and age heterogeneity

	Voted for the AfD		
	(1)	(2)	(3)
No West TV	-0.158*** (0.045)	-0.177*** (0.043)	-0.175*** (0.065)
born pre '80	-0.059* (0.032)	-0.059* (0.032)	-0.034 (0.031)
No TV x born pre '80	0.136** (0.062)	0.152** (0.063)	0.154** (0.067)
Distance	✓	✓	✓
state FE		✓	✓
individual controls			✓
Observations	1573	1573	1573
R2	0.009	0.019	0.101
Mean dep. var	0.110	0.110	0.110
Mean no TV	.062	.062	.062

Notes: Data from pooled 2014 and 2018 SOEP waves. Access to West TV is assigned at the place of residence. Each column represents a spatial OLS regression on a dummy if respondents use social media as a source on Covid. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Results from Covid wave

Table 1.23: Use social media as source on Covid

	Use Social Media		
	(1)	(2)	(3)
No West TV	0.082** (0.040)	0.089** (0.040)	0.082* (0.046)
Distance	✓	✓	✓
State FE		✓	✓
Individual Controls			✓
Observations	2337	2337	2337
R2	0.002	0.014	0.095
Mean dep. var	0.250	0.250	0.250
Mean no TV	.06	.06	.06

Notes: Data from the SOEP Covid waves. Each column represents a spatial OLS regression on a dummy if respondents use social media as a source on Covid. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.24: Believe in conspiracies

	“Agree” or “Mostly Agree” with Statement on				
	(1)	(2)	(3)	(4)	(5)
	Dark Powers	puppet politicians	Media corrupt	Distrust experts	media manipulated
No West TV	0.174 (0.171)	0.048 (0.182)	-0.101 (0.188)	-0.232 (0.177)	0.317** (0.124)
Distance	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓
Observations	298	297	297	301	299
R2	0.080	0.035	0.010	0.056	0.091
Mean no TV	.06	.06	.06	.06	.06

Notes: Data from the SOEP Covid waves. Each column represents a spatial OLS regression on a dummy if respondents strongly or mostly agree with the statement, controlling for local TV signal strength and state fixed effects and Conley standard errors with a 30 km cut-off. The statements are: "Secret dark powers are at work", "Politicians are just puppets", "Media and politics are in cahoots", "I trust instincts more than experts", "The media manipulate information". P-value significance: * 0.10 ** 0.05 *** 0.01.

Other values and attitudes

Table 1.25: Importance in life

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Wealth	Creativity	Rules	Support Weak	Security	Politics	Fun	Work	Emotions	Altruism	Egoism
No TV	0.118	-0.0246	0.0890	-0.0591	-0.0161	-0.0915	0.0463	0.0250	-0.0558	-0.0512	-0.0656
	(0.138)	(0.0875)	(0.117)	(0.0952)	(0.114)	(0.0944)	(0.104)	(0.0975)	(0.125)	(0.0694)	(0.106)
N	3800	3790	3799	3792	3793	3784	3792	3793	3781	3798	3790
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
mean(y)	5.353	5.093	5.834	4.490	6.115	2.882	5.500	5.735	4.914	5.718	4.704
mean(x)	.124	.124	.124	.124	.124	.125	.124	.124	.125	.124	.124

Notes: Data from the 1993 SOEP Covid wave in East Germany. Each column represents a spatial OLS regression on a 1-7 scale for the importance of the category for the respondent. Controls: gender, age, income, education, employment, family situation. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.26: Knowledge about politics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	SPD left of CDU	Merkel is CDU	CDU larger than SPD	know voting age	follow politics	know current debates	MCA of (1)-(6)
No West TV	-0.0129	-0.193*	-0.178*	0.0444	0.289	0.0459	0.136
	(0.0895)	(0.106)	(0.0967)	(0.0400)	(0.178)	(0.154)	(0.250)
Observations	477	484	483	349	482	478	477
Controls	Y	Y	Y	Y	Y	Y	Y
Dep. Mean	0.132	0.600	0.465	0.927	0.856	0.345	0.379

Notes: Data from the 2018 SOEP Covid wave in East Germany. Each column represents a spatial OLS regression on a dummy if the respondent knew the correct answer or agree with the statement. Controls: gender, age, income, education, employment, family situation. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.27: Worried about immigration

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	first	pooled
tv	0.00364	0.00820	0.0193	0.0107	-0.00614	-0.0110	0.00570	0.0156	-0.0211	-0.0169	-0.0266	0.00385	0.0170	0.0279	0.0117	0.0180	0.00959	0.0341***	0.0248	0.0419	0.0157	0.0231	0.00711
	(0.0332)	(0.0360)	(0.0489)	(0.0311)	(0.0329)	(0.0254)	(0.0208)	(0.0284)	(0.0420)	(0.0299)	(0.0460)	(0.0446)	(0.0427)	(0.0399)	(0.0289)	(0.0263)	(0.0221)	(0.00549)	(0.0177)	(.)	(0.0215)	(0.0234)	(0.0178)
N	3767	5816	5420	5410	5343	5221	5028	5262	4994	4705	4435	5286	4695	4657	4275	5212	4831	4594	5009	4835	4859	12412	103654
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
mean(y)	0.151	0.205	0.242	0.210	0.239	0.208	0.167	0.202	0.241	0.263	0.297	0.331	0.284	0.347	0.314	0.277	0.204	0.116	0.139	0.181	0.203	0.246	0.223
mean(x)	.124	.114	.117	.112	.11	.115	.112	.112	.11	.113	.11	.121	.11	.115	.117	.113	.117	.116	.109	.113	.118	.12	.114

Notes: Data from the 2018 SOEP Covid wave in East Germany. Each column represents a spatial OLS regression on a dummy if the respondent is very or somewhat worried about immigration. "First" uses only the first response of an individual in the panel upon entry. Controls: gender, age, income, education, employment, family situation.

Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.28: Worried about Law and Order

	(1)	(2)	(3)	(4)
	1996	2006	2016	all
No West TV	-0.0671 (0.0481)	-0.0317 (0.0781)	0.0609 (0.0699)	-0.0561 (0.0508)
<i>N</i>	3665	5300	4617	13393
Controls	Y	Y	Y	Y
Dev. var. mean	1.514	2.065	0.945	1.787
Indep. var. mean	.125	.111	.117	.118

Notes: Data from the 2018 SOEP Covid wave in East Germany. Each column represents a spatial OLS regression on a dummy if the respondent is very or somewhat worried about law and order. Controls: gender, age, income, education, employment, family situation. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.29: Self-placement on a 0 (left) to 10 (right) scale

	(1)	(2)	(3)	(4)
	2005	2009	2014	Pooled
No West TV	0.323** (0.145)	-0.000704 (0.176)	-0.0213 (0.147)	0.140 (0.0905)
<i>N</i>	5063	4455	5235	19160
Controls	Y	Y	Y	Y
Dev. var. mean	4.316	4.253	4.235	4.461
Indep. var. mean	.112	.11	.113	.113

Notes: Data from the 2018 SOEP Covid wave in East Germany. Each column represents a spatial OLS regression on a dummy if the respondent is very or somewhat worried about law and order. Controls: gender, age, income, education, employment, family situation. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

Table 1.30: Satisfaction on a 0-10 scale

	(1)	(2)	(3)	(4)	(5)
	Life	Life	Living	GDR	GDR
	now	5 years ago	standard	democracy	society
No West TV	-0.1989*** (0.091)	-0.3125*** (0.114)	-0.2823*** (0.095)	0.1202*** (0.038)	0.0873** (0.042)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,370	4,283	4,366	4,436	4,300

Notes: Data from the 1990 SOEP wave. Each column represents a spatial OLS regression on satisfaction with the indicated category. Controls: gender, age, income, education, employment, family situation. Conley standard errors with a 30 km cut-off. P-value significance: * 0.10 ** 0.05 *** 0.01.

2

Hosting Media Bias: Evidence from the Universe of French Broadcasts, 2002-2020

This chapter is based on a paper co-authored with Julia Cagé (Sciences Po), Nicolas Hervé (INA) and Camille Urvoy (University of Mannheim).

Abstract

Democracies need informed voters – voters who are exposed to a diverse range of views. News media take an active role in the process of informing voters; yet, they vary in their coverage of political parties. In this paper, we explore whether differences in political coverage are mainly driven by the editorial choices of (a few) owners, or by the preferences of diverse journalists, provided that they have some agency. To do so, we build a novel dataset on millions of French television and radio shows over 20 years, with information on the identity of hosts, guests, and guests' political leaning. We estimate a two-way fixed effects model identified thanks to the many hosts working on multiple channels. We show that hosts largely comply with outlet-level decisions, which account for 85% of cross-channel differences in political representation. Complementing these results, we study how hosts adapted to a major ownership-driven change in editorial line, and find that the hosts who stayed after the takeover complied with the new owner's preferences.

Keywords: Media bias; Slant; Journalists; Pluralism; Media ownership; Media capture

JEL No: L15, L82, J40

1 Introduction

For democracies to function, voters need to be exposed to a plurality of views (Pariser, 2011). For this reason, regulators in many countries have sought to preserve pluralism in news media. With the idea that media ownership may influence editorial lines, they have

promoted ownership diffusion across competing outlets (external pluralism).¹ They have also created rules requiring that each outlet features a balanced representation of political forces, thereby setting bounds to channel editorial policies (internal pluralism).² While today people can access a virtually infinite number of opinions, reach and attention patterns are such that they are actually exposed to a reduce set of news sources, themselves controlled by a small number of conglomerates (Prat, 2018; Kennedy and Prat, 2019). It has raised concerns that some media tycoons may disproportionately influence the political process, and renewed discussions on media concentration and polarization.³

Contrasting with the small number of owners, there are many journalists and hosts in charge of the daily production of media content. Their diversity – in terms of specialization, views or backgrounds – is a potential source of pluralism, provided that they have some agency vis-à-vis their employers’ editorial policies. In today’s world, engaging directly with their audience online may for example give them leverage and independence,⁴ while employment insecurity may be a disciplining force, pushing them to conform to the editorial policy of their outlet. Furthermore, journalists may chose their employers based on political affinity, which may amplify each outlet’s tendency to prioritize certain views.

In this paper, we study how much agency hosts have regarding opinion representation in their shows. We examine an important choice they have to make on a recurrent basis: who to invite. To do so, we use novel show-level data on French broadcast between 2002 and 2020 and track hosts as they work for distinct outlets over time. We estimate to what extent differences in representation of political views across channels are driven by host-level

¹In the Unites States, the Federal Communications Commission (FCC), designed regulations in line with its mission to ensure “*the diversity of viewpoints from antagonistic forces.*” The US Supreme Court has supported the “*assumption that diversity of ownership would enhance the possibility of diversity of viewpoints*” (Fisch, 2010). The European Commission writes that: “*independent media, and in particular news media, provide access to a plurality of views and are reliable sources of information to citizens and businesses alike. They contribute to shaping public opinion and [...] are essential for the functioning of our democratic societies and economies.*” In case of mergers or acquisitions, the Commission recommends to assess “*the impact of the concentration on media pluralism, including its effects on the formation of public opinion*” (COM/2022/457).

²In the US, the 1949 FCC fairness doctrine required that media with a broadcast license give the public “*a reasonable opportunity to hear different opposing positions on the public issues of importance and interest in the community*” (Fisch, 2010). In France, the Regulatory Authority for Audiovisual and Digital Communication (ARCOM) monitors the equity and diversity of political expression on broadcast media. Most European country have some kind of internal pluralism rules (see “*Internal Media Plurality in Audiovisual Media Services in the EU: Rules and Practices,*” ERGA Report, 2018).

³The literature provides evidence that media content can be impacted by ownership (Durante and Knight, 2012b; Martin and McCrain, 2019; Mastrorocco and Ornaghi, 2020, for instance), and that media content impacts voters’ behaviors (DellaVigna and Kaplan, 2007; Bursztyn et al., 2020; Moreno-Medina et al., 2022, among others).

⁴Respectively, 21% of US and 29% of French respondents report paying more attention to the journalist than to the news brand when consuming news online (Reuters Institute, Digital News Report, 2022).

decisions on the one hand, and hosts adapting to the channel they work for on the other hand. We complement this quantification exercise with a case study. We track how hosts reacted to a major owner-induced change in editorial line around the 2015 takeover of three television channels by the Vivendi conglomerate, owned by the so-called “French Murdoch,” Vincent Bolloré.

As in many countries, media power in France is concentrated in a relatively small number of news outlets, with television and radio being at the center stage of the news ecosystem (Kennedy and Prat, 2019).⁵ Outlets topping the lists of main news source among French respondents are television channels, ahead of social media like Facebook (4%). In 2019, 71% and 53% of the respondents reported that they got their daily news from television and radio respectively, compared to only 47% online (Sumida et al., 2019). Our data includes all the major news sources: it comprises all the most consumed television and radio outlets from 2002 to 2020, with detailed show-level information compiled and enriched by the National Audiovisual Institute (INA) archives. The 2.1 million shows in our data are not restricted to newscasts, but also include talk and entertainment shows.⁶ They feature 39,322 distinct hosts and more than 260,413 distinct guests. With the ample time frame covered, we can track hosts as they move from an outlet to the other and observe how they adapt to their new work environment upon move. Data granularity ensures we can finely control for viewership composition and news events at the time each show airs.

We first document that political forces are unevenly represented across channels.⁷ For instance, on average during our time period, left-wing parties account for 40% of the speaking time on LCI, but 60% on France 4. To show this, we classify guests by political leaning in six groups (radical left, green, left, liberal, right, radical right). We use lists of candidates running in elections and lists of government appointees to identify politicians. Given the increasing coverage they receive in talk shows, we also classify guests who are not politicians in a strict sense, but are politically vocal (activists, think tank commentators, public intellectuals, etc.).⁸ To do so, we rely on think tank contribution or affiliation, endorsements,

⁵See also Newman et al. (2022).

⁶We include all the shows with at least one host and one guest. We do not include fictions and sport games.

⁷The diversity in coverage is clearly visible despite the regulatory agency’s guidelines requiring channels to represent political forces ‘equitably,’ which here means in proportion to their contribution to the political debate (see Section 2 for more details on the institutional background). The differences that prevail nonetheless partly reflect the ambiguity and weak enforcement of this rule.

⁸We call “public intellectuals” here all the intellectuals that are publicly “engaged”, in the sense of the French expression “*intellectuels engagés*”. As will appear clearly from our empirical results, in recent years, media owners have increasingly substituted talk shows to news programs, both to reduce costs (Cagé, 2015), but also as a way to escape broadcast regulation on pluralism.

and party-event participation. Overall, we classify 16,380 distinct individuals, accounting for 661,295 appearances (of course, we allow the political leaning of the guests to vary over time). From there, we can compute the screen time share of each political group at the show- or channel-level.

What explains the differences in political coverage across channels? One explanation is that channels have distinct editorial policies, to which hosts comply by adapting their invitations to the channel they work for (*contextual* factors).⁹ Another is that channels employ distinct hosts on average, who invite distinct guests, potentially due to the hosts' preferences or specialization (*individual* factors). We estimate the relative role of contextual and individual factors in a two-way fixed effects model that allows channel effects to vary over time (Lachowska et al., 2022). We regress the political time share of a given host at the show level on host fixed effects, channel-times-period fixed effects¹⁰, and media platform (radio or television), date, and hour fixed effects. Time fixed effects capture news shocks, potentially making one party more news-worthy than the others at a given moment of time (e.g. because there is a change in the leadership of the party), as well as viewership by controlling for the characteristics of potential viewers or listeners for each hour of each day, by media type. Among the 14,492 hosts in our data who invited politically-classified guests, 9,810 are observed working on at least two of the 20 channels in our sample. Changes in who they invite as they move from one channel to the other reveals to what extent they adapt the content of their shows to their employer. In other words, if hosts moving from channel C to channel C' systematically invite more left-wing guests upon move, everything else equal, we interpret it as a sign that channel C' prioritizes left-wing guests with respect to C. We can also estimate the extent to which hosts have agency with respect to their outlet's editorial policy. If hosts keep inviting an above average share of right- or left-wing guests as they move from one outlet to the other, it implies that they also partly contribute to slanting shows, potentially based on their own preferences or specialization. We investigate whether the agency hosts have varies with their observable characteristics.

We show that hosts largely adapt who they invite based on which channel they work for. According to our estimations, when moving to a channel that grants 1 extra percentage point of screen time to a political group than their origin channel, they increase their coverage

⁹Distinct editorial policies can be driven either or both by supply-driven or demand-driven factors. We come back to this point below.

¹⁰Here, each period corresponds to two 'seasons', where seasons are one-year periods from September to August, so as to match the time frame media outlets use to plan their shows or to adjust their programs. In the spirit of the rolling AKM approach (R-AKM) proposed by Lachowska et al. (2022) – and to allow for time-varying channel effects – we indeed estimate the model separately for successive two-season time intervals.

of this group by 0.63 percentage points on average. We decompose differences in political representation across channel-period pairs using our two-way fixed effects model. Based on the linear decomposition, channel-level decisions are crucial and explain 87% (respectively 90%) of the differences in left-wing (respectively right-wing) parties time share. Host characteristics account for the remaining 13% (10%). A variance-decomposition exercise leads to similar conclusions – channels account for around 82% (85%) of the difference for the left (right) – while highlighting host sorting: covariance between channel and host effects account for 16% (13%) of the variance. Host effects only explain the remaining 2.2% (2.1%). Hosts therefore largely comply to channel-level editorial policies. This finding sheds new light on the mechanisms through which media slant happens, by quantifying the relative role played by owners and hosts.

Analyzing trends over time, we find that the dispersion of channel effects increased over the sample period, which can be seen as reflecting polarization in editorial policies. One reason for this may be that profit-maximizing owners specialize each channel ideologically; another is that owners want their channels to prioritize certain views ([Gentzkow and Shapiro, 2010](#)). We find that, within owner, channels often tend to prioritize the same political forces, suggesting that the latter explanation might be at play.

We then explore the factor that predicts hosts over- or under-representing certain political groups. Female hosts and hosts who are more central to the political guest-host network tend to deviate more to the left relative to their channel, but the effect is small. Interestingly, when looking at absolute deviations from the channel line, we find that hosts tend to deviate more if they are more famous as proxied by their total screen time, the existence of a Wikipedia entry, or the number of times they interview the ruling President.¹¹ At the same time, hosts who work as journalists on channels, who are more central to the political host-guest network and who have more political screen time tend to deviate less in absolute terms from the channel line. This suggests that journalists specialized in politics follow more closely the outlet’s editorial line, while more famous hosts are allowed to deviate more from it.

In the second part of the paper, we focus on a large owner-induced change in editorial policy, and study two hosts’ response margins: complying or leaving. In 2015, Vincent Bolloré – a French billionaire often compared to Rupert Murdoch – became the main shareholder of the Vivendi conglomerate, the parent company of the Canal Plus group, which owns several television channels. Journalistic accounts of the event have highlighted the proximity of

¹¹In France, the President tends to grant very few interviews, contrary to the US for example where the President holds regular press conferences. Interviewing the President can thus be seen as a form of “consecration” for a French journalist.

Vincent Bolloré with conservative figures, and noted shows swiftly moved rightwards (see also [Capozzi, 2016](#); [Cagé, 2022](#)). We compare Vivendi channels to others in our sample before and after the takeover. Our event-study specification includes host-channel fixed effects, meaning that we exploit within host-channel variations. After the takeover, we show that right-wing parties' screen time share increased by 5.5 percentage points, and that the one of left-wing parties decreased by 6.8 percentage points. We find no evidence of diverging pre-trends. Hosts who remained on the acquired channels adapted the content of their show to the new editorial policy implemented after the takeover.

We further analyse whether hosts left the channel in response to the change in editorial policy. We find that the probability that a host stays decreases by 15 percentage points following the takeover, from a 38% baseline. The effect is driven by hosts who invite political guests, who have above median political screen time, who are credited as 'journalists' and whose shows are newscasts. It suggests that hosts who were the most exposed to the change in editorial policy were precisely the ones most likely to leave. Male hosts, famous hosts, and hosts with higher ratings are more likely to stay in the medium run. Regarding hosts who leave, a majority of them is no longer observed on one of the channels in our sample following the takeover, suggesting their career has been negatively impacted.¹² Those who work on another channel are more likely to work on a channel that represents the right relatively less, hinting at potential sorting on editorial policy.

Literature Our work sheds light on the inner workings of media outlets. A burgeoning literature studies how journalists' work is impacted by new technologies ([Cagé et al., 2020a](#); [Hatte et al., 2020](#)) or by the resources of their outlets ([Djourelouva et al., 2021](#)). Some empirical papers have focused on reporting bias at the journalist level, but essentially from a theoretical perspective [Dyck and Zingales \(2003\)](#); [Baron \(2006\)](#). Our paper contributes to this literature by studying host invitation decisions, and the extent to which these decisions are determined by the outlets hosts work for.

Our paper also contributes to the ongoing discussion on media ownership, media concentration and news reporting. [Gentzkow and Shapiro \(2010\)](#), studying local newspapers, asks whether differences in political reporting across outlets is explained by owners responding to local readers' demand, or rather by owners' ideological views. They find support for the former. Since, several papers have documented that changes in media control can impact media content, in the context of private television networks acquisition ([Martin and McCrain, 2019](#);

¹²This is consistent with existing anecdotal evidence documenting that a large share of the former journalists working for the news channel acquired by Bolloré have quit journalism (which is unfortunately not a surprise in a context where the overall number of journalists in France is declining).

Miho, 2020; Mastrococco and Ornaghi, 2020), or public broadcasters’ control (Durante and Knight, 2012b); and a large body of work shows that media content impacts attitudes and behaviors down the line (DellaVigna and Kaplan, 2007; Chiang and Knight, 2011; Martin and Yurukoglu, 2017; Knight and Tribin, 2019; Bursztyn et al., 2020; Djourelouva, 2022; Simonov and Rao, 2020, among others).¹³ This paper helps understand the potential consequences of media ownership change by studying the response from hosts. We document that journalists are largely constrained by their environment. Studying a takeover-induced change in editorial line, we find that hosts either comply or leave, the latter potentially disrupting their careers. Our paper also adds to works in other disciplines on political representation on Vivendi channels (Sécail, 2022).

Finally, our empirical strategy draws on recent work on two-way fixed effects models meant to tease out effects of individual characteristics from context effects using moves across geographic areas, institutional environments or organizations. They have been used to explain a variety of outcomes, which include wage earnings (Abowd et al., 1999; Card et al., 2013a; Lachowska et al., 2022), health care consumption (Finkelstein et al., 2016), political participation (Cantoni and Pons, 2022), bureaucrats’ productivity (Best et al., 2017; Fenizia, 2022), or teachers’ performance (Chetty et al., 2014). Our paper is the first to use this type of model to study the relative role of hosts and their environment in media content creation.

The rest of the paper is organized as follows. Section 2 below provides details on the institutional setting, and Section 3 on the data. Section 4 presents the decomposition of across-channel differences in political representation and show that channel-level decisions account for the largest share of differences across outlets . Section 5 focuses on hosts reaction to Vincent Bolloré’s takeover. Finally, Section 6 discusses the policy implications of our results and concludes.

2 Institutional background

News sources Television and radio are the main sources of news in France. In 2017, 71% of French adults reported getting their news at least daily from television, 53% from

¹³Our work builds on the large literature measuring media bias. Some articles have relied on endorsements (Ansolabehere et al., 2003; Chiang and Knight, 2011), think tank quotes (Groseclose and Milyo, 2005), language (Gentzkow and Shapiro, 2010), issue coverage (Puglisi and Snyder, 2011; Galvis et al., 2013). Our work is closest to Durante and Knight (2012a) and Knight and Tribin (2019) as we also use time shares to measure political representation on screen. Yet, we build this measure for a broader range of shows, including entertainment, at the show-level, and for a broader variety of guests. Beyond professional politicians, we also include other politically vocal guests, taking into account the literature on “celebrity politics” (West and Orman, 2003; Wood and Herbst, 2007; Wheeler, 2013).

radio, 47% online, and 23% from print. When asked about their main news source, 16% answer TF1 (private television), 15% BFM TV (private television), 15% France TV (public television), 6% *Le Monde* (newspaper), 6% Radio France (public radio), and 4% Facebook (Sumida et al., 2019). The list is largely dominated by television networks, social media being far behind. 25% of the surveyed individuals get their news daily from only one type of source, with television also being the most common source among those individuals. In 2022, the three most mentioned journalists are three presenters (either on television and/or on radio): Pascal Praud (CNews and RTL), Anne-Claire Coudray (TF1), and Jean-Jacques Bourdin (BFMTV and RMC) (Newman et al., 2022).

Channels Appendix Table 2.6 lists the main 30 national television channels in France (excluding cable and satellite channels) with the corresponding audience share over the period studied. The most watched television channels in 2020 (at the end of our sample) are TF1 (private), France 2 (public), France 3 (public), M6 (private), and France 5 (public), and are all included in our dataset. Appendix Table 2.7 lists the main radio stations, excluding music-only stations and local stations. Those with the largest audience are France Inter (public) and RTL (private). Appendix Section A.2 provides additional details on each channel.

Ownership Public broadcast in France counts several channels among the most influential ones (France 2 and France Inter, among others). Public broadcasters fall under the umbrella of France Télévision for television channels and Radio France for radio stations. ARTE is jointly run by the French and German public broadcasts, and LCP is overseen by the French parliament. Regarding private outlets, four major groups dominate the market: the TF1 Group (property of Bouygues), the Bertelsmann group, NextRadioTV (property of Altice) and the Canal Plus group (Vivendi).¹⁴ These groups often own media outlets of different types. E.g. NextRadioTV owns television channels, radio stations, and some magazines; Vivendi, the parent company of Canal Plus, also owns the publishers Editis and Prisma media. Bertelsmann owns both radio stations and television channels.

¹⁴The TF1 Group belongs to Bouygues and encompasses several television channels, including TF1 (general), TMC (general), TFX (entertainment), and LCI (news). The Bertelsmann conglomerate owns several television channels – M6 (generalist), W9, 6ter (entertainment), Gulli (youth) – and the radio station RTL. NextRadioTV (owned by Patrick Drahi’s Altice) owns several television channels including BFM TV (news), and several radio stations among which RMC. The Canal Plus group (property of Vincent Bolloré’s Vivendi) owns several channels, including Canal+ (general), C8 (general) and CNews (news). Appendix Section A.2 provides more details on each of these channels, their ownership structure and ownership changes during our period of interest.

Broadcast regulation and pluralism The 1986 Law on Freedom of Communication¹⁵ laid the foundation of broadcast regulation in France. Its first article explicitly mentions the constitutional principle of “the pluralist nature of the expression of currents of thought and opinion” as one of its objectives. To this end, it has set rules limiting ownership concentration, with the idea that diffused ownership helps preserve media independence and diversity of editorial content – a reasoning similar to that developed in the 1947 Hutchins Commission report in the US. These rules are specific to the broadcast sector and apply on top of anti-concentration rules. They consider each platform separately (television, radio, etc.). For instance, according to the law, a given group cannot own more than 7 national television channels (excluding cable and satellite); a natural person cannot own more than 49% of a national television channel whose mean viewership exceeds 8%.; etc.

The 1986 Law is also at the origin of the creation of an independent regulatory agency, which is known today as the *Autorité de régulation de la communication audiovisuelle et numérique* (Arcom).¹⁶ The Arcom is the French equivalent of the Federal Communications Commission (FCC) in the United States. One of its missions is to “*ensure respect for the pluralist expression of currents of thought and opinion in the programs of radio and television services, in particular for political and general information programs*” (article 3). In practice, the Arcom requires that a third of the speaking time be dedicated to the president and the government. The remaining two thirds should be dedicated to all political parties (including the government party), in proportion to the electoral results, the number of elected officials, popularity in the polls and a party’s contribution to the public debate.¹⁷ Public debate contribution and popularity not being unambiguously measurable, it is a general principle, left to the discretion of the media outlets, not a working rule. We indeed document in this article large differences in the speaking time of each party across outlets. Channels have to record the speaking time of each politician¹⁸ and communicate aggregate quarterly figures to the Arcom.¹⁹

¹⁵Loi 86-1067 du 30 septembre 1986.

¹⁶Created in 1989 under the name *Conseil Supérieur de l’Audiovisuel* (CSA), the Arcom is the regulatory agency in charge of delivering frequencies, of overseeing mergers and acquisitions in the media market, of setting rules regarding diversity and pluralism, of labeling whether programs are appropriate for young audiences. It can also impose sanctions in case of hate speech or discrimination. See [Cagé and Huet \(2021\)](#) for more details on the regulatory environment of French broadcast.

¹⁷See the Arcom’s website for additional details: <https://www.csa.fr/web/index.php/Protéger/Garantie-des-droits-et-libertes/Protéger-le-pluralisme-politique>.

¹⁸Only professional politicians are monitored, not commentators, activists, or union leaders. We will come back to this point in the Data section below.

¹⁹Speaking times are added up irrespective of whether the show is broadcast during “prime time” or in the middle of the night. Anecdotal evidence suggests that some channels sometimes broadcast several times during the night interviews of politicians belonging to parties they under-represent (see e.g. <https://www.arretsurlimages.net/articles/quotas-31-fois-yannick-jadot-sur-lci>). In our analysis, when

Stricter equal-time rules apply during presidential and parliamentary electoral campaigns.²⁰ As a robustness check, we drop those periods when equal time rules apply as the time share of each political group is artificially balanced across candidates and does not necessarily reflect the decisions of hosts or of channels.

Political parties The French political landscape counts many parties, ranging from radical left to radical right. For clarity, and because parties split, merge, and change name over time, we aggregate them in ideology-based groups following the Chapel Hill Expert Survey (CHES) classification. The resulting six political groups are: *i.* radical left (communist party, *France insoumise*); *ii.* greens (*Europe Écologie-Les Verts*); *iii.* left (socialist party, “other left”); *iv.* liberals (*MoDem*, *République en Marche*); *v.* right (*les Républicains*, *Union des démocrates et indépendants*, “other right”); and *vi.* radical right (*Rassemblement National*, *Debout La France*).

3 Data and descriptive statistics

In this article, we build a novel dataset on television and radio shows from the *Institut National de l’Audiovisuel* (INA)²¹ archives that we clean and complement using a number of different resources. In this section, we describe the data, explain how we define the sample and outcomes of interest, and present descriptive statistics. In Section 4, we will then study the factors behind the documented differences in relative political representation across channels.

Content and coverage The INA manually documented hosts and guests appearing in television and radio shows starting in 2002, focusing on the main television and radio stations. For each show, INA staff indicated the title of the show, the date, the start time, the end time, the show type, and the list of persons related to the show. For each person, we have her first name and last name, as well as a show time-invariant description of her profession (politician, journalist, singer, actor, etc.), and a show-specific role. Most common roles are ‘host’ and ‘guest’, but there are other labels, such as ‘voice-over’ (common for documentaries), ‘musician’ (if the show has a band for instance), etc. The data also includes information on segments within longer shows. That is typically the case for newscasts, where the main show credits the main host, and each sub-show references the reporter who went on

studying speaking time shares, we take into account the average audience of the different time slots.

²⁰See online Appendix Section A.2 for a precise description.

²¹The INA collects and archives television and radio shows. Show data can be accessed via the following interface: <http://inatheque.ina.fr/>. For previous research using the INA data, see Cagé et al. (2020b,a).

the grounds as host of the sub-show, and the persons she interviewed as guests. Sub-shows therefore help measuring how much time is dedicated to each host or guest.

Regarding coverage, the INA collected data on a large variety of shows with hosts and guests: not only newscasts, but also talk shows, infotainment shows (in the style of late shows), investigation shows, etc. The shows that are not included are fiction, sports, games, and documentaries that feature no guests. In Appendix Section A.1, we compare the time length of the television shows in the INA data to shows documented in another dataset.²² We document that newscasts, shows about news and politics, and talk shows are nearly all included in INA data. The coverage is lower in the entertainment shows (including games), sports shows, youth programs, and documentaries categories. It is expected, since many of those shows do not feature guests. Overall, with INA data, we can reliably analyze the content of a broad range of shows, while most previous works only focused on a specific type of shows.²³

Sample definition Our dataset covers French television and radio shows between 2002 and 2020. However, in our preferred empirical specification, we are going to focus on the sub-period September 1st 2005 - August 31st 2019, including 14 seasons (which are one-year periods from September to August).²⁴ In 2005, the French TV system transitioned from analog to digital, and new country-wide channels became available for free. The sample ends in 2019 since, after that date, the number of documented shows sharply decreases due to budget cuts at the INA; less staff is since then in charge of show referencing and data on guests are entirely missing for certain channels past that date. As a result, we have a balanced sample of channels ranging from September 2005 to August 2019.

There are 14 television networks and six radio channels in our sample. For television, we focus on country-wide digital television networks (not cable, not satellite) that have shows with

²²To benchmark the INA data coverage, we use information from Plurimedia, a company that collects scheduled television shows before they are broadcast.

²³Most papers in the existing literature focus on news casts (see [Durante and Knight, 2012b](#); [Gambaro et al., 2021](#), for instance). Some have also specifically focused on entertainment shows (see e.g. [Jensen and Oster, 2009](#); [La Ferrara et al., 2012](#); [DellaVigna and Ferrara, 2015](#)). To the extent of our knowledge, our article is the first to take into account all the different kinds of shows consumed by citizens on both television and radio, which seems of particular importance given the consumption of content that might influence political knowledge and behavior is not limited to the news broadcasts.

²⁴Seasons match the time frame outlets use to plan their shows or to adjust their programs. They typically hire new hosts between seasons, around the summer. With the data at our disposal, we could have included the programs broadcast during the summer. However, we have decided not to do so for several reasons. First, there are many more reruns of shows during the summer than during the rest of the year. While channels decide which shows to rerun, this is not up to the hosts' decision. Second, most of the programs do not run during the summer and tend to be replaced by shows with both less hosts and guests.

hosts and guests each season (see Appendix Section A.1 for more details on the sample), i.e. the following channels: ARTE, BFM TV, C8, Canal+, CNews, France 2, France 3, France 4, France 5, LCI, LCP/PublicSénat, M6, TF1 and TMC. The included channels accounted for 71.6% of viewership in 2020 (85.2% in 2007). The six radio stations included in the sample are France Culture, France Info, France Inter, Europe 1, RMC, and RTL. While the audience share of country-wide non-music radio station was 54.9% in 2020, the stations in our sample accounted for 46.3%. As a result, television and radio networks in our sample account for a large share of audience on both platforms, and for nearly all shows with hosts and guests broadcast on country-wide channels.

Guests The 260,413 unique guests in our sample account for 2.3 million appearances. The INA considers that a guest appears in a show if she speaks during the show, whether or not she is in the studio.²⁵ The top five guests in number of appearances are François Hollande (14,278 appearances, politician, left), Nicolas Sarkozy (13,169 appearances, politician, right), Manuel Valls (7,860 appearances, politician, left), François Fillon (6,279 appearances, politician, right) and Marine Le Pen (5,592 appearances, politician, radical right). They account for 2.0% of all appearances, and 7.1% of politically-classified appearances.

The data include each guest’s gender, birth year, country, and a time-invariant description of the profession. Using keywords, we create indicator variable for whether each guest falls into a given category (one guest can fit several categories). The keywords and categories are precisely described in Appendix Section A.1 and Appendix Figure 2.15 plots the relative frequency of each category. One in four appearances is by a guest who is a politician, meaning that a majority of guests are not involved in politics. Their job can be related to entertainment (actor, comedian, singer, etc.) or sports (player, coach, etc.) for instance.

Political leaning of guests We next map each guest appearance to a political group (if any). This measure of political leaning is allowed to vary over time: a guest might become a politician during our period of study, leave politics or change political affiliation over time. We use two sets of data sources. The first set of sources centers on elections and government appointments. We track for which party a given guest ran and in which elections (house, senate, EU, *région*, *canton*, municipalities), whether when in parliament she was affiliated to a political group, and whether she worked for the government under a given majority. Appendix Section A.1 describes in detail how we combine these different data sources. With this first set of sources, we finely track how the affiliations of guests who are explicitly

²⁵E.g. if a minister gives a press conference and snippets from the conference are broadcast during a newscast, then the minister is listed as guest, even though she is not present in the studio during the show.

involved in politics change over time.

Motivated by the presence of guests who express their political views in shows like talk shows but are not politicians, we use a second set of data sources. Our goal is to find tangible signs of political leaning for guests who do not run in elections or work for the government but might still regularly be in the media. To this end, we collect data from three different sources. The first one is the list of speakers in political parties' summer events (*universités d'été*). These events typically gather politicians and non-politicians like experts, columnists, activists, etc. Second, we collect the names of people who endorsed in the press one of the candidates running in the first round of presidential elections. For the third source, we focus on think tanks and proceed in two steps. We compile a list of French think tanks, and map them to a political group when relevant. Think tanks are linked to a party based (i) on whether founders or top managers were politicians in this party, (ii) on which politicians or political party grants them funds, (iii) on their stated goal, and (iv) on their community on Twitter. For the think tanks that have a political leaning, we use archives and archived versions of their websites to collect the list of members and contributors (report, blog post, etc.). We then combine these data sources and obtain a time-varying measure of the political leaning of guests. Appendix Section A.1 lists all the party summer events along with the number of participants, all the think tanks with their corresponding political leaning, statistics on their Twitter community, and the number of names collected. It also describes in detail how we combine these data sources in a single measure of political leaning.

As a result, we get a time-varying measure of political leaning for each guest. We classify 28.6% of appearances (661,295 in absolute value). Among the 24.1% of appearances by guests whose profession indicates 'politician' and whose country is France, 95.9% are matched to a political leaning. Appearances that are not classified are typically appearances of retired politicians or of not-yet politicians observed when they were not active (e.g. the criminal defense lawyer Eric Dupond-Moretti before he was appointed Minister of Justice). It means that we classify virtually all the guests who are politicians and are therefore expected to be classified. We also classify 7.2% of appearances of people who are not considered to be politician (e.g. Bernard Thibault, a union leader, or Bernard Laporte, a rugby player and rugby coach who became in charge of sports in a government). The remaining 71.4% appearances are by guests who are in the media/publishing sector (36.1%), in the entertainment industry (20%), are experts/academics (14.8%), or whose profession is missing in the data (24.8%). The most common guests who are not politically classified are Barack Obama (4,228 times), Didier Deschamps (football coach, 2,578 times) and Angela Merkel (2,495 times).²⁶

²⁶Regarding Obama and Merkel, note that this is due to the fact that we do not classify the guests who

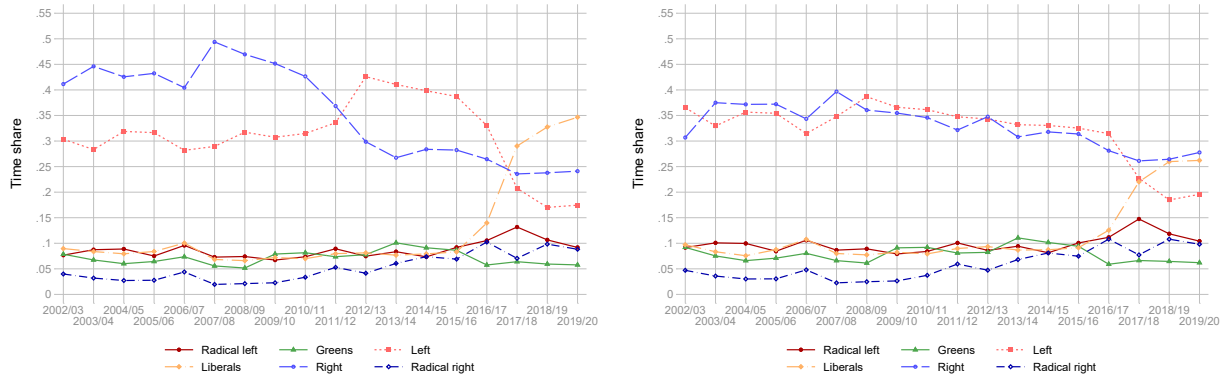
Screen time share To measure the relative amount of time that each outlet dedicates to guests with given political views, we take into account show (or sub-show) length. The idea is to account differently for guests appearing in short segments, and guests giving longer interviews. To this end, we use the length of the show or sub-show²⁷ and divide it by the number of guests. For example, if a one-hour show features two guests, we consider that each guest gets 50% of the speaking time share, i.e. 30 minutes. This measure does not take into account several margins: how long the host speaks, whether the guest is interrupted often, or the tone of the interviewer. To check the validity of our measure, we compare the time share we attribute to each guest in a show (50% for instance) to the share of frames that contain the face of the guest using a subset of television shows for which a face-recognition algorithm has been implemented in the context of a machine learning study by [Petit et al. \(2021\)](#). The right panel of Appendix Figure 2.14 plots the computed time share against image frame share for this subset of shows. We find that our measure explains 87% of the variation of screen time share measured by image frames with a slope coefficient of 1. In other words, our time share measure proxies very precisely the screen time of each guest. Even if our measure does not take into account interruptions, cutaways, or the tone of the host, we still believe that it captures how much time political actors are given to express their views, which is the basic requirement for the public to be exposed to them. In this sense, our measure of political representation is similar to that of [Durante and Knight \(2012a\)](#).

From there, we have a measure of the screen time of each political group for each show, that we aggregate at the season level. Figure 2.1 plots the time share of each political group aggregated across all outlets in our sample. Panel (a) includes all the guests who are politically classified. We can clearly observe the electoral cycles, with the right being in power until 2012, the left from 2012 to 2017, and the liberals gaining power in 2017. The government party is systematically more represented, which echoes the Arcom guideline requiring that a third of the political speaking time be dedicated to the government. Panel (b) excludes government officials. In this case, both the right and the left are similarly represented, until 2017 when the liberal party emerges as winner of the presidential elections and eclipses the left and, to a lesser extent, the right. We also observe a in recent years a significant rise in the speaking-time share of the radical right.

Figure 2.2 juxtaposes the speaking time share of political groups on each outlet, which are sorted by the time share of all left-wing parties combined. We can see that there are substan-

are not French.

²⁷If a guest takes part in a show that contains sub-shows – that could be the case if a guest is invited in a talk show that includes segments like a live performance, a book review, a cooking demonstration, etc. – we net out the length of the sub-shows that do not feature the guest.



(a) All politically-classified appearances

(b) Excluding government officials

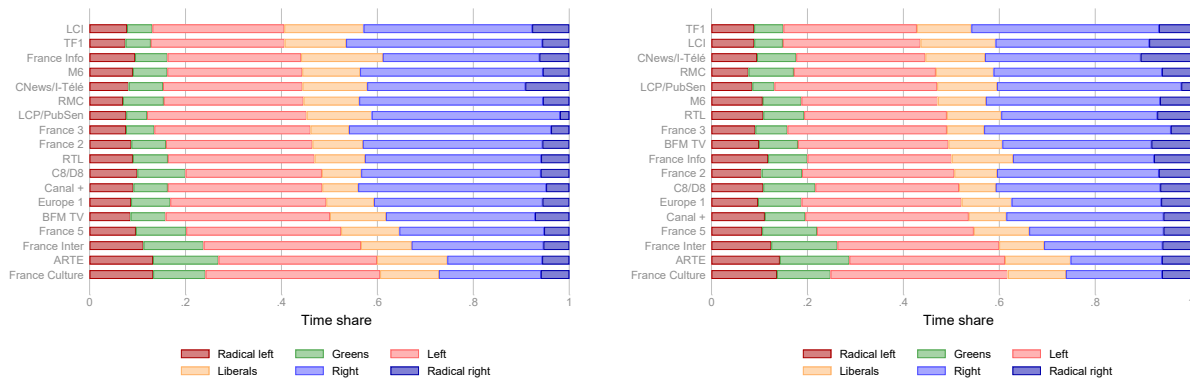
Notes: The figures plot the time share of each political group for each season, aggregating over all the outlets in our sample. Panel (a) includes all the political groups, while Panel (b) excludes the government members.

Figure 2.1: Time share of political groups over time

tial differences across outlets. For example, the 24-hour news channel LCI devotes 40.7% of the time share to left-wing guests, compared to 60.2% for France Culture. Comparing outlets within platforms, there are still substantial differences across networks, even though they all operate on the country-wide television market (and so all potentially serve the same set of consumers). There is a 19.3 percentage-point difference in left-wing parties representation between the TV network representing the left the least and that representing it the most. The figure for radio is 15.8 percentage points. In the rest of the paper, we seek to tease out the relative contribution of host characteristics and of outlet-level decisions, while finely controlling for demand.

Hosts INA data also includes information on show hosts. We have the name and gender of each host. We complement this information by collecting data online from two sources: Wikidata and *Les Biographies* (LB), which is the French equivalent to the *Who's Who*. Appendix Sections A.1 and A.1 provide details on how we compiled data from these sources.

To estimate the relative impact of host- vs. outlet-level decisions on show content, we track hosts as they move from outlet to outlet. Table 2.1 presents descriptive statistics for several sub-samples. Column (1) includes all the hosts included in our sample, and Column (2) only hosts that have at least two shows featuring guests who are politically classified. Column (3) focuses on hosts who have at least two shows with political guests and are observed in distinct outlet-season pairs. Finally, Column (4) features hosts who have at least two shows with political guests and are observed on at least two distinct outlets.

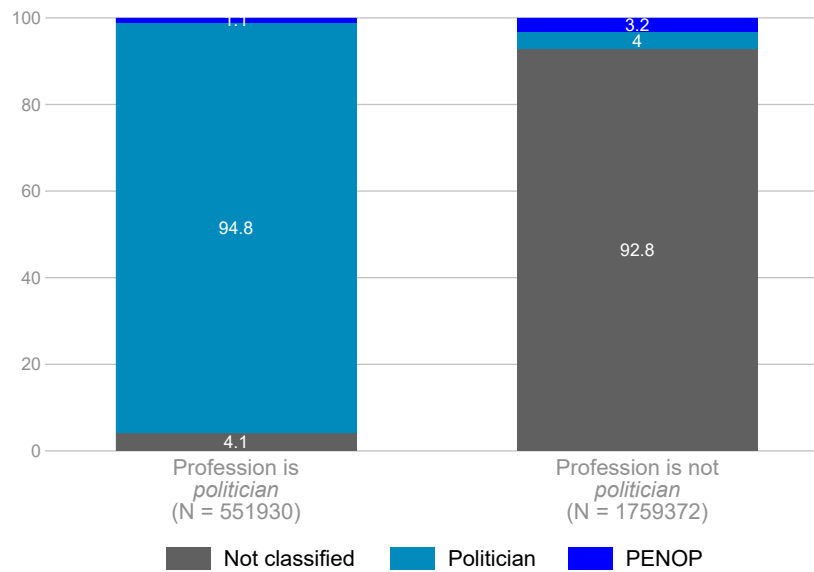


(a) All politically-classified appearances

(b) Excluding government officials

Notes: The figures plot the time share of each political group for each season, depending on the media outlets. Media outlets are ranked depending on the time share they devote to the left-wing parties. Panel (a) includes all political groups, while Panel (b) excludes government members.

Figure 2.2: Time share of political groups across channels



Notes: The figure reports the share of appearances that are politically classified for two subsets of appearances based on whether or not the guests' INA description (which is time invariant) includes 'politician' and 'France' (to exclude foreign politicians). Grey areas account for the share of appearances that are not politically classified. Light blue ones are appearances classified politically based on the first set of sources, the sources use to classify professional politicians (i.e. government position, candidate lists, parliamentary groups). The darker blue share indicates the share of appearances classified politically based on the second set of sources, those meant to classify politically-engaged non-politicians (PENOPs) – i.e. party summer meeting attendants, think tank staff and contributors, endorsements.

Figure 2.3: Output of appearance classification

The dataset includes 39,322 distinct hosts (Column (1)). Among them, more than a third (14,492) have hosted at least two shows featuring guests who are politically classified (either on the same or on different media outlets) and are thus in the estimation sample (Column (2)). Among those, 6,884 are observed on at least two distinct outlets (Column (4)), and 9,810 are observed on an outlet in at least two distinct 2-season time periods (Column (3)). As detailed in Section 4, our model thus estimates 140 channel-time effects with leveraging 6,884 movers (49 per estimate), and 9,810 stayers (70 per estimate).

Hosts in the estimation sample are more likely to have a description in INA data, and that description is more likely to include the word ‘journalist’. Indeed, some hosts exclusively invite guests related to entertainment or sports; journalists by contrast are generally trained to analyze political developments and interview politicians. Hosts in the estimation sample, and *a fortiori* those observed on distinct channels, are more known, as proxied by the existence of a Les Biographies or a Wikidata entry. They have more screen time, are observed on more days, have more guests, dedicate more time to political guests, etc. This is in line with the idea that hosts observed across longer periods of time or across channels are more visible and more advanced in their careers.

Regarding other characteristics such as gender, age, but also the time share dedicated to each political group, we find that hosts are very similar across samples. It means that the hosts whose shows we use to identify channel effects do not systematically differ from others when it comes to which political group they invite in their shows. They are not more right-wing or left-wing than hosts that do not move, or are observed more briefly on a given outlet.

4 What explains the differences in relative political representation across channels?

In this section, we ask to what extent the differences in relative political representation across channels are driven by: (i) the preferences or specialization of hosts working on each channel (host composition), (ii) the editorial guidelines of each channels (host compliance), or (iii) the sorting of hosts across channels which could potentially magnify the other two effects or, conversely, mute them (host sorting).

4.1 Specification

Two-way fixed effects model To decompose the relative influence of each mechanism, we use the following model, in the spirit of [Lachowska et al. \(2022\)](#):

Table 2.1: Descriptive statistics on hosts

	(1)		(2)		(3)		(4)	
	All hosts		Est. sample		Dist. 2y-s		Dist. channels	
	mean	sd	mean	sd	mean	sd	mean	sd
Descriptive characteristics								
% female	36.30	(48.09)	39.17	(48.81)	38.63	(48.69)	37.23	(48.35)
Birth year (pred)	1967.07	(18.47)	1968.64	(17.81)	1968.45	(17.42)	1969.36	(17.48)
% with description	82.31	(38.16)	94.43	(22.93)	96.76	(17.71)	97.09	(16.80)
% 'journalist'	44.29	(49.67)	65.73	(47.46)	71.74	(45.03)	72.92	(44.44)
% 'host'	3.57	(18.54)	5.73	(23.24)	5.98	(23.72)	6.17	(24.07)
% 'producer'	17.32	(37.84)	16.15	(36.80)	15.95	(36.62)	14.80	(35.51)
% w/ LesBios entry	4.65	(21.06)	7.78	(26.78)	8.78	(28.30)	10.95	(31.23)
% w/ Wikidata entry	8.85	(28.40)	12.57	(33.16)	13.62	(34.30)	16.47	(37.10)
Media presence								
# distinct days	46.13	(174.60)	120.12	(272.01)	168.82	(318.56)	166.28	(325.77)
# distinct channels	1.60	(1.17)	2.31	(1.57)	2.48	(1.69)	3.38	(1.58)
# dist. chan x 2y s	3.97	(5.06)	7.94	(6.41)	10.10	(6.55)	10.91	(7.26)
% has any pol. guest	59.49	(49.09)	100.00	(0.00)	100.00	(0.00)	100.00	(0.00)
# guests	158.35	(713.52)	415.20	(1129.69)	583.35	(1337.63)	608.12	(1432.75)
Screen time (hours)	37.47	(197.45)	98.22	(316.07)	137.88	(377.07)	144.54	(409.60)
Time per guest (min)	17.06	(21.76)	13.33	(13.63)	12.70	(12.67)	13.04	(11.83)
Political guests								
# politic. guests	39.67	(285.15)	106.50	(462.13)	151.68	(554.85)	165.53	(562.12)
Time w/ pol. guest (hrs)	8.30	(64.78)	22.28	(105.25)	31.68	(126.68)	36.22	(138.99)
Time per pol.guest (min)	14.10	(18.44)	13.48	(15.19)	12.79	(14.10)	13.26	(13.26)
% time w/ pol. guest	17.81	(25.98)	26.47	(23.14)	25.58	(21.42)	27.28	(22.03)
% time rad. left	9.73	(20.53)	9.35	(14.14)	9.20	(12.20)	9.17	(12.03)
% time greens	8.53	(19.40)	8.63	(14.40)	8.31	(12.19)	8.23	(12.38)
% time left	29.60	(30.57)	30.10	(22.23)	30.94	(19.15)	30.75	(19.78)
% time liberals	9.58	(20.04)	9.86	(15.01)	9.49	(12.48)	9.93	(13.14)
% time right	32.14	(31.91)	32.03	(23.25)	32.09	(19.95)	32.08	(20.36)
% time rad. right	5.84	(16.01)	5.86	(11.54)	6.09	(10.09)	5.99	(10.24)
Observations	39322		14492		9810		6884	

Notes: The Table provides descriptive statistics on the hosts. An observation is a host. Column (1) includes all the hosts included in our sample ("all hosts"). Column (2) only includes hosts who are in the estimation sample, meaning those who have at least two shows featuring guests who are politically classified ("est. sample"). Column (3) focuses on hosts, among those in the estimation sample, who are observed on the same outlet in at least two distinct 2-season periods ("Dist. 2y-s"). Finally, Column (4) features hosts in the estimation sample who are observed on at least two distinct outlets ("Dist. channels"). "% description" reports the share of the hosts for which the INA data provides a short description. More details are provided in the text.

$$y_{ict} = \alpha_i + \gamma_{c(t)} + \tau_t + \epsilon_{it} \quad (2.1)$$

where c indexes the channels, i the hosts and t the time. y_{it} is the time share of a given political group in shows hosted by host i at time t . In our preferred specification, we define this share by using as the numerator the time dedicated to guests of a given political group, and as the denominator the total time dedicated to political guests.²⁸ This share varies between 0, if that political group was not represented at all, and 1, if all political guests in the show were from that political group. The unit of observation is the triple of host i , channel c and time t , where time is measured at the date \times hour level.

τ_t is a time fixed effect at the date \times hour \times platform level, where platform is either television or radio. It controls for time shocks such as news events as well as for viewers' characteristics in each hour of each day. These time fixed effects therefore control for demand characteristics non-parametrically at very high frequency. α_i is a host fixed effect. It accounts for the hosts fixed characteristics, including his preferences or specialization, that could make him susceptible of over- or under-representing a given political group. $\gamma_{c(t)}$ is a channel fixed effect that accounts for how a host changes his invitation pattern based on which channel he works on. In other words, it reflects the editorial guidelines promoted on this particular channel. Channel effects are allowed to change every two seasons, in the spirit of time-varying AKM models (Lachowska et al., 2022). It follows from the idea that assuming that channels' editorial lines are fixed over long periods of time is likely unrealistic (to begin with because there might be changes in channel ownership). Rather, our model allows channel effects to vary, reflecting that their editorial line might be periodically adjusted. This flexibility also implies that channel effects are identified both with movers, switching from one media outlet to the others, and by stayers who are observed in distinct time brackets. Together with the large number of hosts observed on distinct channels, the fact that stayers also contribute to the identification of channel fixed effects ensures that they are estimated with a sufficient number of observations.

Finally, ϵ_{it} represents an unobserved time-varying error that captures random match effects and other unobserved factors.

²⁸We show below that our main findings are unchanged if we rather use as the denominator the total time dedicated to guests (whether or not political).

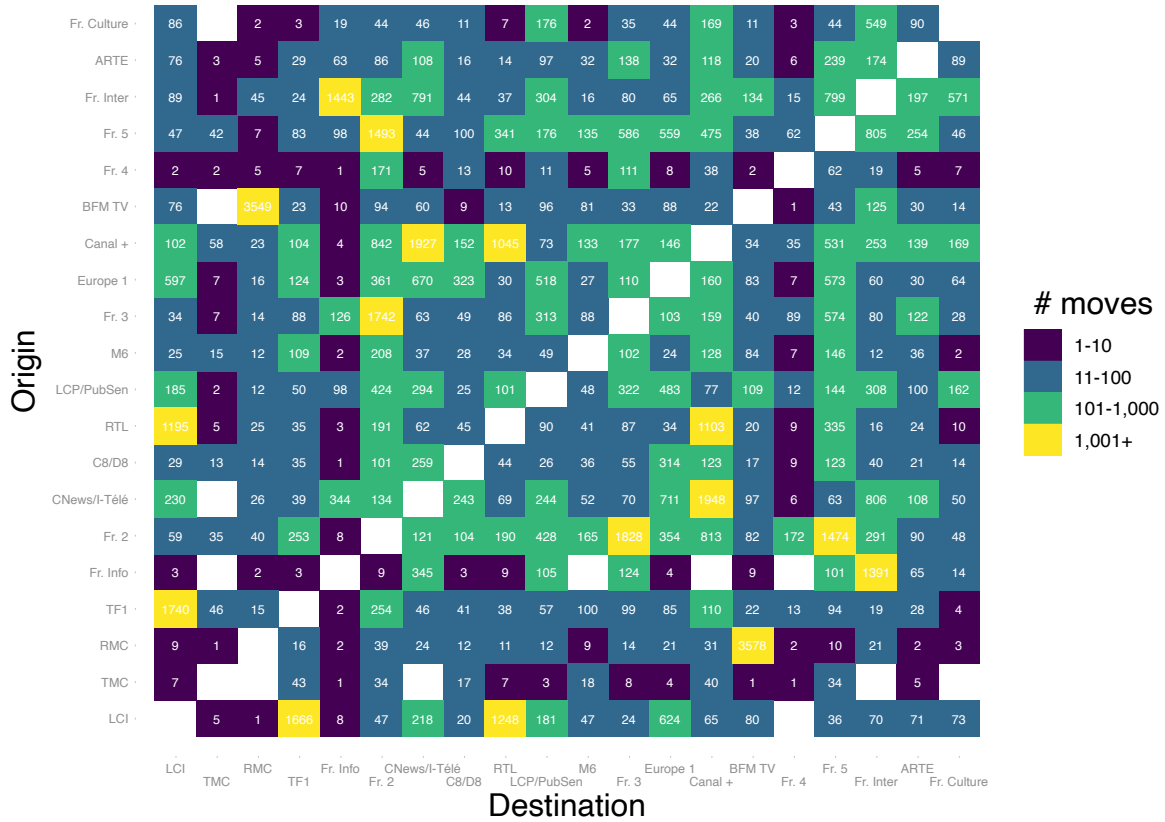
Model assumptions With this two-way fixed effects model, we implicitly assume additive separability of host, channel and time components. Identification requires that hosts’ moves are as good as random, conditional on host fixed effects, 2-season \times channel fixed effects, and date \times hours \times platform fixed effects. More formally, we assume that $E(\epsilon_{it}) = 0$; this orthogonality condition means that hosts *can* sort based on their fixed characteristics and 2-season \times channel effects. For instance, hosts tend to over-represent the right can sort into right-leaning channels without violating the identifying assumption.

Card et al. (2013b) identify three types of endogenous mobility in a standard two-way fixed effect models. One would be hosts sorting on channels based on match quality. The second would be that mobility is associated with trends in channel effects. Our specification flexibly allows channel effects to vary every two seasons. Only moves triggered by short-term changes in channel editorial lines would violate the identifying assumption. Finally, moves should not be triggered by transitory changes in editorial line.

Movers and stayers We estimate the parameters of Equation 2.1 observing the guest composition of shows hosted by movers – hosts observed on distinct channels – and stayers – hosts observed on the same channel in distinct time brackets. Regarding movers, Figure 2.4 plots a matrix reporting the number of moves for each origin-destination pair in the estimation sample. We consider that a host moves if his next show is on a channel that is distinct from the channel of its current show. By that definition, there are 65,666 moves in the data set. Outlets are ranked according to the time share dedicated to left-wing parties, from highest (top, right) to lowest (bottom, left). We observe moves across all outlets, with relatively more moves between similar outlets – as illustrated by lighter shades close to the diagonal. The number of moves is particularly high within outlets of the same group (TF1 and LCI, France 2 and France 3 for instance), which is expected since sometimes hosts have shows on both channels in a given season. The two outlets with the least moves are TMC and France 4, but these two channels only have a few shows including political guests.²⁹

Appendix Figure 2.16 plots the distribution of the differences in the time share devoted to politicians from the right and from the left between destination and origin channels at the time of the move. The distribution is roughly symmetric, meaning that there is a similar number of moves from channels that devote relatively more time to the left than to the right than the opposite. Many moves entail small destination-origin differences, meaning that hosts move between “similar” outlets from this point of view. Yet, for 50% of the moves, the

²⁹TMC’s coverage of politics was initially very limited and expanded around 2015. Later estimates are indeed more precise. Regarding France 4, the channel was close to being interrupted around 2017 and now largely prioritizes youth and educational content. Later periods effects are more imprecise.



Notes: The figure plots the number of moves for each origin and destination pair in the estimation sample. Only shows with at least one politically classified guests are included. We consider that a host moves if his next show is on a channel that is distinct from the channel of its current show. By that definition, there are 65,666 moves in the data set. Outlets are ranked according to the time share dedicated to left wing parties, from highest (top, right) to lowest (bottom, left).

Figure 2.4: Number of moves, by origin and destination outlets

absolute difference exceeds 5.0 percentage points for the left and 4.9 percentage points for the right, meaning that we observe a substantial number of moves across channels with very distinct invitation patterns.

Hosts staying on a given channel over time help identify how the environment impacts show content, holding hosts time-invariant characteristics fixed, and how the effect of this environment may change over time. Appendix Figure 2.17 plots the distribution of the number of days elapsed between the first and last show with a political guest hosted by a journalist on a given channel. We exclude host-channel pairs where the host had a show with a political guest on only one day. There are 19,219 remaining host-channel pairs. The distribution is skewed, with many pairs being short lived (25% of pairs last less than 9 months). Yet, a substantial number of hosts stay for a rather long period, with the median spell length being 943 days, more than two years and a half. Appendix Table 2.10 reports descriptive statistics on spell length by channel. These hosts staying for longer periods help track changes in the channel environment. Again, the two outlets with the shortest median spells are TMC and France 4. For this reason, as a robustness check, we also report our main variance decomposition estimates excluding these two outlets in the Appendix.

Variance decomposition To understand differences in observed political group representation across channels, we want to decompose the share of variation in invitation patterns between two broad sets of factors: on the one hand, channel-specific characteristics, such as the guidelines set by the editorial board, and on the other hand, host-characteristics like specialization or preferences. We also want to analyze how hosts sort across channels, that is whether they tend to work on channels whose guidelines fit their personal inclination.

Our decomposition between those two types of factors follows Finkelstein et al. (2016) and Cantoni and Pons (2021). Let $y_{it}^{net} = y_{it} - \tau_t$ denote the time share of a given political group at time t with host i net of time effects τ_t , which reflect news pressure, political cycles, and media viewership. Let \bar{y}_{cs} and \bar{y}_{cs}^{net} respectively denote the raw and net-of-time-effects expectations of speaking time share on channel c in season s , weighted by political time length. Let $\bar{\alpha}_{cs}$ be the channel-season level expectation of host characteristics α_i , also weighted by political time length. Then, the difference in net time share dedicated to a given political group between two outlets c and c' is the sum of the differences of the channel and host components: $\bar{y}_{cs}^{net} - \bar{y}_{c's}^{net} = (\gamma_{cs} - \gamma_{c's}) + (\bar{\alpha}_{cs} - \bar{\alpha}_{c's})$.

The share of the difference between outlets c and c' that is attributable to channel-level decisions is:

$$S_{channel}(c, c') = \frac{\gamma_{cs} - \gamma_{c's}}{\bar{y}_{cs}^{net} - \bar{y}'_{cs}^{net}} \quad (2.2)$$

It represents by how much the representation gap between two channel-season pairs would fall if the channel level editorial decisions were the same. The share attributable to hosts is:

$$S_{host}(c, c') = \frac{\bar{\alpha}_{cs} - \bar{\alpha}_{c's}}{\bar{y}_{cs}^{net} - \bar{y}'_{cs}^{net}} \quad (2.3)$$

It can be interpreted as by what share would the gap in representation between two channel-season pairs fall if hosts characteristics were the same on average. Note that although the two shares sum to 1, they need not be between 0 and 1, as $\bar{\alpha}_{cs} - \bar{\alpha}_{c's}$ and $\bar{y}_{cs}^{net} - \bar{y}'_{cs}^{net}$ might have opposite sign. That might arise if the average host working on a given channel tends to over-represent a party while the editorial guideline would suggest otherwise.

We can use an alternative decomposition of cross-channel variance in political time share across channel-period pairs. It follows from $\bar{y}_{cs}^{net} = \gamma_{cs} + \bar{\alpha}_{cs}$ that:

$$Var(\bar{y}_{cs}^{net}) = Var(\gamma_{cs}) + Var(\bar{\alpha}_{cs}) + 2Cov(\gamma_{cs}, \bar{\alpha}_{cs}) \quad (2.4)$$

From there, we can express the variance across channel-season pairs as the sum of (i) the variance in channel-level decisions, reflecting differences in editorial views ($Var(\gamma_{cs})$), (ii) the variance in average host characteristics which can be seen as differences in host composition across outlets ($Var(\bar{\alpha}_{cs})$), and (iii) the covariance between the two, which measures the extent to which hosts sort on channels whose editorial line fits their personal inclination ($2Cov(\gamma_{cs}, \bar{\alpha}_{cs})$). This way, we can assess the role played by sorting across hosts and outlets.

We estimate each component of equation (2.4) using a split-sample approach to account for the fact that channel and host effects are themselves estimates (Finkelstein et al., 2016; Cantoni and Pons, 2021). Otherwise, the variance of these estimates would indeed be inflated by the sampling error variance. We thus randomly split the sample in two subsamples of approximately identical size, stratifying by outlet-period-host. We estimate the components of equation (2.4) by taking the covariance between noisy estimates of the two subsamples, assuming that the sampling errors are orthogonal.

Event study To test whether hosts moving from one media outlet to another might already exhibit invitation patterns in line with the destination’s editorial line, we use an event-study specification. We focus on the shows of a host i just around a move from an origin outlet $o(i)$ to a destination outlet $d(i)$. We denote by δ the difference in channel-level average speaking time share of a given political family between the destination and origin at the time of the last pre-move show: $\delta_i = \bar{y}_{d(i)} - \bar{y}_{o(i)}$.

δ_i is positive (respectively negative) for hosts who move to an outlet that represents a given political group more (respectively less) than the origin outlet. The specification writes as follows:

$$y_{ir} = \sum_{t=-2, t \neq -1}^2 \theta^t \mathbf{1}(r = t) \times \delta_i + \mu_i + \nu_r + \epsilon_{ir} \quad (2.5)$$

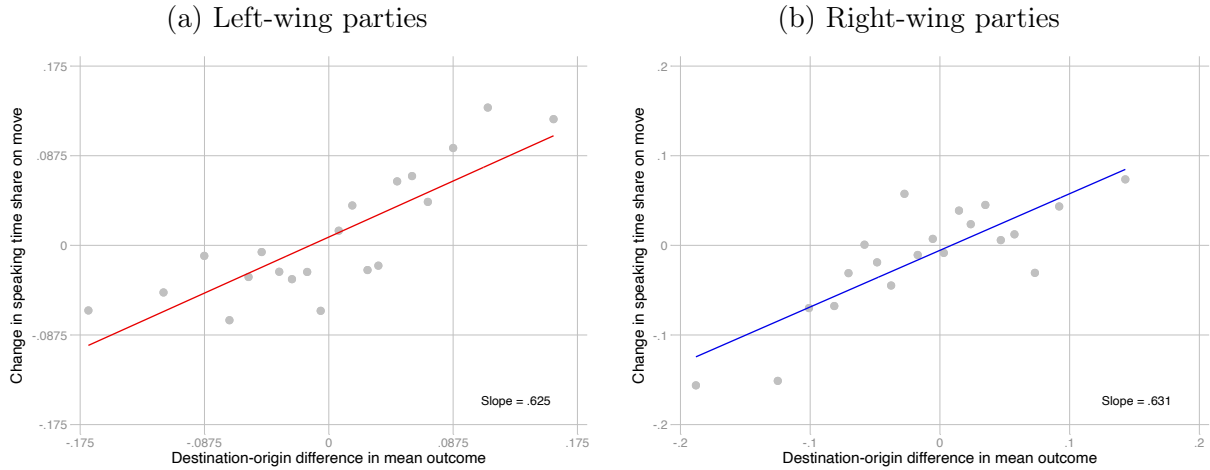
where y_{ir} is the time share of a given political group in a show hosted by host i at relative time r , with $r \in (-2, 2)$. μ_i is a set of host fixed effects and ν_r of relative time effects. Standard errors are clustered at the host level.

4.2 Changes around move

As a first step, we plot how the time share of a given political group changes in the shows hosted by a journalist as the host moves from an outlet to another. For each move, we compute the change in time share as the difference between the average time share in the last two shows on the origin channel, and the first three shows on the destination channel. This difference is plotted against the destination-origin difference in time share for the considered political group at the time of the last pre-move show. If the mover invites similar guests, irrespective of which channel he works for, then the slope should be zero. Conversely, if the fully adapts to outlets’ editorial lines, the slope should be one.

Figure 2.5 shows the relationship for all left-wing parties (Panel A) and all right-wing parties (Panel B). The slopes are around 0.63, meaning that channel-level decisions explain around two-third of the observed variation in channel-level representation of political groups.

The relationship appears linear and symmetric around zero, suggesting that a host moving from c to c' or, symmetrically, from c' to c would experience the same change in political time shares in absolute value. If hosts were sorting based on match quality, a high left-wing (right-wing) time share channel would have a different effects on hosts than a low left-wing (right-wing) time share channel. Here, the effect of moving from a low to a high left-wing



Notes: The figure shows how the political time share of a given host changes before and after a move against the difference in average outcomes across destination and origin channels. The x-axis shows the difference in average speaking time share between destination and origin channels. The y-axis shows the average speaking time share difference for a moving hosts between the three first post-move shows and the last two pre-move shows. The grey dots are averages computed by vintiles. The line is the best linear fit from an OLS regression. The slope is reported in the bottom right-hand corners of the graph.

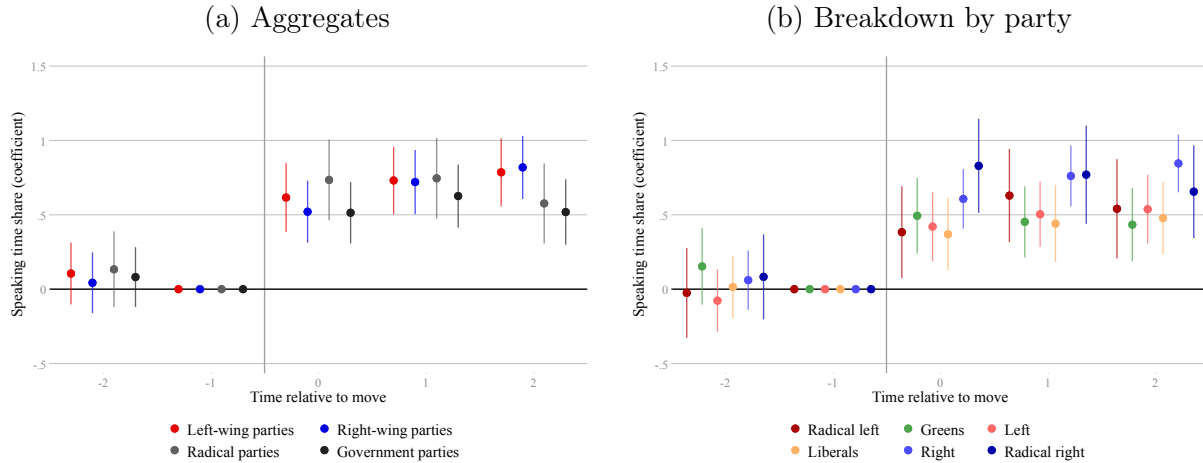
Figure 2.5: Change in moving hosts’ political time share against destination channel - origin channel differences

(right-wing) representation channel appears similar and opposite to that of moving from a high to a low left-wing (right-wing) representation channel.

Figure 2.6 plots estimates of θ^t from Equation 2.5. The reference show is the last show before the move ($r = -1$). Invitation patterns sharply change upon move. Point estimates are similar across political groups and stable after move. They are statistically significant and range between .5 and .8 for post-move shows, which is consistent with the slopes reported above. By contrast, pre-move estimates are close to zero and not statistically significant, illustrating the absence of pre-trends. When still working for their origin outlets, movers do not exhibit signs that they are gradually becoming more in line with the destination editorial line. Moves do not appear to be triggered by changes in hosts preferences, or temporary shocks. Instead, it lends support to the idea that moves in the present framework can be seen as exogenous.

4.3 Decomposition of cross-channel variation in political time share

Estimation We estimate Equation 2.1 with the time share of several political groups as dependent variables. The estimation sample includes 725,785 shows with at least one politically classified guest, weighted by the time dedicated to political guests. The model



Notes: The figure plots the event-study estimates from Equation (2.5). The dependent variable is the time share of a given political group in the shows before and after the move. It is regressed on the difference in average outcome between destination and origin channels interacted with relative time indicator variables. The sample includes all hosts who moved to another channel for their last two shows on the origin channel and the first three on the destination channel.

Figure 2.6: Change in moving hosts' political time share around move

explains between 72.9% and 75.1% of the dependent variable variance. For all dependent variables, a F-test strongly rejects the null hypothesis that all the channel effects are zero (p-value = 0.000). In their shows, hosts comply with the channel editorial policy and adapt the composition of their guests to the channel they work for.

Channel and host shares We follow equations 2.2 and 2.3 to measure the overall and relative contribution of channel and host effects across channel groups. We do so for distinct groups of channel-season pairs, C and C', with respectively a high and low time share dedicated to the political group under consideration.

Table 2.2 reports the results. Column (1) compares outlet-periods pairs whose time share dedicated to left-wing guests (upper part) and right-wing guests (bottom part) are in the top 50% to those in the bottom 50%. Columns (2), (3) and (4) compare the top and bottom 25%, 10% and 5% respectively. Channel effects consistently account for around 90% of the difference between outlets. In contrast, hosts account for only 10%. In other words, equalizing hosts across channel would only reduce the difference in political time share across channels by 10%. Appendix Table 2.11 reports results of the linear decomposition when excluding TMC and France 4, whose effects are more noisily estimated. Results are unchanged. Hosts therefore largely adapt to which channel they work for, and show content is largely dictated by channel-level decisions.

Table 2.2: Linearly additive decomposition of political time share differences

	Outlet-period pairs from the top and bottom			
	50%	25%	10%	5%
	All left	All left	All left	All left
<i>Difference in time share</i>				
Overall	0.148	0.241	0.359	0.458
Overall, net of time effects	0.082	0.139	0.255	0.406
Due to channels	0.071	0.123	0.236	0.379
Dues to hosts	0.010	0.016	0.019	0.028
<i>Share of difference due to</i>				
Channels (%)	87.25	88.31	92.50	93.14
Bootstrapped s.e.	3.87	3.91	4.50	5.37
Hosts (%)	12.75	11.69	7.50	6.86
Bootstrapped s.e.	3.87	3.91	4.50	5.37
	All right	All right	All right	All right
<i>Difference in time share</i>				
Overall	0.166	0.274	0.405	0.514
Overall, net of time effects	0.077	0.139	0.198	0.328
Due to channels	0.069	0.126	0.176	0.295
Dues to hosts	0.008	0.013	0.021	0.033
<i>Share of difference due to</i>				
Channels (%)	89.59	90.56	89.30	89.98
Bootstrapped s.e.	4.74	4.77	6.82	7.57
Hosts (%)	10.41	9.44	10.70	10.02
Bootstrapped s.e.	4.74	4.77	6.82	7.57

Notes: Each column reports the linear decomposition of the difference in average political time share across two sets of outlet-season pairs. Reported shares in rows 5 (“Channels (%)”) and 7 (“Hosts (%)”) correspond to shares presented in Equations 2.2 and 2.3 respectively. Column (1) compares outlet-periods pairs whose time share dedicated to left-wing guests (upper part) and right-wing guests (bottom part) are in the top 50% to those in the bottom 50%. Columns (2), (3) and (4) compare the top and bottom 25%, 10% and 5% respectively. Standard errors are the standard deviation of the corresponding shares bootstrapped with 100 replications.

Table 2.3: Variance decomposition of political time share differences

	All left	All right	Radical	Government
<i>Total variance</i>				
Variance, raw	0.0111	0.0134	0.0059	0.0103
Variance, net of time effects	0.0091	0.0083	0.0048	0.0093
<i>Channel effects</i>				
Variance	0.0074	0.0071	0.0042	0.0080
% variance, net of time effects	81.7	85.0	86.1	85.6
Bootstrapped s.e.	8.9	10.9	6.7	5.9
<i>Host Effects</i>				
Variance	0.0002	0.0002	0.0001	0.0001
% variance, net of time effects	2.2	2.1	1.3	1.6
Bootstrapped s.e.	3.3	4.2	1.8	1.7
<i>Covariance</i>				
2 × Covariance	0.0015	0.0011	0.0006	0.0012
% variance, net of time effects	16.0	12.9	12.7	12.8
Bootstrapped s.e.	10.6	13.1	5.9	5.5

Notes: The table reports components of the variance decomposition laid out in Equation 2.4. The first row reports cross outlet-period variance in time share, the second one does the same, netting out time fixed effects from the time shares. The third row reports the split sample variance of channel-period effects, the fourth row expresses channel effects variance as a share of total variance, net of channel effects. The fifth row reports the standard deviation of bootstrapped shares (100 replications). Rows 6 to 8 do the same for host effects, rows 9 to 11 for the covariance between host and channel-period effects.

Variance decomposition We next follow Equation 2.4 and report an alternative decomposition of the variation in political time shares. Doing so, we can test for the presence of sorting between host and channel effects.

Table 2.3 reports the results for distinct political groups – left-wing parties, right-wing parties, radical parties (i.e. the sum of the radical left and the radical right) and government parties. Again, we find that channel effects account for the largest share of variance – between 81.7% and 86.1%. The remaining variance is almost entirely explained by sorting, as covariance between host effects and channel effects accounts for between 12.7% and 16%. Hosts composition only account for a residual part, meaning that who hosts are matter only minimally to explain differences in political coverage.

Appendix Table 2.12 reports the same variance decomposition when excluding France 4 and TMC. Variance shares are of the same magnitude but are more precisely estimated. The two main options hosts seem to have are either complying with the editorial policy or work on an outlet more compatible with their baseline inclinations.

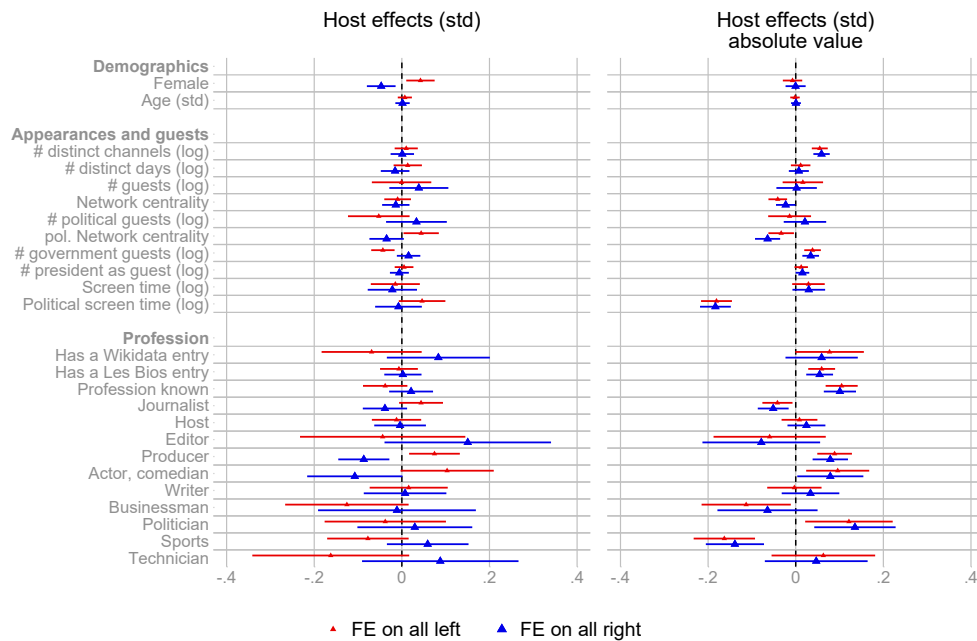
4.4 Host effects

To better understand the determinants of host fixed effects, we next correlate them with a broad set of individual characteristics. We are interested in the standardized value of the host fixed effects. A more positive (negative) value indicates the host tends to over-represent (under-represent) a given group, potentially due to preferences or specialization. We also look at the absolute value of standardized host effects, as we want to know which hosts tend to deviate from the channels' editorial policies.

Figure 2.7 presents the result of a multivariate OLS regression of various host characteristics for the 14,492 hosts in the estimation sample (Column (2) in table 2.1) for both left and right wing fixed effects of hosts. The left panel indicates that female hosts' fixed effects are associated with a deviation to the left from the channel editorial line. Moreover, hosts who are more central to the political host-guest network – as measured by their degree centrality – are somewhat more left-wing relative to their channel. Similarly, host who invite more government guests represent the left relatively less. In terms of individual's professions, estimates are rather imprecise. Moreover, Figure 2.18 in the appendix further shows that in a lasso regression that accounts for potential over-fitting most profession dummies are deselected. However, hosts who work as artists or producer tend to deviate more to the left of the channel line. All estimated correlations are rather small and less than 0.1 standard deviations of the estimated host fixed effects (1 SD = 0.18%).

The right panel of Figure 2.7 looks at absolute values of the host fixed effect, indicating whether hosts tend to systematically deviate from the average political mix, whether by over- or under-representing a given group. Whether we look at fixed effects estimated with the left or the right time share as the dependent variables, the patterns are very similar. Hosts who work on several channels, who have more screen and who have had a French president as a guest tend to deviate more. The same applies to hosts who are more known, as proxied by having an entry on *Wikipedia* and *Les Biographies*. In short, more popular hosts deviate more. They may derive this agency from their notoriety.

Interestingly, conditional on total screen time, hosts who are more central in the political guest-host network and hosts who have more political screen time deviate less from the channel line. The same applies to hosts whose identified profession is 'journalist' (rather than 'host'). Channel may select well-situated journalists to cover political issues close to the editorial line of the channel, while hosts who are *per se* not specialised on political journalism have a greater ability to deviate from the channel line.



Notes: The figure reports estimates and robust 95% confidence intervals from multivariate OLS regressions on standardised host fixed effects for left and right wing parties (left side) and their absolute values (right side).

Figure 2.7: Correlation between host effects and characteristics

4.5 Channel effects and ownership

So far, we have shown that hosts largely comply with their channel’s editorial line. We further explore how these editorial lines evolve over time. Appendix Figure 2.19 plots for each channel in our sample how its channel effects has evolved from the first period in our sample to the last. We report 95% confidence intervals bootstrapped with 100 replications, and rank channels based on their last period effect.

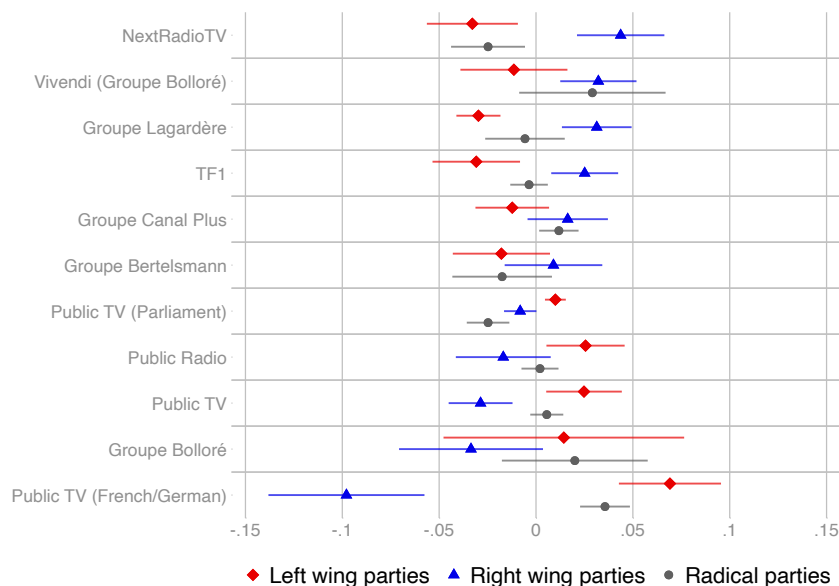
For the representation of left-wing parties, channel effects ranged from -8 to 9 percentage points in the first period, and from -12 to 12 percentage points in the last. The split-sample standard deviation of channel effects, reported in the legend, has increased from 0.04 to 0.07. A similar pattern, albeit more muted, is visible when using the time share of right-wing parties as a dependent variable. Channels’ editorial lines, when it comes to politics, has been diverging over time and are now more polarized.

Appendix Tables 2.13 and 2.14 report the variance decomposition following Equation 2.4 for three distinct periods: September 2005-August 2011, September 2011-August 2015 and September 2015-August 2019. Both for left and right-wing time shares, the share of variance explained by channel effects has been increasing over time, while both host effects and covariance have decreased. It suggests that over the considered period channels have strengthened their grip over show content.

Why have editorial guidelines changed over time? Over the same period, the French media landscape has experienced growing ownership concentration (see e.g. Cagé and Huet, 2021). The documented divergence in editorial lines may be linked to ownership changes. Oftentimes, a takeover is followed by changes in the top management and editorial board.³⁰ There are several reasons why ownership may impact the editorial line (Gentzkow and Shapiro, 2010). On the one hand, owners that have several outlets might seek to segment the market and specialize each outlet in their portfolio such that it serves a specific political segment. For instance, two television channels operating in the same market have the same *potential* viewer – whoever switches on television at a given point in time. Differentiating channels based on politics might be one way to limit competition between outlets ultimately owned by the same group. On the other hand, owners might have specific views on the type of content they want and the outlets they own might all have similar editorial lines, reflecting those views.

Figure 2.8 plots estimates from a regression of channel effects on ownership indicator vari-

³⁰For instance, when Bolloré gained control of Vivendi and Canal+ channels, he started by replacing the incumbent top management.



Notes: The figure reports estimates and robust 95% confidence intervals from multivariate OLS regressions of channel-period fixed effects on indicator variables for owner identity.

Figure 2.8: Correlation between channel effects and ownership groups

ables. Media outlets belonging to some owners systematically have editorial lines pointing in a specific direction (some are favorable to the right, others to the left, etc.). The regression R-squared are around 30%, meaning that ownership explains a non-trivial share of differences in channels' editorial policies. We explore the relationship between ownership change and channel effects into more details in Section 5, when studying the case of the takeover of three television channels by Vivendi.

5 Case study: the Bolloré takeover

5.1 Bolloré's takeover of Vivendi in a nutshell

Vivendi is an advertising, entertainment, media and publishing conglomerate whose market value fluctuated around 12 billion euros in 2022. It is the parent company of the Canal Plus Group – a television group that owns several television outlets, the leading ones being Canal+, CNews and C8.

Vincent Bolloré is the main owner of the Bolloré Group (valued 15 billion euros in 2022), which operates in a variety of industries – transport and logistics, plastics, energy, telecommunications, advertising – and in several countries, mostly in Europe and Africa. Until 2012,

the Bolloré Group owned several free newspapers and two television channels: Direct Star (later renamed CStar, a channel dedicated to music) and Direct 8 (later renamed C8). It sold 60% of its television channels to the Canal Plus Group (owned by Vivendi), 2012, in exchange for 1.7% of Vivendi shares.

Bolloré then took control of Vivendi in 2015. While the Bolloré Group owned 5.1% of Vivendi at the start of 2015, it owned more than 14.4% by April 2015. Leveraging a French law (*loi Florange*) aimed at favoring long-term investors³¹, he obtained 26% of the vote shares of Vivendi, thereby taking control of the group. Rodolphe Belmer, who was the CEO of Canal+ at the time was replaced by Maxime Saada in July 2015. Ara Apkarian, who was in charge of C8 and CNews, also left in July 2015. Vincent Bolloré himself becomes chairman of the supervisory board of Canal+ in September 2015. C8 is rebranded, its name changes from D8 to C8 in September 2016. Several C-level executives of CNews (called at the time I-Télé) are fired in July 2016, where a major strike breaks out in October 2016 in response to a change in editorial line. The channel changes name, from I-Télé to CNews and is completely rebranded in February 2017. As of March 2022, the Bolloré Group owned 29% of Vivendi, and has effective control of the company.

5.2 Compliance

In this section, we seek to understand more precisely how ownership affects hosts and invitation patterns. To this end, we study shows around the time when Vincent Bolloré took control of the Vivendi Group, the parent company of three television channels in our sample – Canal +, C8 and CNews. In a first step, we explore whether shows on these three channels features a different mix of guests compared to others in our sample in a difference-in-differences framework. Our specification writes as follow:

$$\begin{aligned}
 y_{ict} = & \beta_1 1[Treated]_c \times 1[t \in (Apr.2015, Aug.2017)]_t \\
 & + \beta_2 1[Treated]_c \times 1[t \in (Sept.2017, Aug.2019)]_t \\
 & + \delta_c + \tau_t + \gamma X_{it} + \epsilon_{ict}
 \end{aligned} \tag{2.6}$$

where y_{it} is the time share of a given political group in a show hosted by host i , on channel c , at time t . δ_c are channel fixed effects, and τ_t are date-hour time fixed effects.³² $1[Treated]_c$ is

³¹The law grants double voting rights to established shareholders.

³²Since there is no variation in treatment status within radio channels, the control group effectively narrows down to 9 television channels. To maximize power, we use date-hour time fixed effects in our main

an indicator variable for whether the channel belongs to Vivendi (Canal+, C8, and CNews). $1[t \in (Apr.2015, Aug.2017)]_t$ and $1[t \in (Sept.2017, Aug.2019)]_t$ are indicator variables for whether the show is broadcast between April 2015 and August 2017, or between September 2017 and August 2019, respectively. The two coefficients of interest are β_1 , – which captures short-term changes after the takeover, between April 2015 and August 2017 – and β_2 – accounting for medium run changes, observed from September 2017 until the end of our sample in August 2019. Splitting the ‘post’ period between a short- and a medium-run is motivated by the fact that changes occurring on channels were gradual, with each experiencing changes in C-level executives and rebranding between 2015 and 2017 (I-Télé became CNews in February 2017 for instance. By September 2017, most changes had already been implemented. X_{it} includes an indicator variable equal to C8 from 2005 to 2011. It accounts for potential differences due to C8’s past ownership.

To get a sense of whether changes in political time share are due to composition effects – some hosts leave and are replaced by new ones who invite other guests – or, rather, to continuing hosts complying with new editorial policies, we include channel-host fixed effects. The idea is to study changes in invited guests within channel-host pairs. This specification writes:

$$\begin{aligned}
 y_{ict} = & \beta_1 1[Treated]_c \times 1[t \in (Apr.2015, Aug.2017)]_t \\
 & + \beta_2 1[Treated]_c \times 1[t \in (Sept.2017, Aug.2019)]_t \\
 & + \alpha_{ic} + \tau_t + \epsilon_{ict}
 \end{aligned} \tag{2.7}$$

where, as before, y_{it} is the time share of a given political group in a show hosted by host i , on channel c , at time t , but we now control for the channel-host pair fixed effect α_{ic} . For our estimates to have a causal interpretation, the parallel trend assumption needs to hold. We test it by interacting the treatment indicator with a set of season indicator variables. Figure 2.9 plots the coefficients on the interaction terms between season indicators and the treatment status of channels. Panel (a) corresponds to Equation (2.6) and Panel (b) to Equation (2.7). We find no evidence of diverging pre-trends. Nearly all of the pre-2015 estimates are not statistically significant and hover around zero. In contrast, there is a visible increases (decrease) in the share of right (left) wing guests time share after 2015. It brings support to the validity of the difference-in-differences design, meaning that estimates

specification. We also test the robustness of our result to the inclusion of date-hour-platform fixed effects – i.e. date-hour effects that are specific to radio or television as in the previous section.

can have a causal interpretation.

Table 2.4 reports estimates from Equations 2.6 and 2.7. Comparing Bolloré channels to others, we find that in the medium run, the time share of left-wing parties declined by 6.8 percentage points (Column 1) compared to a 46.4% baseline in control channels. In contrast, that of right-wing parties increased by 5.5 percentage points (Column 3), while it was equal to 32.7% on control channels after April 2015. In both cases, it implies an increase (decrease) by more than 10% of the time share dedicated to right-wing (left-wing) guest. In the short run, the time share of radical parties increased by 1.3 percentage points (Column 5). The channels controlled by Vincent Bolloré clearly started to prioritize right-wing guests to the expense of left-wing guests after he took control of Vivendi.

Estimates in Columns (2), (4) and (6) report the change in time shares within channel-host pairs. Compared to coefficients reported in Columns (1), (3) and (5) respectively, we find that estimates are very similar. Their absolute value is slightly lower for left-wing parties, and slightly larger for right wing and radical parties, but are qualitatively the same. It implies that changes in the mix of guests on Bolloré channels is not entirely driven by hosts being replaced by others. Instead, hosts who stayed adjusted who they invite to the new editorial policy in the same proportions as the overall change. It shows that compliance was one of the mechanisms underlying the ownership-induced change in editorial line.

Appendix Table 2.15 reports estimates for each political group. For hosts who stayed, the time share of radical right guests decreased by 2.5 percentage points (10.8% on control channels), and that of left-wing hosts decreased by 2.6 percentage points (28.9% on control channels). On the right, the increase is largely driven by an increase in the radical right time share: +5.3 percentage points, with respect to an average 7.9% on control channels. The increase in the right-wing guest time share is therefore driven by far-right guests who crowded out left and radical-left guests.

Appendix Table 2.16 reports baseline estimates separately for each Bolloré channel. Coefficients are less precisely estimated. The time share of radical parties increased by 9.4 percentage points on C8. That of right-wing guests increased by 5.9 percentage points on CNews, while that of left-wing guests decreased by 8.0 percentage points. On Canal+, left-wing time share decreased by 3.4 percentage points, that of right-wing parties increased by 3.6 percentage points, and that of radical parties decreased by 3.6 percentage points. Point estimates are overall similar when including channel-host fixed effects, but standard errors are larger.

Appendix Tables 2.17, 2.18 and 2.19 report robustness checks. We estimates the same

Table 2.4: Effect of the takeover on the time share of political groups

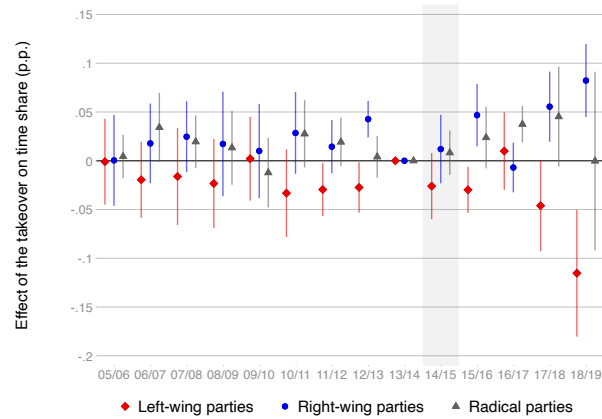
	(1)	(2)	(3)	(4)	(5)	(6)
	All left-wing parties		All right-wing parties		Radical parties	
Treated×2015/17	0.00597 (0.0107)	0.00389 (0.0100)	0.00504 (0.00914)	0.0108 (0.00971)	0.0132* (0.00719)	0.0174** (0.00783)
Treated×2017/19	-0.0676*** (0.0227)	-0.0594** (0.0245)	0.0550*** (0.00954)	0.0645*** (0.0111)	0.00668 (0.0339)	0.0281 (0.0276)
Observations	771080	754993	771080	754993	771080	754993
R^2	0.623	0.638	0.621	0.637	0.619	0.635
Channel FE	Yes	No	Yes	No	Yes	No
Channel-host FE	No	Yes	No	Yes	No	Yes
\bar{y} (control, post)	.464	.464	.327	.327	.187	.187

Notes: The outcome variable is the time share of distinct political groups: left-wing parties (radical left, greens and left) in Columns (1)-(2), right-wing parties (right and radical right) in Columns (3)-(4), radical parties (radical left and radical right) in Columns (5)-(6). Estimates in odd-numbered columns correspond to Equation 2.6, estimates in even-numbered columns correspond to Equation 2.7. The last row reports the mean of the outcome variable on control channels during for the period ranging from April 2015 to August 2019. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

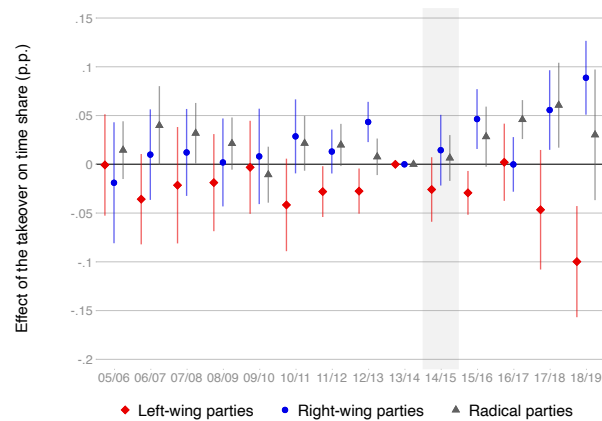
specification with distinct time fixed effects, excluding equal-time period mandated by the ARCOM, using an inverse hyperbolic sine transformation of the outcome variable, excluding government members, excluding the guests who are not politicians but who we classify politically, and excluding summer months. Overall, point estimates remain very stable across specification, and are nearly all statistically significant.

5.3 Sorting

Results so far show that hosts who stayed on the Bolloré channels complied to the new to editorial guidelines. In this section, we explore whether hosts reacted to the owner-induced change in editorial line by leaving treated channels. To do so, we collapse our data set at the host-channel-quarter level and define an indicator variable equal to one if a host observed on a given channel in quarter t is still observed on this channel in quarter $t + 4$ – i.e. one year later. We compare the likelihood that a host stays on the channel across treated (Canal+, C8 and CNews) and control channels in our data. The specification writes as follows:



(a) With channel fixed effects



(b) With channel-host fixed effects

Notes: The Figures plots estimates from event-study specifications corresponding to Equation 2.6 (Panel a) and to Equation 2.7 (Panel b). The dependent variables are the time share of left wing parties (red diamonds), of right-wing parties (blue dots), and of radical parties (grey triangles). The shaded area corresponds to the season running from September 2014 to August 2015 during which Vincent Bolloré took control of the channels. Standard errors are clustered at the channel level, vertical bars indicate 95% confidence intervals.

Figure 2.9: Event-study regressions: time shares around takeover

$$y_{ict} = \sum_{q \neq 2013q1} \beta_q 1[Treated]_c \times 1[t = q]_t + \alpha_{ic} + \delta_t + \epsilon_{ict} \quad (2.8)$$

where y_{ict} indicates whether host i observed on channel c in quarter t is still on the channel in quarter $t + 4$. α_{ic} are host-channel pair fixed effects, which capture any fixed characteristics that are specific to the match between a host and a channel. δ_t are quarter fixed effects. $1[Treated]_c$ indicates whether the channel considered is one of those controlled by Vincent Bolloré in 2015. $1[t = q]_t$ are quarter dummy variables. The coefficients of interest are β_q , which account for the difference that existing host-channel matches are continued across treated and control channels.

Figure 2.10 plots the event-study estimates. Before the takeover, the propensity of hosts to continue working for their network followed similar trends across treated and control channels in our sample. The absence of diverging pre-trends lends support to the causal interpretation of our estimates. Starting around September 2015, we find that hosts on acquired channels are significantly more likely to discontinue their work. Hosts who worked on one of the Bolloré channel in 2016 were 20 percentage points less likely to still be on the channel the next year. As a reference point, the probability to keep working on at control channel at the same time was around 38%, meaning that the probability that hosts stay was halved after the takeover. Panel (b) of Figure 2.10 reports similar estimates, weighted by the speaking time of guests. Doing so, estimates are more negative around 2016, nearing -40 percentage points. It suggests that hosts with many guests were especially likely to leave.

Table 2.5 shows the difference-in-difference estimates interacted with several hosts characteristics. We first find that the hosts more likely to leave were also those most exposed to changes in the editorial line: those that had politically classified guests, and among them those who have an above-median share of politically-classified guests. Hosts whose shows were newscasts and hosts described as ‘journalists’ in the credits were also more likely to leave.³³ Table 2.20 provides the breakdown by channel; the effect is present on all three channels.

We also find that male hosts were much more likely to stay on treated channels than their female counterparts. Famous hosts, as proxied by a LesBiographies entry, were more likely to leave in the short run, but much more likely to stay in the medium run. It suggests that

³³Regarding the last find, it may partly be driven by the Brachard law (1935) that allows in France journalists (defined as employees with a *carte de presse*) to resign from their job and receive benefits (one month of wage per year of seniority) in case of ownership change and/or major change in editorial line.

some of them decided to leave early after the takeover, but that past the first wave, renowned hosts were more likely to stay. Similarly, we find that hosts who have been on the channel for at least two years – potentially flagship hosts – were initially more likely to leave (-6pp) – but ultimately more likely to stay (+12pp). Although estimates are less precise, we find that hosts whose shows were during prime time and whose ratings were higher were more likely to stay. One potential explanation is that these hosts have more bargaining power. Some might have decided to leave early on, confident that they could work somewhere else, or decided to stay, thinking that their bargaining power was such that they could negotiate favorable conditions.

We next turn to the destination channels of the hosts who left following the takeover. Appendix Figure 2.20 plots event study estimates for several outcomes. Panel (a) shows that the takeover caused a 30 percentage-point increase in 2016 of the number of host not observed on any channel in our sample in quarter $t + 4$. This figure is around 15 percentage points in 2017 and 2018. Compared to the corresponding figure on control channels at the same time – 58.3% – this is a 25-50% increase in the probability of stopping working on one of the sample channels. It suggests that, for many departing hosts, the takeover implied a drastic change in career, potentially leading hosts to take up a job in other types of media organizations (pure players, newspapers, etc.) or simply leaving journalism. Panel (b) studies the share of hosts who leave and who are working on another channel of the sample. This fraction increases by about 3 percentage points, nearly doubling the 3.8% share on control channels. Panels (c) to (f) split destination channels across quartiles of right-wing time share. Most hosts leaving Bolloré channels went to work on one of the 5 channels with the smallest right-wing time share (+2 percentage points, with respect to 1.2% on control channels).³⁴ Inflows are smaller for outlets in the second quartiles, and close to zero for other networks. It suggests that the hosts who left Bolloré channels for another one when the editorial policy was pushing for more right-wing guests disproportionately joined channels that invite relatively fewer right-wing guests, hinting at a potential sorting based on political preferences.

In Appendix Tables 2.21 and 2.22, we study the characteristics of hosts who left and appeared on any or no other channel. Being a journalist largely increases the probability to be observed on no other channel, but has no effect on the probability to be observed on another channel. Conversely, male hosts are equally likely to be observed on another channel, but are much less likely to be observed on no other channel. Being more renowned is associated with a higher probability of being observed on another channel, and a negative probability of being

³⁴Channels in the first quartile in terms of right-wing parties time share are ARTE, France Culture, France Inter, France 5 and France 4.

observed on no other channel. Overall, it suggests that while some journalists were the most likely to exit, either because they are those for whom the labor market is the most precarious, or because their skills are more portable to other platforms (newspapers, pure players, etc.). Renowned hosts may react more quickly precisely because they can find a position on another media outlet more easily.

Taken together, the results show that as acquired channels experienced a shift in editorial policy to the right, many hosts left these channels. The majority appeared on none of the channels of our sample a year later, meaning that their careers could have been negatively impacted. Those who started working on other channels in our sample went to work on those giving relatively less speaking time to the right. They may have left due to disagreements with the new editorial policy and found those destination channels more compatible with the type of shows they want to create. For those who stayed, as evidenced in the previous section, they largely complied with the new editorial policy, with a significant increase in right wing time share from 2017-2018, after most hosts had already left.

6 Discussion and Conclusion

In a context of decreasing advertising revenues and increased media competition, business tycoons' appetite for traditional media outlets does not seem to wane. Recent empirical evidence has shown that changes in ownership can affect media content, therefore potentially impacting the set of information viewers have and their ability to hold elected officials accountable. These concerns warrant a better understanding of the mechanisms through which owners may impact media slant. This paper opens the black box of news production and highlights the mechanisms through which slant happens.

Our article is the first to quantify the contributions of media outlet and journalist-specific factors in slanting the news. Of course, our analysis suffers from a number of caveats. Not least, we only consider media slant using information on the guests and do not study the content of the shows. While analyzing content could be of interest for future research – and keeping in mind the fact that doing so would raise important technical challenges, given it would require not only to use the transcripts of millions hours of shows but also to determine who says what – we nonetheless believe that the platform given to political parties through the presence of guests in the media is an important component of media slant as of today.

The main contribution of our article is with respect to the political economy literature studying pluralism and how (well) voters are informed. However, we think that our work can also inform policy-makers on the relevance of existing media pluralism regulations. In

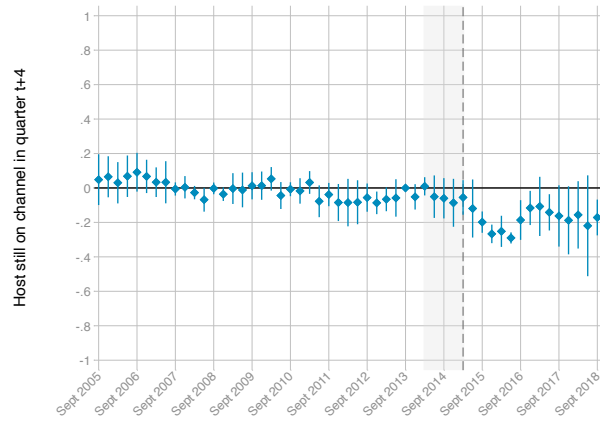
Table 2.5: Hosts staying on acquired channels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	1(Pol guests)	1(Many pol)	1(Journalist)	1(Newscast)	1(Male)	1(LesBios)	1(2y ago)	1(Prime)	1(Ratings)	1(Res ratings)
Treated × 2015/17	-0.154*** (0.0429)	-0.0823*** (0.0209)	-0.182*** (0.0503)	-0.113*** (0.0442)	-0.113** (0.0441)	-0.246*** (0.0475)	-0.149*** (0.0411)	-0.0931*** (0.0291)	-0.138** (0.0510)	-0.168*** (0.0366)	-0.155*** (0.0445)
Treated × 2017/19	-0.151 (0.0883)	-0.140* (0.0705)	-0.173 (0.107)	-0.0888 (0.0879)	-0.119 (0.0883)	-0.296*** (0.0837)	-0.164* (0.0920)	-0.190*** (0.0643)	-0.152 (0.0927)	-0.245 (0.148)	-0.198* (0.103)
Treated × 2015/17 × 1.Inter	-0.122*** (0.0269)	-0.122*** (0.0269)	-0.0848*** (0.0244)	-0.118*** (0.0182)	-0.253*** (0.0840)	0.125*** (0.0201)	-0.0428** (0.0185)	-0.0663*** (0.0249)	-0.0535 (0.0700)	-0.0290 (0.0879)	0.0359 (0.0527)
Treated × 2017/19 × 1.Inter	-0.0127 (0.0380)	-0.0127 (0.0380)	-0.00915 (0.0331)	-0.231*** (0.0479)	-0.337*** (0.0734)	0.193*** (0.0348)	0.175** (0.0695)	0.116*** (0.0294)	0.0212 (0.0305)	0.0549 (0.0700)	0.0515** (0.0208)
Observations	263832	263832	143131	263832	263832	263832	263832	263832	263832	146100	154436
R ²	0.468	0.470	0.480	0.469	0.469	0.469	0.469	0.471	0.468	0.492	0.487
\bar{y} (control, post)	0.379										
\bar{y} (control, post, inter=0)	0.280	0.280	0.399	0.388	0.338	0.379	0.372	0.242	0.349	0.298	0.337
\bar{y} (control, post, inter=1)	0.455	0.455	0.513	0.371	0.461	0.380	0.467	0.553	0.438	0.445	0.403

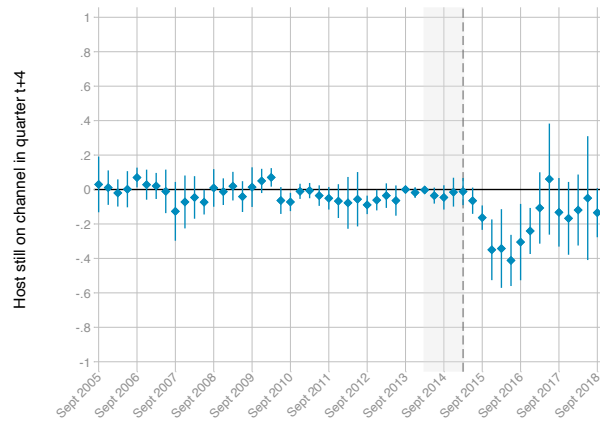
Notes: The outcome variable is an indicator for whether a given host-channel pair existing in quarter t is still existing in quarter $t + 4$. Column (1) presents the baseline specification. Column (2) includes an indicator for whether the host has guests who are politically classified. In Column (3), the indicator is for, among hosts who have political guests, those that have an above channel-quarter specific median share of political guests. In Column (4) the indicator is for whether the host is credited as a journalist for the show. The dummy in Column (5) indicates whether the host's show is a newscast. In Column (6), the variable indicates whether the host is male and in Column (7) whether he has a 'Les Biographies' entry. The indicator variable in Column (8) is for whether the host was already on the channel two years ago. The indicator variable in Column (9) is for whether the host's shows are during prime time (7:00-9:00am for radio, 19:00-21:00 for TV). In Column (10), the indicator is for whether the host has above median viewership (within channel-quarters). The indicator in Column (11) is similar, except that the viewership share is residualized on date-hour FEs and channel-season FEs, to measure whether the host tends to over- or under-perform. The last rows report the mean of the outcome variable on control channels for the period ranging from April 2015 to August 2019. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

particular, from a descriptive point of view, we show that media owners tend to bias the content of broadcast shows not only by disproportionately inviting politicians from one side of the political spectrum, but also by inviting non-politician yet politically-involved guests from the same side. The most likely explanation for such a behavior is that the latter are not accounted for by existing pluralism regulations (not in France nor in other democracies).

Note that this also has consequences for the existing literature on media bias that, by only considering politicians – i.e. by not taking into account the guests who are not politicians but nonetheless politically vocal – may miss an important part of slant, and thus also of its consequences.



(a) Unweighted



(b) Weighted by guest screen time

Notes: The Figures plots estimates from event-study regressions corresponding to Equation 2.6. In Panel b, observations are weighted by guest screentime, the are not weighted in Panel a. The dependent variable is a dummy for whether a given host-channel pair observed in quarter t is still observed in quarter $t + 4$. The shaded area corresponds to the season running from March 2014 to March 2015, which is when Vincent Bolloré took control of the channels. Standard errors are clustered at the channel level, vertical bars indicate 95% confidence intervals.

Figure 2.10: Hosts' probability to stay on the channel after the takeover

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A Appendices

A.1 Dataset

INA data coverage benchmark

We use another data source, Plurimedia, to benchmark INA data coverage. Plurimedia is a company that collects metadata on scheduled television shows before they are broadcast, and sells them to websites and magazines publishing television schedules. The data set includes all shows, 24 hours a day, for all the television channels from September 2009 to December 2020. For each show, the data provide information on the channel, date, scheduled start time, length and title.

Building on Plurimedia show classification, we devise 12 show categories: (i) newscasts, (ii) shows about news and politics (interviews, in-depth analysis of specific news topics, etc.), (iii) talk shows about politics (debates, news commentary with pundits or commentators), (iv) entertainment talk shows (which also include infotainment talk shows such as late shows), (v) entertainment shows (reality TV, home makeover shows, cooking shows, etc.), (vi) sports shows, (vii) youth shows (cartoons, educational programs), (viii) games, (ix) performance shows (concerts, plays, etc.), (x) fiction, (xi) documentaries, and (xii) other shows (weather forecast, lottery, undetermined night-time programs, etc.).

Figure ?? depicts the time share of each television program category for the fourteen television channels of our sample using Plurimedia data. Newscasts, shows about news and politics, and talk shows³⁵ account for about a third of the total screen time. Panel (b) focuses on these categories. The time share dedicated to newscasts has decreased from about 15% to less than 10% between 2009-10 and 2019-20, and is now similar to that of political talk shows, which accounted for less than 5% of the total screen time in 2009-10.³⁶ This stylized fact motivates our decision to study a broad range of shows, rather than only newscasts.

We match shows in Plurimedia data with shows in INA data, and determine for each category the time share of shows that are in both datasets. Figure 2.11 contrasts the coverage of shows by type across Plurimedia and INA data. While newscasts, shows about news and politics, and talk shows are nearly all included in INA data, only a subset of entertainment, sports,

³⁵Many entertainment talk shows are *infotainment* shows. They also discuss recent news and political events, and regularly invite politicians or activists. Such shows include *Le petit journal* or *Touche pas à mon poste*.

³⁶In most of the analysis, we work at the "season" level. A season refers to a twelve-month period ranging from September 1st to August 31st.

youth programs and documentaries are covered. Most of the difference between INA and Plurimedia data coverage can be explained by fiction shows. Overall, the figure shows that INA data provides are broad coverage of shows that have hosts and guests, which makes it ideal to measure political slant using guest speaking time shares. Notably, while most studies in the media bias literature only focus on news shows, we cover a much broader range of programs, whose total length far exceeds that of newscasts only.

Sample definition

Regarding television, we exclude channels that have only fiction programs (e.g. TFX, NRJ12), music programs (e.g. CStar), or youth programs (e.g. Gulli). We also exclude channels that were created later – this is the case of franceinfoTV, which launched in 2016 – and we exclude channels that require subscription (e.g. Paris Première, Planète+). We do include Canal+ even though programs during some time slots are only available to subscribers. There are however shows available for free around prime time that gather a substantial audience, which is why we include the channel.

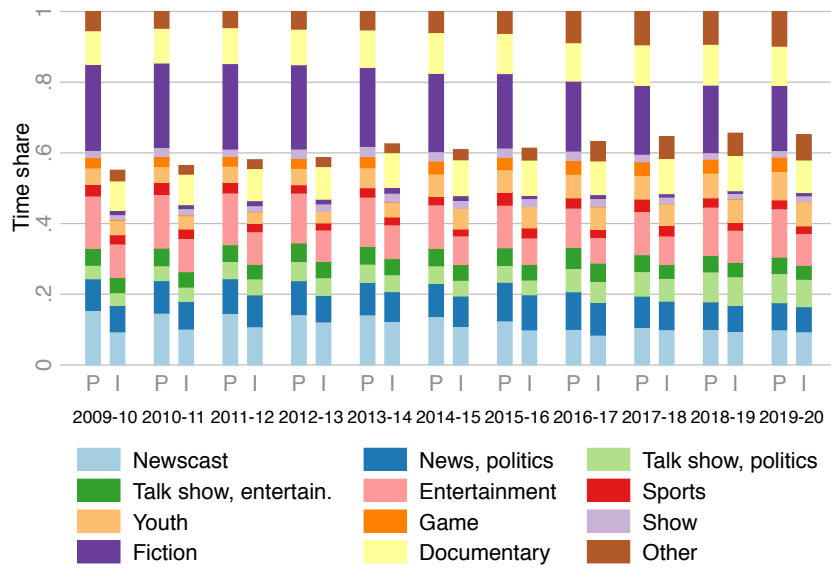
Regarding radio, BFM Radio and Radio Classique are not included due to scarce coverage in INA data.

Classifying guests

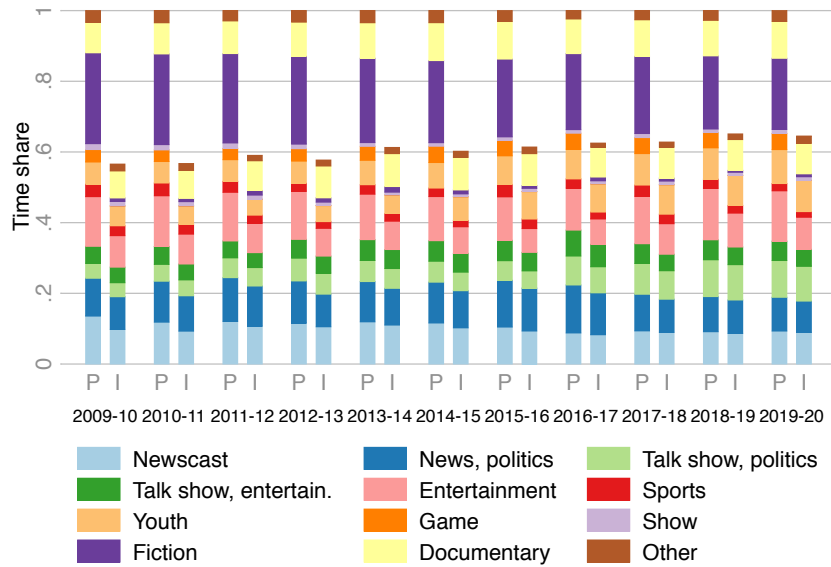
In this section, we provide details on the methodology we use to classify the guests in our sample. We distinguish between politicians on the one hand, and politically-engaged non-politicians, which we call PENOPs, on the other hand.

Politicians To classify the politicians, we use several data sources:

- **Arcadie project.** The Arcadie project is an open data website that gathers information on elected officials. For instance, their age, gender, profession, place of birth, spouse job, electoral district, committee assigned to, social media accounts, etc. We collect data on the group affiliation of MPs. Each year, they are supposed to pay a membership fee to the parliamentary group they are assigned to. Some of them, when they switch party during their term start paying their membership to another group. This is the information we collect. This way we can track the party affiliation of MPs, who are major political figures in the French political landscape.
- **Elections data.** We then collect election data for several elections: legislative elections (National Assembly), senate elections, European elections, regional elections,



(a) All programs



(b) Excluding programs starting between 11pm and 5am

Notes: “P” refers to Plurimedia data, and “I” refers to INA data. The vertical bars show the breakdown of programs by type for the 14 channels in our sample. Bars denoted “P” depict the time dedicated to programs of each category, divided by the total screen time in the considered semesters as documented in Plurimedia data. Bars denoted “I” depict the time dedicated to programs of each category in INA data, divided by the total screen time in the corresponding categories in Plurimedia data. Shorter “I” bars reflect that some shows are not documented in INA data.

Figure 2.11: Data coverage comparison between Plurimedia data and INA data

departmental elections and municipal elections.³⁷ If candidates run by lists, we get all the names on the list (European elections for example). One exception are municipal elections. Given some municipalities are very small, the last candidate on a municipal election list almost never gets elected and never appears in the media. In this case, we keep the top 5 candidates of each list in municipalities with at least 100,000 registered voters, and the first on the list for municipalities with at least 20,000 registered voters. For elections, we consider candidates are affiliated to the party whose label they are running with three month before the election date (to account for the campaign period), and three months before the end of the mandate (they might be running again with a different affiliation).

- **Government.** We collect government members (*ministres*, *secrétaires d'état*, and *directeur de cabinet du président*), and consider they are affiliated to the president's party.

Next, for each person in a given month, we search the above mentioned data sets for a political affiliation. We give some data sources precedence over others. The first one is the Arcadie data set, as party affiliation is allowed to change within terms. Next, we use legislative elections (National Assembly elections), Senate elections, and then whether the person is in the government. Government data comes after legislative and senate elections data because, sometimes, the government includes politicians from distinct adjacent parties. For instance, politicians from the Green party have worked under the socialist president, while not affiliated to the socialist party. We then use other election data sources in the following order: European, regional, departmental, and municipal elections. If some politicians have “holes” in their electoral careers, we extend their past affiliation in the future.

Politically-engaged non-politicians (PENOPs) To determine the political leaning (if any) guests who are not politicians, we use data from three different sources: (i) the annual summer meetings organized by political parties (*universités d'été*), (ii) think tank staff and contributors, (iii) endorsements of politicians in op-eds published in the press. Our goal is to collect data on behaviors that we consider, when aggregated, reveal the political leaning of a person. These behaviors are analyzed with a probabilistic model in which the recurrence of such behaviors is considered indicative of a given political leaning.

Summer meetings of political parties We collect data on the participants of political party summer meetings. These meetings typically gather politicians and party executives but also academics, media personalities, businessmen, activists, or union representatives. By

³⁷ *Régions* and *départements* are intermediate tiers of government in France. Municipalities are the lowest.

participant, we here mean people whose name was on the program and who were invited to give a speech or take part in a round table. Although taking part in such events does not imply that the person is affiliated to a party, we consider it is suggestive of the political leaning of a person.

We collect data from various sources. For recent meetings, we retrieve the program on the party website (typically, events from 2021 and sometimes 2020). For older events, we used the Wayback machine search engine (Web archive). We also directly contacted parties and asked them the program of their past meetings. Some answered positively to our requests and shared copies of the programs from their own archives (UMP/LR, Modem and Les Verts/EELV).

Overall, we have an extensive coverage of the French political landscape: close to one hundred programs (n=96), from the radical right to the radical left. It is to be noted, however, that the information was scarcer on the right than on the left: Parti socialiste, Parti communiste and Les Verts/EELV nearly account for 50% of the programs (47, 51 if you include the more recently born LFI), while liberal parties account for 20% of the sample (18 programs for the Modem, UDI and LREM). Meanwhile, important right-wing parties such as FN and UMP/LR account for less than 15% of the sample, with 12 programs retrieved for the two parties combined. As a general observation, summer meetings of left wing parties are large events directed at a substantial audience, reaching beyond the circle of political activists, hosting hundreds of speakers from the party leadership and civil society; they are also generally held every year. Right wing parties' events are however different. Their audience is mostly restricted to political activists, and sometime include the youth section of the party, with the goal of training young political activists and letting them meet important figures of the party. These parties hold summer meetings less regularly, with many blank years (especially on presidential elections years), and there are less speakers. These discrepancies may be explained by historical and ideological reasons, summer universities or large instructional events being a traditional tool of the progressive political forces to reach a broader audience, as opposed to conservative parties centering on a network of local elites, without needs of propagating their ideology to large segments of the population. For this reason, we also collect data on the summer meetings of smaller right wing parties: *Action Française* (a nationalist and royalist micro-party), *La Manif pour Tous* (a political movement created in opposition to same-sex marriage in 2013 which later transformed in a political party), *Chrétienté-Solidarité* (a Catholic traditionalist political organization close to the National Rally), *Oser la France* (Christian socially and economically conservative political movement), *Renaissance Catholique* (traditionalist catholic political movement), *Acteurs*

d'Avenir (Christian organization aimed at educating “tomorrow’s Christian leaders”), and *La Convention de la Droite* (a summer meeting organized by radical right politicians to foster alliances with traditional right-wing parties).

- **La France Insoumise** (radical left). 4 summer meetings, 2017-2020. Programs found online.
- **Parti de Gauche** (radical left). 6 summer meetings, 2011-2013, 2015-2017. Online and Wayback machine.
- **Parti Communiste Français** (radical left). 11 summer meetings, 2008, 2009, 2011-2020. Found with the Wayback machine.
- **Europe Ecologie Les Verts** (greens). 20 summer meetings, 2002-2021. Received from party’s archivists, and online.
- **Mouvement Républicain Citoyen** (left). 6 summer meetings, 2008-2012, 2014.
- **Les Radicaux de Gauche** (left). 2 summer meetings, 2018-2019. Online.
- **Parti socialiste** (left). 16 summer meetings, 2002-2015 and 2020-2021. Received from the Fondation Jean Jaurès, and found with the Wayback machine
- **Le Vent se Lève** (left). 2 summer meetings, 2018-2019. Online.
- **Mouvement Démocrate** (liberals). 13 summer meetings, 2008-2020. Received from party’s archivists, and online.
- **La République En Marche** (liberals). 2 summer meetings, 2019, 2020. Found online.
- **Union des Démocrates et Indépendants** (right). 3 summer meetings, 2018-2020. Obtained from Wayback machine and online.
- **Union pour la Majorité Présidentielle/Les Républicains** (right). 9 summer meetings, 2003, 2006, 2008, 2009, 2011, 2015, 2017, 2020, 2021. Received from party’s archivists.
- **Acteurs d’Avenir** (right). 11 summer meetings, 2010-2015 and 2017-2021. Online and Wayback machine.
- **Osons la France** (radical right). 3 summer meetings, 2018-2020. Online and Wayback machine.
- **La Manif pour Tous** (radical right). 7 summer meetings, 2013-2019. Online and Wayback machine.
- **Chrétienté et Solidarité** (radical right) 10 summer meetings. 2008-2013, 2015, 2016, 2019, 2021. Online and Wayback machine.
- **Front National/Rassemblement National** (radical right). 3 summer meetings, 2011, 2013 and 2016. Found with the Wayback machine.
- **Convention de la droite** (radical right). 1 summer meeting, 2019. Online.

- **Action Française** (radical right). 4 summer meetings, 2017-2019, 2021. Found online.

Think tanks Next, we collect data on staff members and contributors of think tanks. Many intellectual figures, pundits, or more generally policy commentators regularly contribute to think tanks publications. These publications can be long and detailed reports, or posts on recent news events on the think tank’s website. Our goal is to collect the name of contributors and staff members as, plausibly, choosing to associate one’s name with a think tank reflects some form of political alignment.

We start by identifying the main French think tanks. To do so, we start with the list compiled by the Open Think Tank Directory, and sort them according to their number of Twitter followers, as documented in the data set. We focus on think tanks that have more than 5,000 followers, as others are generally really niche. We then discard the think tanks that do not have a web site, or that have no publications. It is the case of, for instance, the *Fondation Danielle-Mitterrand - France Libertés* that mostly raises funds and financially supports targeted projects. We also discard think tanks that can be assimilated to research centers (INRAE, CERI, etc.) and do not exhibit a particular political leaning, or that are affiliated to an administration (France Stratégie, CEPIL, etc.) as their leaderships change with elections. We also do not consider very recent think tanks, such as *Hémisphère Gauche*, *Institut La Boétie* (both created in 2020). We decided to include all organizations, whether a foundation or a non-profit organization, whose stated goal is to inform the political debate and which, for that purpose, produces reports and (or) organizes conferences. Some of these think tanks are generalists, others focus on economic, geopolitical, judicial or environmental issues for example.

For each think tank, we map them to political parties based on several criteria. First, founders or top management staff are sometimes clearly politically involved. For instance the *Fondapol*’s founder, Jérôme Monod, was the cabinet director of Jacques Chirac, and its current director, Dominique Reynié, is a right-wing elected official. The *Fondation Gabriel-Péri*, named after a communist politician, was created by the Communist Party itself. *Terra Nova* was created by Olivier Ferrand, a Socialist Party executive. Next, we rely on the think tank’s own stated goal. For example, *Polemia*, founded by far-right politician Jean-Yves Le Gallou, claims on its “About us” that its work is structured around “identity defense, criticism of oligarchy, and media tyranny,” which are typical of the far right rhetoric. ATTAC, a radical left organization, states that it fights for “social and environmental justice and conducts actions against the power of finance and multinational companies,” which in this case is ideologically typical of radical left movements. We also study the funding of these

think tanks. We have data on which organization members of parliament decided to grant part of their discretionary budget line (known as *réserve parlementaire*) to.³⁸ Finally, we collect the Twitter handle of each think tank and of members of parliaments. Using simple retweets (retweets without comments), we situate each think tank in the French political space. This is illustrated in Figure 2.12. If, with these methods, the political positioning of think tanks is still ambiguous, or if they do not seem to be politicised, then we consider they are not political and do not classify them.

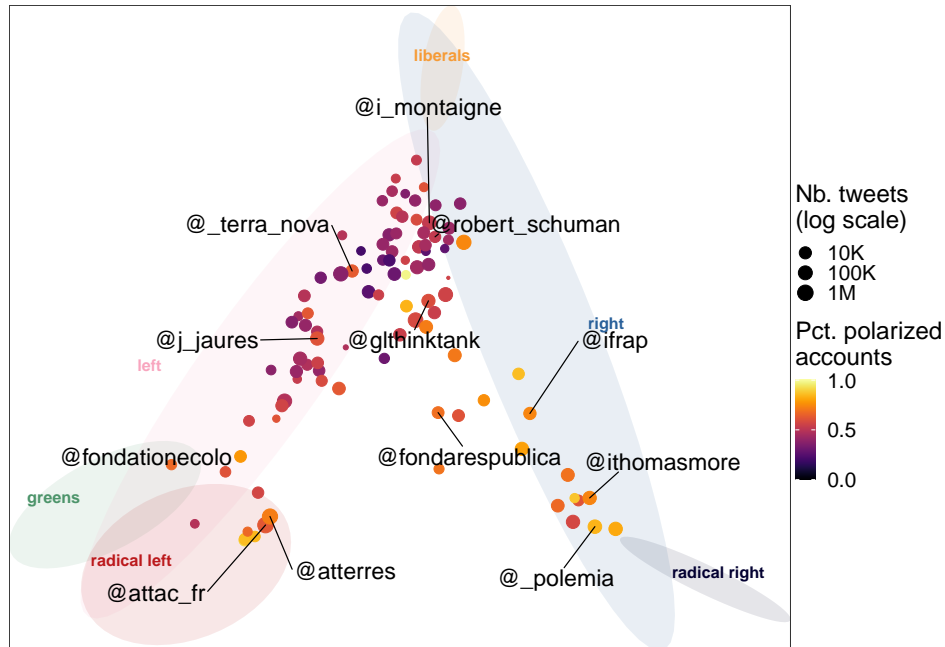


Figure 2.12: Think tanks projected on the French political Twitter space

We then collect data on staff members and contributors. For staff members, we use the think tank’s web page “Our team” (or the equivalent). Using the Wayback machine, we collect all the names of people on this web page for every year since 2002, or for as many years as possible. For contributors, we scrape publication title, dates and authors. Table 2.9 reports the list of think tanks for which we collect data, their creation date and political family. The next two columns present the number of staff members and contributors that we found for each think tank. The same person can be counted several times if she has been part of the staff for several years, or contributed to several publications. For some think tanks, no staff was found. It is the case of Polemia, which does not disclose this information on its website. For some think tanks, there are no contributors (Fondation Copernic, Fondation pour la

³⁸This dataset is called “Reserve Parlementaire” and is available from 2013 to 2017. We look at the party affiliation of the MPs who granted money to think tanks drawing from their own budget line that they can use at discretion for either fund non-profit organizations or local governments.

Nature et l’Homme, and The Shift Project). That is either because all publications are not signed at all, or signed as a team (Copernic). Sometimes, the format of publication being very ad hoc and different each time, we were not able to scrape author names (Fondation pour la Nature et l’Homme and The Shift Project). In the last two columns, the Table reports the number of occurrences of staff members and contributors that were matched with INA data. The figures are always smaller, which is because people never appearing in the media. Overall, we match nearly 9,000 occurrences of staff members, and more than 18,000 occurrences of contributors.

Endorsements in newspapers We collect the names of people who signed opinion pieces in newspapers in which they endorse a candidate running in the first round of the presidential elections. Such opinion pieces are generally signed by several persons and detail the reasons why they support a given candidate. We only focus on endorsements published before the first round. Voting decisions as stated between the first and second round of elections might be driven by the willingness to defeat the opponent (especially when a radical right politician qualified in the second round, as in 2002 and 2017), rather than real endorsement of the candidate’s platform and values.

Combining party meetings, think tanks and endorsements data We finally combine the data described above in a probabilistic model. Using the Chapel Hill Expert Survey, we place each political family on a left right scale, ranging from 0 to 100. Each behavior (summer meetings attendance, think tank participation, and endorsement) is mapped to a political family, and is attributed a left right score between 0 and 100. For each behavior, we extend it temporally with a decay using an asymmetric Gaussian distribution: its intensity decays very fast before the event, and slowly after. When the intensity slips below a threshold, we consider the individual in unaligned.

When an individual has taken part in events matched to distinct families (for example, attended summer meetings of the Green party, and contributed to a socialist think tank), we compute a decay-weighted average of her left-right placement. In the end, we discretize this left right placement using the midpoint between political families. For example, if in a given month, an individual has a left-right placement of 40, then we consider she belongs to the party whose left-right placement is the closest.

Figure 2.13 illustrates the procedure for Daniel Cohn-Bendit, a Green politician who was a member of the European Parliament from 1994 to 2014. The x-axis represents time, the y-axis the left-right scale, from 0 to 100. Yellow lines correspond to the midpoint between

political families' left-right placement as computed from the Chapel Hill Expert Survey. They define each political family's political space over time. Blue lines are contour lines of the asymmetric Gaussian distributions. Red dots represent the monthly weighted average of the political placement on the left-right scale, and green dots represent the variance of the placement.

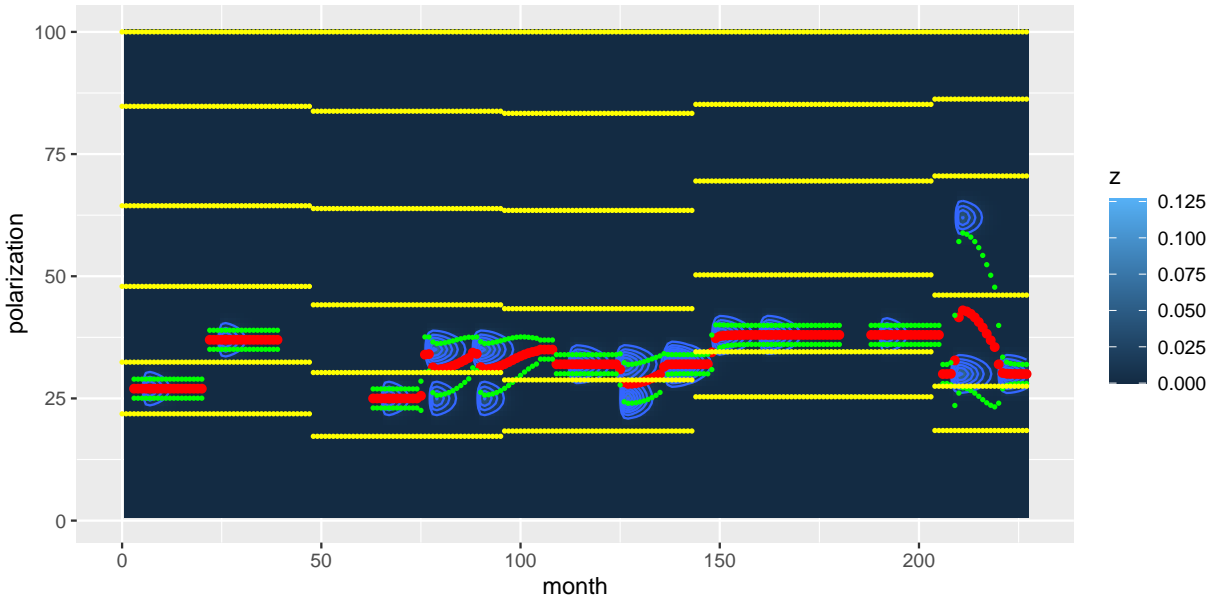


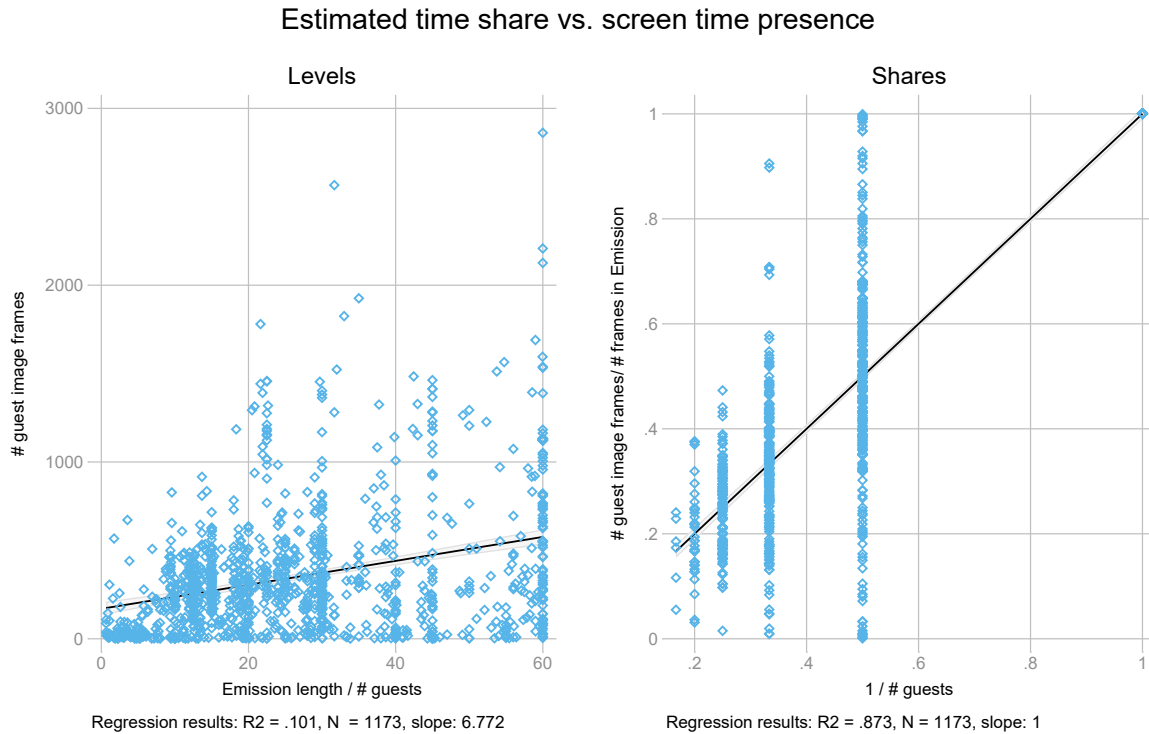
Figure 2.13: Political classification using endorsements, party events and think tanks

Precision of time share measure To check how much our time share measure - emission length divided by the number of guests - captures actual variation in time shares, we rely on a subset of shows for which we have data from a facial recognition algorithm provided by Petit et al. (2021). They develop a tool to recognise image frames of guests on television, allowing us to proxy the actual screen time presence of a person in a show with the number of recognised frames. This measure itself is a proxy for actual *speaking* time shares. First, one frame can correspond to 1-3 seconds since they are cut as a function of changes in the image statics on screen. Second, screen time presence of a face of a person does not always coincide with speaking, as sometimes people's faces are superimposed while another person is speaking. This measure is still very granular on the show level. We restrict the analysis to shows for which all guests are in their dictionary and can be detected, leaving us with a sample of 1177 shows.

Figure 2.14 shows the correlation between the actual screen time presence as proxied by recognised image frames with our naive measure of speaking time. The left panel compares imputed levels of speaking time. In levels, the naive measure explains 10 % of variation in

image frames, and the slope suggests that one additional minute in our measure translates into 6 more image frames of a person in a show ($\tilde{18}$ seconds).

The right panel correlates *relative* screen time presence of a guests with our outcome, the native relative speaking time share of a guest in an emission. Our measure explains 87.3 % of the observed variation in screen time presence with a slope of 1, making us confident that our measure proxies screen time shares sufficiently well.



1 frame ~ 1-3 seconds. Conditional on guests being in facial dictionary.

Figure 2.14: Political classification using endorsements, party events and think tanks

Other data on guests

In addition to political classification, we use several data sources to describe guests demographic and professional characteristics.

INA data We first use INA data which, for each individual, provide a short description of the guest profession, her gender, her year of birth, and her country. For gender, INA data indicate whether the person is male or female. Table ?? plots the share of women across seasons, for all appearances, and only for appearances that we classify politically. It has increased between 2002 and 2020, from 18% to 27%.

INA data also provide a short description of guests' age and profession. This information is rather general ("politician" rather than "mayor of Paris" for instance) and not time-varying. If an individual however had several professions during her career, both are generally detailed. For example David Douillet, a judo gold medalist who later became Minister of Sports, has "judoka, politician" listed as profession. We then classify professions into groups by searching keywords in the guest description. A given guest can fall in multiple categories if her description contains keywords corresponding to distinct categories. The categories are the following:

- **Politicians:** "homme politique," "femme politique," and "personnalité politique."
- **Activist:** union leader, think tank director or member, foundation director, NGO director, etc.
- **Media:** any profession related to the media and publishing sector.
 - **Journalist:** journalist, reporter, editor, newspaper director, etc.
 - **Director and producer:** director, producer, assistant producer, film editor ("monteur"), audiovisual technician, etc.
 - **Host**
 - **Opinion:** columnist, critic, etc.
 - **Writer:** writer, novelist, poet, essayist, etc.
 - **Director:** publication director, program director, production director, channel director, etc.
- **Business and finance:** businessman, CEO, market analyst, banker, asset manager, etc.
- **Administration:** senior civil servant ("haut fonctionnaire"), supreme court, diplomat, military officer, judge, magistrate, etc.
- **Entertainment.**
 - **Cinema and theater:** actor, actress, stage director, screenwriter, etc.
 - **Music:** singer, musician, songwriter, opera singer, DJ, etc.
 - **Dance:** dancer, choreographer, etc.
 - **Pictorial arts:** painter, photographer, etc.
 - **Festival:** festival director, etc.
 - **Other:** clown, magician, model, Miss France, etc.
- **Sports.**
 - Football
 - Rugby
 - Tennis
 - Cycling

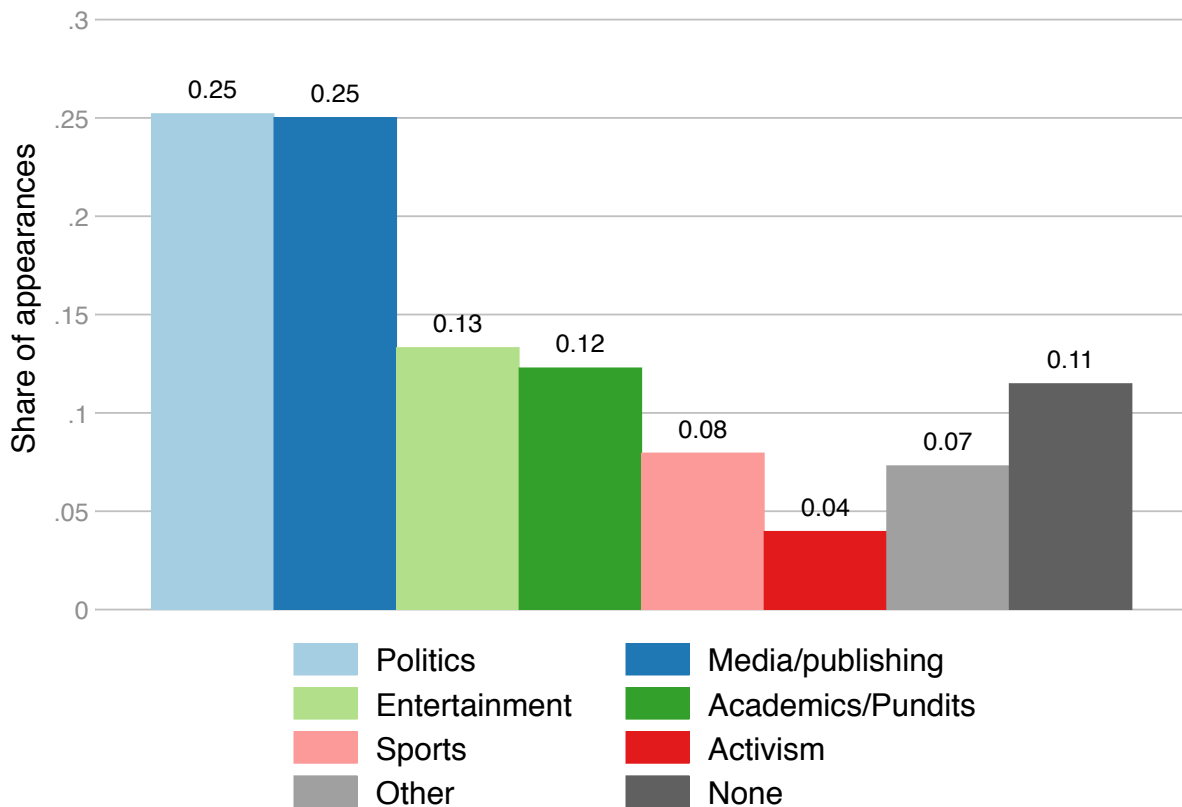
- Etc.
- **Pundits.** It should be noted that people classified with these key words re far from all being academics. Some of them hold PhDs and now work in consulting or think tanks, others for example are described as economist because they have written books about economic issues.
 - **Social sciences and humanities:** economist, sociologist, political scientist, geopolitics specialist, demographer, philosopher, historian, archaeologist, etc.
 - **Hard sciences and medicine:** medical doctor, surgeon, climatologist, physicist, chemist, etc.
- **Polls and communication:** opinion polls, communication consultant, publicist, etc.

We have data on profession for 88% of appearances, and 81% of guests are classified is at least one category. Figure 2.15 depicts the appearance share of guests in each category.

Wikidata We also use Wikidata to collect data on people in the INA data set (journalists and guests). We collect data on: date of birth, place of birth, education, profession, employers and citizenship. The procedure is as follows: for each name in our data set (first name and last name), we search Wikidata and get the top 10 results, of which we discard those that are not an instance of “human” (i.e. a book, a place, etc.). For each name, we get between 0 and 10 results.

We then merge each Wikidata search result with the INA dictionary of name (*thesaurus*) and assess match quality. To do so, we create a score. A match’s score is obtained as follows:

- Whether the first name and last name match. While the first Wikidata result might refer to the right person, the second might refer to a sibling or parent. There might be false negatives if the person uses a different name (Léa Salamé vs. Hala Salamé), or only their first name (Arthur, Magloire).
- Whether the birth year matches. Unfortunately, birth year is often missing in INA data.
- Whether the birth year is plausible. We give a higher score to Wikidata matches whose birth year is in the top 90% of the distribution (born after 1937). It helps discard people who have common names and have a homonym in history (military officer in the 19th century, etc.)
- Whether the gender matches.
- Whether the country of citizenship matches.
- Whether there is overlap between, on the one hand Wikidata label and profession strings, and profession in INA data.



Notes: The figure plots the profession of the invited guests as a share of the appearances. The data covers the time period ranging from January 1st 2002 to December 31st 2020. It includes the following 14 television channels: TF1, France 2, France 3, Canal+, France 5, M6, ARTE, C8/D8, TMC, France 4, BFM TV, I-Télé/CNews, LCI, LCP/Public Sénat, and 8 radio stations: France Inter, France Info, France Culture, and RTL, RMC, Europe 1, Radio Classique, and BFM Business.

Figure 2.15: Guests of the shows: Profession, 2002-2020

For each name, we keep the Wikidata match that has the best score. In case of tie, we keep the highest ranked in the Wikidata search results (likely more famous). We then drop all search results in the bottom decile, as the low score often indicates that most data fields were missing, and assessing the match quality is impossible. Of the about 40,000 with at least 10 appearances that were searched in Wikidata, we find 21,048 valid matches, a fraction of them being journalists.

Data on journalists

INA data, as for guests, also provide information on journalists characteristics (gender, year of birth, country). Similarly, we collect data from Wikidata and match it to our data set for both guests and journalists. Because, in the case of journalists, we are particularly interested

in their work experience, we additionally collect data from *Les Biographies*.

Les Biographies Data on journalists come from the online version of a publication, akin to *Who's Who*, which contains concise biographical information on notable people in France. Each notice generally indicates the date and place of birth, the education and professional career (position, firm, start and end date) of the considered individual.

We focus on hosts and journalists, and for this reason we only retrieve notices of people related to the media industry. To do so, we use a key word search on the *Les Biographies* website using a premium account. The key words refer to channel names or media groups. They are the following: Arte, BFM, BFMTV, C8, Canal +, CNews, Europe 1, France 2, France 3, France 4, France 5, France Bleu, France Classique, France Culture, France Info, France Inter, France Télévision, I-télé, Groupe Les Echos, Groupe RTL, Groupe TF1, Groupe M6, Lagardère Active, LCI, M6, Mediawan, NextRadioTV, Radio France, RMC, RMC Sport, RTL, TF1, TMC, Vivendi, and W9. We collect the notice content of any person whose description contains at least one of these tokens.

We then focus on the career of these people. For each job entry, we disentangled the firm from the job title, and the classified job titles into several categories.

- Journalists and hosts. This category is broadly defined and refers to all positions related to the media content: journalist, reporter, host, editor, columnist, etc.
- Participants. This category gathers people who regularly participate in shows, typically talk shows or debate shows.
- Top executives. It includes people that have a C-level position in a media outlet (CEO, CFO, etc.). We also create a dummy variables for whether the person was the CEO.
- Others. It generally includes people whose job is neither C-level, nor directly related to content creation, like for instance head of marketing, head of advertising, etc.

As a result, for each person that has a notice on *Les Biographies*, we have his or her professional time line, with the duration of each position, the firm, and the job type. Of course, young hosts or journalists, that rarely appear on screen are less likely to have a *Les Biographies* notice. Overall, we collect data on 5,001 individuals.

Additional details on the data construction

We winsorize show length to the 99th percentile (180 minutes) to avoid time shares to be driven by outlying shows whose length may be mis-measured.

A.2 The French media and political landscape: Detailed Information

As of today in Metropolitan France, there are 30 national digital terrestrial television channels: 7 public channels, 18 free national private channels, and 5 national pay channels. Table 2.6 describes these channels.

Table 2.6: French national digital terrestrial television channels

#	Channel	Sample	Free/Pay	Creation	Ownership		Audience share		
					2002 (or inception)	2020	2002	2007	2020
1	TF1	Yes	Free	1935	Bouygues	Bouygues	32.7	30.7	19.2
2	France 2	Yes	Free	1964	Public	Public	20.8	18.1	14.1
3	France 3	Yes	Free	1972	Public	Public	16.4	14.1	9.4
4	Canal+	Yes	Mixed	1984	Canal Plus	Bolloré	3.7	3.4	1.2
5	France 5	Yes	Free	1986	Public	Public	2.3	3.3	3.5
6	M6	Yes	Free	1987	Bertelsmann	Bertelsmann	13.2	11.5	9.0
7	Arte	Yes	Free	1992	Public	Public	1.6	1.8	2.9
8	C8	Yes	Free	2005	Bolloré	Bolloré	–	0.2	2.6
9	W9		Free	2009	Bertelsmann	Bertelsmann	–	0.9	2.6
10	TMC	Yes	Free	1954	AB & Bouygues	Bouygues	–	1.2	3.0
11	TFX		Free	2005	AB	Bouygues	–	0.6	1.6
12	NRJ 12		Free	2005	NRJ	NRJ	–	0.4	1.3
13	LCP	Yes	Free	2000	Public	Public	–	–	–
14	France 4	Yes	Free	2005	Public	Public	–	0.4	1.2
15	BFM TV	Yes	Free	2005	Weill	Altice	–	0.2	2.9
16	CNews	Yes	Free	1999	Canal Plus	Bolloré	–	0.3	1.4
17	CStar		Free	2005	Lagardère	Bolloré	–	0.4	1.1
18	Gulli		Free	2005	Lagardère & Public	Bertelsmann	–	0.8	1.3
20	TF1 Séries Films		Free	2012	Bouygues	Bouygues	–	–	1.8
21	L'Equipe		Free	1998	Amaury	Amaury	–	–	1.3
22	6ter		Free	2012	Bertelsmann	Bertelsmann	–	–	1.7
23	RMC Story		Free	2012	Diversite TV	Altice	–	–	1.5
24	RMC Découverte		Free	2012	Weill	Altice	–	–	2.3
25	Cherie 25		Free	2012	NRJ Group	NRJ Group	–	–	1.1
26	LCI	Yes	Free	1994	Bouygues	Bouygues	–	–	1.2
27	Franceinfo		Free	2016	Public	Public	–	–	0.7
41	Paris Première		Pay	1986	Paris & L. des eaux	Bertelsmann	–	–	–
42	Canal+ Cinéma		Pay	1996	Canal Plus	Bolloré	–	–	–
43	Canal+ Sport		Pay	1998	Canal Plus	Bolloré	–	–	–
	Planète+		Pay	1988	Canal Plus	Bolloré	–	–	–
Total sample viewership							90.7	85.2	71.6

Notes: Audience data from Mediametrie. Data is missing either when the channel did not exist yet, or when Mediametrie reports did not display the information (mostly for smaller channels).

Our dataset covers the period 2007-2018, and 23 different television and radio channels that we describe in turn in this section. We also provide in this section to give a sense of the relative importance of these different channels aggregate figures on their audience in March 2021.

Table 2.7: French radio stations, excluding music only and local stations

Station	Sample	Creation	Ownership		Audience share	
			2002	2020	2003	2020
France Inter	Yes	1947	Public	Public	9.8	14.7
France Info	Yes	1947	Public	Public	4.9	4.7
France Bleu		1947	Public	Public	5.7	5.8
France Culture	Yes	1947	Public	Public	–	2.7
RTL	Yes	1933	Bertelsmann	Bertelsmann	11.5	12.6
Europe 1	Yes	1955	Lagardère	Lagardère	7.8	3.9
RMC	Yes	1943	Weill	Altice	2.8	5.3
Radio Classique		1983	LVMH	LVMH	–	2.4
BFM Business		1992	Altice	Altice	–	–
Audience share of non-local, non-music only stations					–	54.9
Audience share of our sample					36.8	46.3

Notes: Audience data from Mediametrie.

Public broadcasters

In France, there are 9 public television stations: France 2, France 3, France 4, France 5, France Ô, Arte, and LCP-Public Sénat. Our dataset includes information for the FIVE main channels: France 2, France 3, France 4, France 5, and Arte. The audience share of France 2 in March 2021 was 14.4%, the one of France 3 9.1%, and the one of France 4 0.9%.³⁹

We also have information for 4 public radio channels: France Bleu, France Culture, France Info and France Inter, which are the four main public radio stations with news programs. The audience share of France Inter in November-December 2020 was 14.7%, the one of France Info 4.7%, and the one of France Bleu in 5.8%. (The remaining channels are France Musique, Fip, and the Mouv’.)

Appointment of public media groups directors The French public broadcasting service is made of *France Télévision* for television on the one hand (i.e. in our dataset France 2, France 3, France 4, France 5, and franceinfo TV), and *Radio France* for radio on the other hand (France Culture, France Info, and France Inter). As of today, the heads of *France Télévisions* and of *Radio France* are appointed by the ARCOM. However, this has not always been the case during our period of interest. Indeed, between 2009 and 2013, a law gave the President of the Republic the task of appointing the president of *France Télévisions*,

³⁹In comparison, the audience of France 5 was 3.3%; the one of Arte 2.9%.

after receiving the assent of the ARCOM. This law was strongly criticized for it places the nominally independent public sector media under direct state control. In 2013, this provision was reversed and the authority of the ARCOM to name the director of *France Télévisions* restored (see e.g. Benson et al., 2017).

Private broadcasters

Regarding private television, our dataset covers all the channels which have at least some news programs, i.e. C8/D8, Canal +, M6, TF1, and TMC.

It excludes those channels whose focus is only on entertainment: CStar that devotes more than 75% of its airtime to music; Gulli, aimed primarily at children aged 4 to 14; NRJ TV mainly devoted to music and culture; TFX; W9 whose airtime is mostly devoted to music; TF1 Séries Films that is dedicated to audiovisual fiction and cinematographic works; L'Equipe that is devoted to sport; 6ter; RMC Story; RM Découverte, a documentary channel dedicated to discovery and knowledge.; and Chérie 25 focused on magazines and documentaries.⁴⁰

Our dataset also includes the 3 24-hour news channels: BFM TV, CNews/I-Télé, LCI, as well as 4 private radio channels broadcasting news programs: Europe 1, RMC, RTL, and Radio Classique. Europe 1, RMC, and RTL are the three private generalist radio services in France.

These different television channels and radio stations have changed hands a number of times during our period of interest. For the sake of the presentation here, we regroup them depending on their shareholder.

Groupe TF1. TF1, which was a public channel at the time of its creation, became private in 1987 after its acquisition by Bouygues (an industrial group specialized in construction, real estate development, telecommunications, and transportation). As of today, Bouygues owns 43.90% of the channels' capital, the rest of the capital been divided as follows: 28,80% floating stock abroad, 20,00% floating stock in France, and 7,30% for TF1 employees (TF1 shares are listed on the Premier Marché of the Paris Stock Exchange – Euroclear code 005490). The audience share of TF1 in March 2021 was 20.5%.

LCI was launched in 1994 on behalf of the media group TF1 as a pay television channel. It became a free channel in 2016. It is still owned by the “Groupe TF1”. The audience share

⁴⁰Furthermore, these television stations tend to have a rather low audience: 2.5% for W9; 3% for TMC; 1.6% for TFX; 1.1% for NRJ12; 1.1% for CStart; 1.1% for Gulli; 1.6% for TF1 Séries Films; 1.5% for L'Equipe; 1.5% for 6Ter; 1.4% for RMC Story; 2% for RMC Découverte; 1.2% for Chérie 25.

of M6 in March 2021 was 1.1%

The Groupe TF1 also owns the channel **TMC**. Launched in 1954, TMC is selected in 2003 by the CSA to be broadcast free-to-air on preselection No. 10 of the free TNT. This allowed it to obtain maximum coverage of the French territory as soon as it was launched on TNT in 2005. In 2005, the Groupe TF1, together with the Groupe AB (a business group in the field of broadcasting), bought the capital shares owned by Pathé in the channel (80% of the capital, the remaining 20% been owned by the Principality of Monaco. In 2010, the Groupe TF1 bought the shares owned by the Groupe AB (a transaction allowed by the CSA). In 2016, the Groupe TF1 finally bought the capital shares owned by the Principality of Monaco and became the unique shareholder of TMC.

Groupe M6. **M6** (Métropole Télévision) was launched in 1987. 48.26% of its capital is own by the "SA Immobilière Bayard d'Antin", i.e. RTL Group (Bertelsmann). The rest of the capital is divided as follows: 7,24% is owned by the "Compagnie nationale à portefeuille" (a family-owned professional shareholder), and 43.35% corresponds to floating stock. The audience share of M6 in March 2021 was 9.5%

RTL Group (Bertelsmann) also owns the radio station **RTL**.⁴¹ The audience share of RTL in November-December 2020 was 12.6%.

NextRadioTV. NextRadioTV, founded in 2000 by Alain Weill, is a company consisting of BFM TV and RMC. In 2015, Altice (a multinational telecommunications corporation founded and headed by Patrick Drahi, and the parent company of SFR) bought 49% of NextRadioTV, 51% of the capital been still held by Alain Weill.⁴² In 2016, SFR Group / Altice took exclusive control of Groupe News Participations, which holds 99.7% of NextRadioTV's capital (a transaction permitted in 2017 by the competition authority⁴³ and approved in 2018 by the CSA).

BFM TV was launched in 2005 by NextRadioTV. As of today, 100% of the capital of BFM TV is owned by NextRadioTV whose 99.7% of the capital is owned directly or indirectly by the company "Groupe News Participations" (GNP), 99.7% of the capital of the latter being

⁴¹Founded in 1933 as Radio Luxembourg, the station's name was changed to RTL in 1966. It broadcast from outside France until 1981, because only public stations had been allowed until then. In 1981, privately run radio stations were allowed to broadcast in France and RTL has since then broadcast in France.

⁴²As part of this operation, two new companies were created: one the one hand, News Participation, which owns NextRadioTV – 51% controlled by Alain Weill and 49% by Altice –, and on the other hand, Altice content, whose goal is to invest in media companies.

⁴³décision n° 17-DCC-76 en date du 13 juin 2017.

owned by "Altice Content Luxembourg", i.e. SFR (Patrick Drahi). The audience share of BFM TV in March 2021 was 2.8%

NextRadioTV also fully owns the private radio station **RMC**. RMC, founded in 1943, was bought in 2001 by NextRadioTV. The audience share of RMC in November-December 2020 was 6.1%.

Groupe Canal Plus. As of today, the "Groupe Canal Plus" is made of the following television channels: Canal+, C8, and CNews.⁴⁴ A limited company, the "Groupe Canal Plus" is itself 100% owned by Vivendi. Since 2015, the "Groupe Bolloré" (with Vincent Bolloré) is the main shareholder of Vivendi with 26.28% of the capital (all the other shareholders own less than 5% of the capital).

C8 (formerly Direct 8 – D8) was launched in 2005 by Vincent Bolloré⁴⁵, and bought by the "Groupe Canal Plus" in 2011. As of today, 100% of the capital of C8 is owned by the "Groupe Canal Plus". The audience share of C8 in March 2021 was 2.7%.

CNews (formerly I-Télé), a 24-hour news channel, was launched in 1999 by the "Groupe Canal Plus". Initially a subscription-based television services, it is transformed into a free channel as of its arrival on French digital terrestrial television in October 2005. 99.8% of CNews is owned by the "Groupe Canal Plus SA" (the remaining 0.20% been owned by Canal+ Finance SA). The audience share of France 2 in March 2021 was 1.9%.

Canal+ was launched in 1984 as the first French premium television (and the first private national television company.⁴⁶) At the time of its launch, its main shareholder was the "Groupe Havas", a publicly-traded company whose main shareholder was the State itself. The capital share owned by Havas – the company was privatized in 1987 – in Canal Plus progressively decreased, and in 1987 the channel was listed on the stock exchange. At the time, its two main shareholders were Havas and the Compagnie Générale des Eaux.⁴⁷

⁴⁴As well as CStar that is not included in our sample given it is not a generalist channel.

⁴⁵The official creation of the channel took place in 2001, with a number of tests. It was officially launched in 2005 with the "Télévision numérique terrestre" – digital terrestrial television platform.

⁴⁶In 1984, the government initially granted Canal-Plus a public service concession for twelve years. The concession was renewed in 1994.

⁴⁷More precisely, in 1984, more than 60 percent of the capital of the channel was held by state-controlled shareholders: Havas (42.13%) and nationalized banks (the Société Générale, the Banque Nationale de Paris (BNP), the Crédit Lyonnais, the Crédit Commercial de France (CCF), and the Banque Régionale d'Escompte et de Dépôt (Bred), 18.18 % in all).The other (private) shareholders were the Compagnie générale des eaux, L'Oréal, the Garantie Mutuelle des Fonctionnaires (GMF) (5%) and the regional daily newspaper *Ouest-France* (1.66%). Agence Havas, while remaining the largest shareholder in Canal Plus, held only 25% of its capital at the end of March 1986, through a number of capital increases and the sale of 12.5% of its shares. Furthermore, thanks to a capital increase, Perrier became a shareholder in 1986 with 5% of the capital, as well as Gilbert Gross's SGGMD (5%), the British group Granada (3%), and the Compagnie Financière

The audience share of Canal+ in March 2021 was 1.1% (but remind that Canal+ is a premium television channel).

Europe 1 Europe 1 is a privately owned radio station created in 1955, owned and operated by Lagarère since 1974 (Lagarère SCA at the beginning of the period, Lagarère Active as of today). The audience share of Europe 1 in November-December 2020 was 3.9%.

Radio Classique Launched in 1983 by Christian Pellerin, Radio Classique broadcast mainly classical music, but also segments of economic and political news. In 1986, the station was 25% owned by RTL and 75% by the real estate company Lucia (a land holding company created by Christian Pellerin). In 1992, Pellerin sold Radio Classique to Sagem, a group specialized in professional and military electronics. In 1999, Desfossés International, a subsidiary of Bernard Arnault's group, LVMH (and media division of LVMH), bought 100% of the capital of Radio Classique. In 2000, Desfossés International became DI Group.⁴⁸ In 2008, as a result of the buyout of the economic daily *Les Echos* Bernard Arnault, DI Group is renamed "Groupe les Echos" (with Nicolas Beytout as the CEO).

Note that all the private television channels have to establish a convention with the CSA.

Changes in media ownership

Bouygues Group buys AB Group's shares of TMC in 2009. In 2005, TMC is sold to Bouygues Group and AB Group, each of them holding 40% of TMC. In December 2006, Bouygues bought 33.5% of the shares of AB Group. A clause in the 2006 agreement ensured that TF1 could not buy TMC. This clause expired in April 2009. In May 2009, TF1 announces that it is negotiating with AB group to buy its 40% of TMC. In January 2010, the competition authority approves the transaction. TF1, with 80% of the shares, has control over TMC.⁴⁹

Saint-Germain (2%), a holding company. In March 1986, the Compagnie Générale des Eaux (CGE) was still the leading private partner of the channel with 15.65% of its capital. It was followed by L'Oréal (10.41%), the Société Générale (10%), the Garantie Mutuelle des Fonctionnaires (GMF) (5.21%) and a group of banks (12.5%). The balance is held by various mutual funds and regional press groups associated with the creation of Canal Plus from the outset. In 1987, the CGE has strengthened its position in the capital of Canal Plus, increasing its capital share from 15.65% to 21.49% (through the purchase of the 5.21% of the shares held by the GMF and the acquisition of the shares (0.63%) of the Bred). At the time Canal Plus went public (in November 1987), its main shareholder were Havas (24.23%), CGE (20.72%), L'Oréal (7.7%), Société Générale (8.08%), CCF (6.82%), and Perrier (5%).

⁴⁸Bernard Arnault bought Desfossés International (that edited the financial dailies *La Tribune* and *l'Agefi*) in 1994.

⁴⁹<https://www.lesechos.fr/2010/06/reperes-le-rachat-de-tmc-et-nt1-par-tf1-440812>

Bolloré sells Direct 8 to the Canal Plus Group in 2011. In September 2011, Canal Plus Group (owned by Vivendi) announces the acquisition of 60% of the television branch of the Bolloré Group, which owns Direct 8 (which will later be named D8 and C8). The Bolloré Group is paid in Vivendi shares. In exchange for the 60% of its television channels, the Bolloré television obtained 1.7% of the Vivendi Group, which owns of the Canal Plus Group. As a result the Bolloré Group owns 4.41% of Vivendi shares. The transaction is approved by the CSA and the Competition Agency in September 2012. Direct 8 is renamed D8.⁵⁰

Bolloré takes over the Canal Plus Group in 2015. At the beginning of 2015, the Bolloré Group had 5.1% of the shares in the Vivendi Group, a publicly traded company that owns the Canal Plus channels (Canal +, D8 and I-Télé). Vincent Bolloré, at the head of the Bolloré Group had been a chairman of the surveillance committee of Vivendi since June 2014. On March 26th 2015, the Bolloré Group registered more than 10% of the shares in Vivendi. In April 2015, it had raised its equity up to 14.4%. Mid-April, Vincent Bolloré obtained during the general meeting of shareholders with more than two thirds of votes that a French law doubling the vote shares of long-term owners applies.⁵¹ In exchange for this approval, he had promised extra dividends. As a result of the vote, the Bolloré Group obtained about 26% of the vote shares, making it the reference shareholder. In July 2015, he named Maxime Saada CEO of the Canal Plus Group.⁵²

Altice gradually takes control of NextRadioTV from 2015. NextRadioTV is publicly-traded group owning the television channels BFM TV, RMC Sport and RMC Story as well as the radio stations RMC and BFM Radio. It was created by Alain Weill in 2005, who owned 37.8% of its capital and 48.6% of the vote share at the beginning of 2015. In July 2015, he announces a “strategic partnership” with Patrick Drahi, a long-standing business partner. Patrick Drahi owns Altice, a group that includes SFR (a mobile telecommunica-

⁵⁰https://www.challenges.fr/high-tech/bollore-a-4-41-de-vivendi-apres-la-vente-de-direct-8-a-canal_260850, <https://investir.lesechos.fr/actions/actualites/canal-achete-60-de-direct-8-et-direct-star-a-bollore-370842.php>, <https://www.capital.fr/entreprises-marches/nouveau-feu-vert-de-la-concurrence-au-rachat-de-d8-par-canal-922262>

⁵¹This law, also named Loi Florange, voted in 2014, aimed at favoring long-term firm ownership rather than speculation by opportunistic shareholders.

⁵²<https://www.bollore.com/bollo-content/uploads/2018/01/03-26-15-bollore-vivendi.pdf>, <https://www.bollore.com/bollo-content/uploads/2018/12/bollore-rs-2015.pdf>, <https://www.lesechos.fr/2015/04/bollore-continue-de-monter-en-puissance-dans-le-capital-de-vivendi-247478>, <https://www.lesechos.fr/2015/04/chez-vivendi-vincent-bollore-paracheve-sa-prise-de-pouvoir-258929>, <https://www.lopinion.fr/edition/economie/comment-vincent-bollore-prend-contrôle-vivendi-petite-porte-105199>, https://www.challenges.fr/entreprise/vivendi-cette-ag-qui-pourrait-porter-bollore-au-pouvoir_67801.

tion company), Numericable (a cable operator and telecommunication company) and Altice Content (Libération, L'Express, Strategies, Mieux Vivre Votre Argent, L'Expansion). They create a holding named News Participation, controlled at 51% by Alain Weill and at 49% by Altice Contents. This holding will become the new owner of NextRadioTV. In exchange, Alain Weill obtains 24% of Altice Content. In February 2016, News Participation owns more than 97% of NextRadioTV. In June 2017, the Competition Authority approves the takeover, the CSA in April 2018. In November 2017, Alain Weill becomes the CEO of Altice France, which includes Altice Content and, therefore, NextRadioTV.⁵³ As a result, although NextRadioTV is now owned by Altice (Drahi), its CEO, Alain Weill, has remained in control all along, as he now the CEO of the Altice branch that owns NextRadioTV.

Pluralism and equal-time rules

The *Conseil Constitutionnel* – the French equivalent of the US Supreme Court – in a 1990 decision states that pluralism “is one of the conditions for democracy.”⁵⁴ A 1986 law explains that media outlets’ freedom of communication to the public should be reconciled with pluralism. Outside of electoral campaigns, the *Autorité de régulation de la communication audiovisuelle et numérique* (ARCOM) requires television and radio outlets to represent a plurality of viewpoints in their programs. In practice, the ARCOM guidelines are that a third of the political speaking time relative to the national political debate be devoted to the president and the government. The remaining two thirds should be split across political forces based on vote shares, elected officials’ count, parliamentary groups’ size, opinion polls, and political groups’ contribution to public debate. The ARCOM asks each outlet to tabulate speaking time of politicians. This is done quarterly to average out news events. All programs are taken into account since 2018, previously, only shows on news and politics where subject to this rule. Only elected politicians or party members are accounted for.

In the context of elections, the pluralism principle is replaced by an equal-time rule that is strictly enforced.

Regarding the presidential election, we need to distinguish between the so-called *intermediate period* (from the publication of candidate lists to official start date of the campaign) and the thirty-day *official campaign* itself (two weeks before the first round, then another two

⁵³<https://www.reuters.com/article/nextradiotv-altice-idFRL5N10713P20150727>, <https://www.strategies.fr/actualites/medias/1021127W/alain-weill-et-patrick-drahi-s-associent-pour-racheter-nextradio-tv.html>, https://www.lemonde.fr/economie/article/2015/07/27/le-groupe-de-patrick-drahi-se-positionne-pour-racheter-nextradiotv_4700363_3234.html, <https://www.autoritedelaconurrence.fr/fr/communiqués-de-presse/13-juin-2017-medias>

⁵⁴CC, 86-217 DC, 18 septembre 1986, cons. 11

between the first and second rounds). The official campaign begins on the second Monday preceding the first round of voting and comes to a halt at midnight on the eve of the ballot. It then resumes on the day when the two front-runners are announced and comes to a final halt at midnight on the eve of the second round. Today, the principle of “equitable” speaking time prevails during the intermediate period.⁵⁵ Under the supervision of the ARCOM, the speaking time of the various parties during the “intermediate” campaign must reflect the extent to which they are representative of the French political landscape, as well as their capacity to demonstrate their intention to run candidates. There are three criteria of a party’s “representativeness”: its results in the most recent elections; the number and position of elected officials that it claims to have; and the evidence of opinion polls.⁵⁶ During the official campaign, and equal-time rule applies. Each candidate should be granted the same speaking time.

As to parliamentary elections, the French electoral code states that – for the broadcasting of video clips – the parties with formally constituted groups in the National Assembly shall together have a total of three hours for the first round, while parties without such groups may each have seven minutes’ broadcasting time provided they can show that at least seventy-five candidates are running in their name.

Political landscape

There are many political parties in France, ranging from far left to far right. The political landscape has historically been dominated by two parties: the socialist party on the left (PS), and a conservative party (RPR, then UMP and now *Républicains*). A liberal party (REM, now *Renaissance*) emerged in in 2016 and won both presidential and house elections in 2017. There are many other smaller parties – communist parties, green parties, centrist parties, anti-immigration parties, etc. – whose names changed and that merged or split over time. For this reason, we aggregate parties in six political groups using the Chapel Hill Expert Survey party classification (Bakker et al. 2015). They define several so-called families: radical left, green, socialist (left), liberal, conservative (right) and radical right.

⁵⁵The organic law of April 25, 2016, updated the rules governing presidential elections, including the allocation of speaking time. Previously, strict equality had been stipulated for candidates and their supporters throughout the “intermediate” period, which was naturally advantageous to the “smallest” campaigns. (Note, however, that this strict equality related only to speaking time, not to total airtime, and that the latter included TV and radio editorial material on candidates and their supporters.) On the rules governing pluralism during and outside election periods, see the information available on the CSA website, <https://www.csa.fr>.

⁵⁶See the CSA recommendation no. 2016-2 of September 7, 2016 to the radio and television services for the presidential elections: <https://www.legifrance.gouv.fr/affichTexte.do?cidTexte=JORFTEXT000033104095&categorieLien=id>.

Table 2.8 reports the main French parties, along with their Chapel Hill family, their general left-right score (averaged over time), their economic left-right score and their social left-right score. Parties in bold are parties that were in power over the period we study.

We sometimes aggregate political groups in more aggregated groups. In this case, we combine radical left, green and socialist parties into a ‘left-wing parties’ group. Similarly, we group conservative and radical right parties in a ‘right-wing parties’ group.

Table 2.8: Main Political Parties

Party	Family	L-R general	L-R economics	L-R social
Parti Communiste Francais	Radical left	1.1	1.1	3.8
La France Insoumise	Radical Left	1.7	1.1	2.4
Europe Ecologie-Les Verts	Greens	2.5	1.9	1.6
Parti Socialiste	Socialists	3	3.1	2.8
Mouvement Démocrate	Liberal	6.1	6.2	4.5
La République En Marche	Liberal	6.3	6.3	3.2
Les Républicains	Conservatives	7.9	8.1	6.9
Debout la France	Radical Right	9	7	8.3
Front National	Radical Right	9.6	5.9	8.9

Notes: L-R values are drawn from the Chapel Hill Expert Survey and range from 0 (Left) to 10 (Right). When available, 2019 data is used, 2014 otherwise. L-R general corresponds to a general placement on a left-right scale from 0 to 10. L-R economics refers to the party’s ideological stance on economic issues such as privatization, taxes, regulation, etc. Parties on the economic left advocate for the government taking an active role in the economy, the right, a reduced role. L-R social corresponds to the variables “galton”, the party positioning on social and cultural values, from 0 - Libertarian or postmaterialists in favor of the expansions of personal freedoms to 10 - Traditional or authoritarian in favor of order, tradition and stability. The political parties in bold are those that have been in power at least once over the past two decades.

A.3 Additional tables and figures

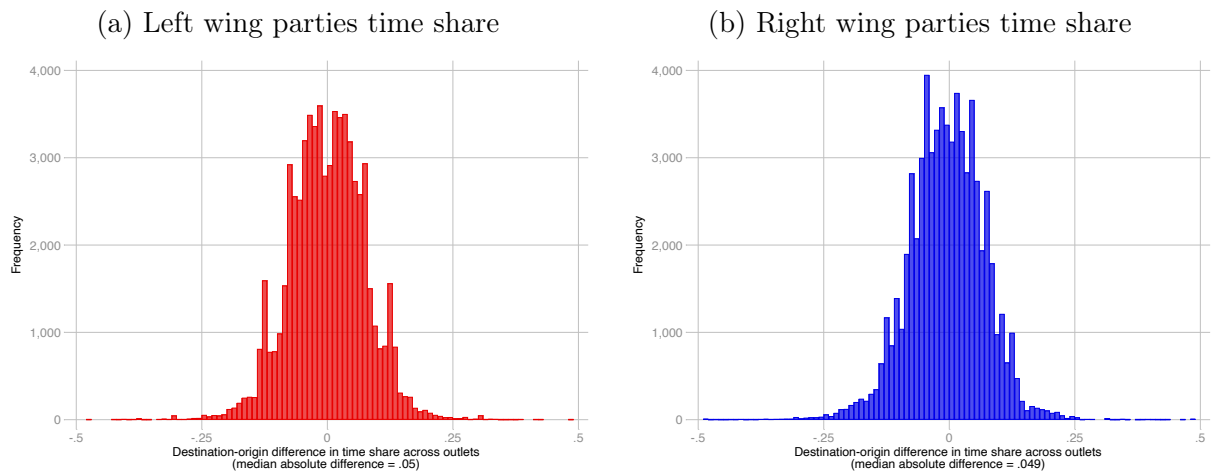
Guest classification

Table 2.9: Think tanks staff and contributors: descriptive statistics

Name	Creation	Family	Number found		Once merged with INA data	
			Staff	Contributor	Staff	Contributor
Fondation Gabriel Peri	2004	Radical left	373	814	238	447
ATTAC	1998	Radical left	1,029	2,708	807	1,857
Fondation Copernic	1998	Radical left	1,898	–	1,292	–
Les Economistes Atterres	2011	Radical left	458	210	335	188
Fondation pour la nature et l’homme	1990	Greens	1,295	–	817	–
Fondation de l’ecologie politique	2012	Greens	412	53	348	36
Fondation Jean Jaures	1992	Left	878	3,904	634	2,728
Institut Jacques Delors	1996	Left	429	1,793	334	1,098
Republique des Idées	2002	Left	123	121	95	118
Fondation Res Publica	2005	Left	590	82	479	65
Terra Nova	2008	Left	1,488	1,392	1,117	861
The Shift Project	2010	Left	287	–	110	–
Fabrique de l’Ecologie	2013	Left	386	803	307	388
Fondation Robert Schuman	1991	Liberals	518	1,568		
Institut Montaigne	2000	Liberals	632	3,678	501	2,327
Generation Libre	2013	Liberals	178	57	123	32
IFRAP	1985	Right	75	3,220	65	2,661
Fondapol	2004	Right	595	1,785	449	824
Groupement de recherches et d’études pour la civilisation européenne	1969	Radical right	58	2,140	27	1,007
Fondation Polemia pour l’identité la sécurité et les libertés européennes	2002	Radical right	–	3,723	–	1,111
Institut Thomas More	2004	Radical right	527	946	271	702
Institut des Libertés	2012	Radical right	76	1,069	50	946
		Total	12,405	30,066	8,921	18,609

Notes: This table reports the number of staff and contributors. The figures refer to the number of occurrences in our data, not the unique number of staff members or contributors. An individual who contributes once each year between 2010 and 2019 will account for 9 occurrences of contributors. The number of occurrences after the merge with INA data is smaller because some contributors and staff members never appear in the media.

Movers and stayers



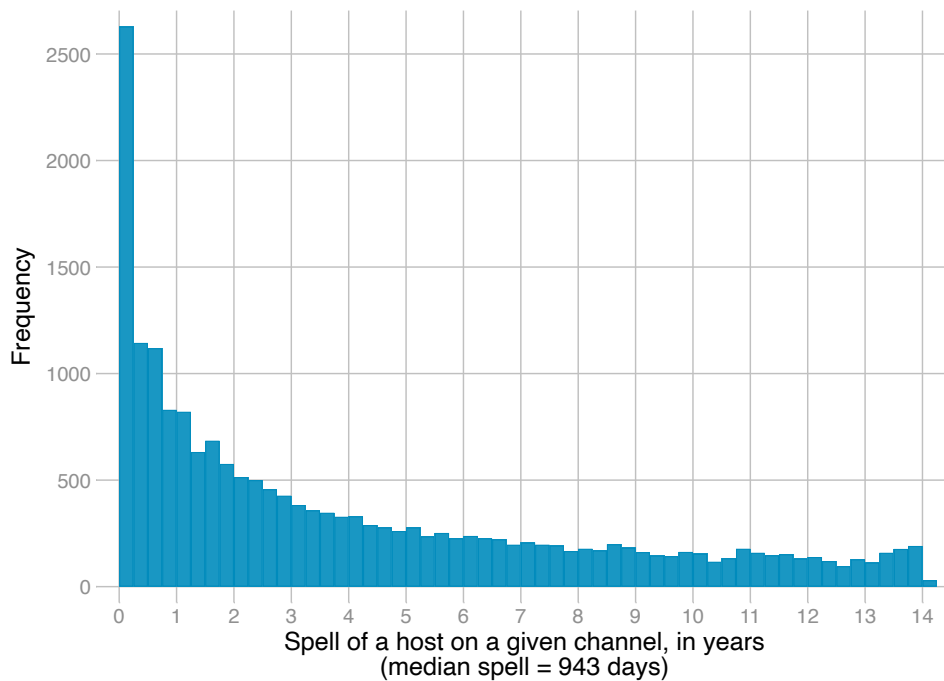
Notes: The figure plots the distribution of differences in political group time share between destination and origin outlets at the time of the move. We consider that a host moves if his next show is on a channel that is distinct from the channel of its current show. By that definition, there are 65,666 moves in the data set.

Figure 2.16: Difference in political time share between destination and origin outlets

Table 2.10: Distribution of spell length of host-channel pairs, by channel

	Host-channel pairs spell length (days)							count
	mean	sd	p5	p25	p50	p75	p95	
ARTE	1367	1430	5	224	805	2235	4417	650
BFM TV	1213	1213	8	248	770	1900	3819	684
C8/D8	720	942	7	103	314	973	2637	606
CNews/I-Télé	1067	1130	7	169	679	1524	3565	863
Canal +	1201	1264	14	224	682	1816	3943	1282
Europe 1	1324	1299	15	270	854	2131	4139	673
France 2	1765	1593	20	364	1287	2945	4870	2318
France 3	1686	1475	18	388	1300	2767	4607	2235
France 4	639	1069	1	28	189	539	3736	249
France 5	1281	1317	7	249	811	1968	4202	1605
France Culture	1779	1655	4	288	1272	3042	4938	960
France Info	1619	1486	2	273	1123	2930	4377	331
France Inter	1828	1579	18	386	1386	3080	4845	1929
LCI	1429	1452	11	251	819	2525	4286	734
LCP/PubSen	1144	1229	14	205	696	1640	3858	1082
M6	1239	1230	28	271	780	1879	3899	874
RMC	1439	1375	8	268	949	2505	4147	433
RTL	1756	1357	70	549	1561	2854	4165	399
TF1	1791	1602	35	397	1192	3154	4825	1234
TMC	631	877	7	62	344	713	2736	78
Total	1473	1448	12	266	943	2383	4501	19219

Notes: The table reports descriptive statistics on the distribution of spell lengths of each host-channel pair with the host appearing on at least two distinct days in the estimation sample. The spell length is measured as the time elapsed between the first and the last time a host is observed hosting a show with a politically classified guest on this channel. By that definition, there is a total of 19,219 host-channel pairs with at least two shows with political guests on distinct days.



Notes: The figure plots the distribution of spell length of each host-channel pairs with the host appearing on at least two distinct days in the estimation sample. The spell length is measured as the time elapsed between the first and the last time a host is observed hosting a show with a politically classified guest on this channel. By that definition, there is a total of 19,219 host-channel pairs with at least two shows with political guests on distinct days.

Figure 2.17: Distribution of spell length of host-channel pairs

Linear decomposition

Table 2.11: Linearly additive decomposition of political time share differences, excluding TMC and France 4

	Outlet-period pairs from the top and bottom			
	50%	25%	10%	5%
	All left	All left	All left	All left
<i>Difference in time share</i>				
Overall	0.126	0.197	0.270	0.316
Overall, net of time effects	0.054	0.078	0.131	0.139
Due to channels	0.046	0.067	0.120	0.128
Dues to hosts	0.008	0.011	0.011	0.011
<i>Share of difference due to</i>				
Channels (%)	85.66	85.71	91.86	91.95
Bootstrapped s.e.	4.48	4.74	4.75	6.47
Hosts (%)	14.34	14.29	8.14	8.05
Bootstrapped s.e.	4.48	4.74	4.75	6.47
	All right	All right	All right	All right
<i>Difference in time share</i>				
Overall	0.136	0.221	0.306	0.364
Overall, net of time effects	0.053	0.083	0.121	0.125
Due to channels	0.047	0.075	0.111	0.118
Dues to hosts	0.006	0.007	0.010	0.007
<i>Share of difference due to</i>				
Channels (%)	88.82	91.03	92.08	94.75
Bootstrapped s.e.	5.29	5.15	4.89	6.33
Hosts (%)	11.18	8.97	7.92	5.25
Bootstrapped s.e.	5.29	5.15	4.89	6.33

Notes: Each column reports the linear decomposition of the difference in average political time share across two sets of outlet-season pairs. Reported shares in rows 5 and 7 correspond to shares presented in Equations 2.2 and 2.3 respectively. Standard errors are the standard deviation of the corresponding shares bootstrapped with 100 replications.

Variance decomposition

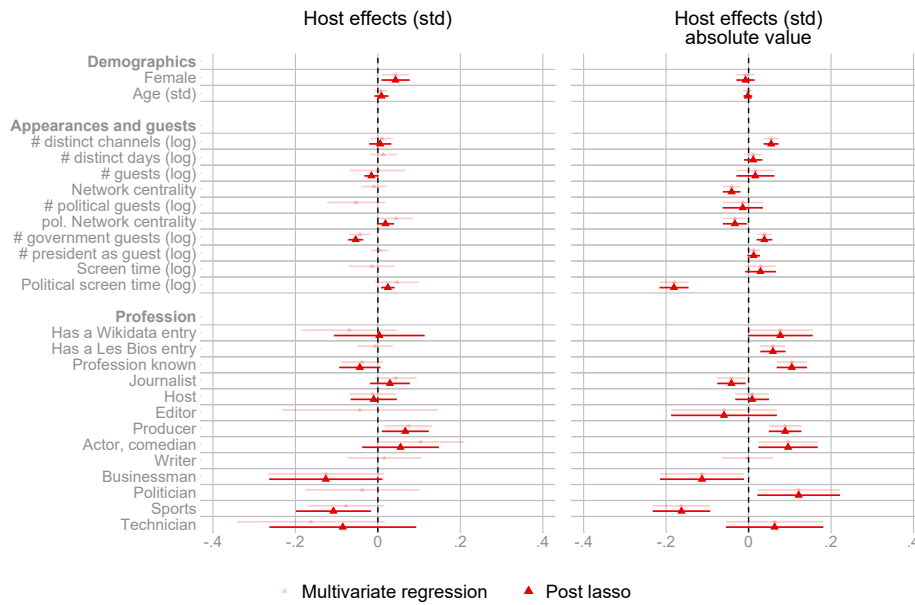
Table 2.12: Variance decomposition of political time share differences, excluding TMC and France 4

	All left	All right	Radical	Government
<i>Total variance</i>				
Variance, raw	0.0061	0.0076	0.0026	0.0049
Variance, net of time effects	0.0027	0.0028	0.0009	0.0023
<i>Channel effects</i>				
Variance	0.0022	0.0024	0.0008	0.0019
% variance, net of time effects	79.7	84.0	94.8	85.7
Bootstrapped s.e.	6.9	6.2	8.0	6.3
<i>Host Effects</i>				
Variance	0.0001	0.0001	0.0000	0.0001
% variance, net of time effects	3.8	3.1	3.2	4.2
Bootstrapped s.e.	2.0	1.4	2.9	2.6
<i>Covariance</i>				
$2 \times$ Covariance	0.0004	0.0004	0.0000	0.0002
% variance, net of time effects	16.5	12.9	2.0	10.1
Bootstrapped s.e.	6.5	6.1	9.2	6.6

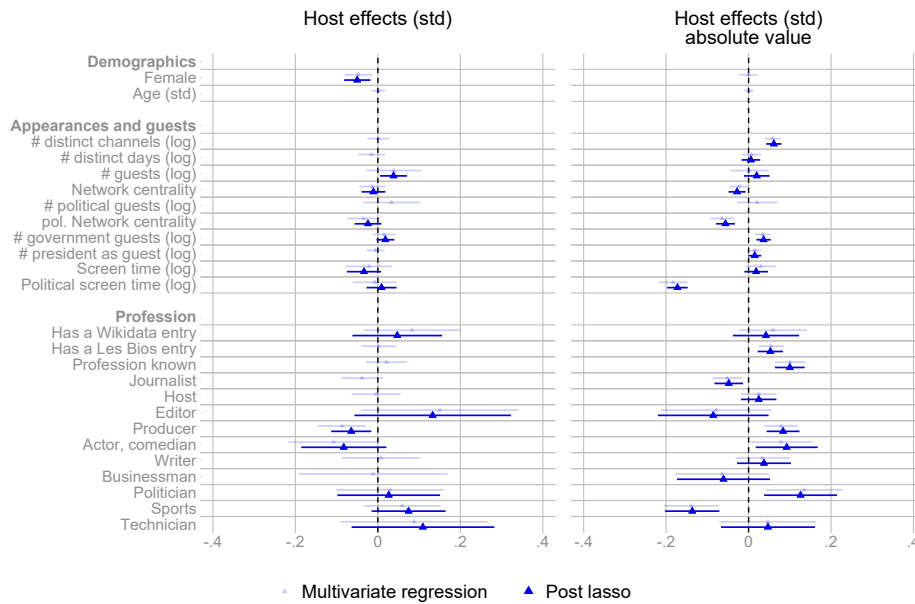
Notes: The table reports components of the variance decomposition laid out in Equation 2.4. The first row reports cross outlet-period variance in time share, the second one does the same, netting out time fixed effects from the time shares. The third row reports the split sample variance of channel-period effects, the fourth row expresses channel effects variance as a share of total variance, net of channel effects. The fifth row reports the standard deviation of bootstrapped shares (100 replications). Rows 6 to 8 do the same for host effects, rows 9 to 11 for the covariance between host and channel-period effects.

Host fixed effects

(a) Fixed effects on all left parties



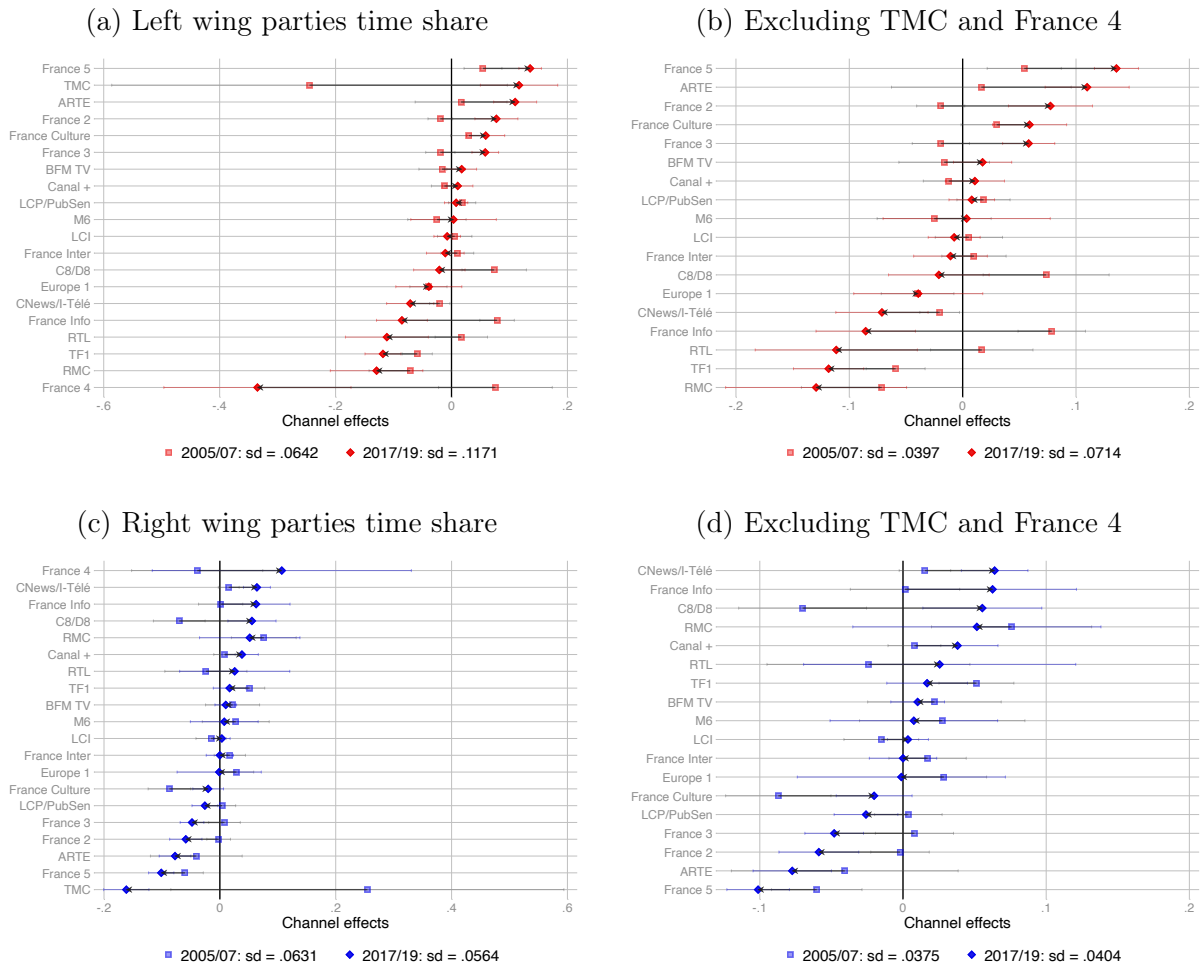
(b) Fixed effects on all right parties



Notes: The figures plot the OLS and lasso regression coefficients on the left and right wing host fixed effects.

Figure 2.18: Correlates of host fixed effects: OLS vs Lasso

Channel effects over time



Notes: The figures plot channel effects in the first and last periods of the sample, respectively Sept. 2005-Aug. 2007 and Sept 2017-Aug. 2019. In panels a and b (c and d), channel effects correspond to the premium in time share dedicated to left wing (right wing) parties that hosts give when working on the considered channel in the considered period. 95% confidence intervals are computed using bootstrapped standard errors (100 replications). Panels a and c include all the channels, panels b and d exclude TMC and France 4. The reported standard deviations in the legend are computed use the split-sample approach described in Section 4.1.

Figure 2.19: Channel effects over time

Table 2.13: Variance decomposition of left-wing political time share differences over time

	2005/11	2011/15	2015/19
<i>Total variance</i>			
Variance, raw	0.0042	0.0021	0.0081
Variance, net of time effects	0.0025	0.0021	0.0037
<i>Channel effects</i>			
Variance	0.0016	0.0017	0.0034
% variance, net of time effects	65.5	83.9	89.9
Bootstrapped s.e.	9.5	10.3	7.2
<i>Host Effects</i>			
Variance	0.0001	0.0001	0.0001
% variance, net of time effects	5.7	3.2	2.1
Bootstrapped s.e.	4.0	2.9	1.9
<i>Covariance</i>			
2 × Covariance	0.0007	0.0003	0.0003
% variance, net of time effects	28.8	12.9	8.1
Bootstrapped s.e.	8.8	10.9	7.3

Notes: The table reports components of the variance decomposition laid out in Equation 2.4. The first row reports cross outlet-period variance in time share, the second one does the same, netting out time fixed effects from the time shares. The third row reports the split sample variance of channel-period effects, the fourth row expresses channel effects variance as a share of total variance, net of channel effects. The fifth row reports the standard deviation of bootstrapped shares (100 replications). Rows 6 to 8 do the same for host effects, rows 9 to 11 for the covariance between host and channel-period effects.

Table 2.14: Variance decomposition of right-wing political time share differences over time

	2005/11	2011/15	2015/19
<i>Total variance</i>			
Variance, raw	0.0063	0.0023	0.0034
Variance, net of time effects	0.0028	0.0028	0.0029
<i>Channel effects</i>			
Variance	0.0023	0.0024	0.0024
% variance, net of time effects	80.8	85.9	85.3
Bootstrapped s.e.	9.8	7.9	7.1
<i>Host Effects</i>			
Variance	0.0001	0.0001	0.0001
% variance, net of time effects	3.3	2.8	2.9
Bootstrapped s.e.	2.6	2.3	2.0
<i>Covariance</i>			
2 × Covariance	0.0004	0.0003	0.0003
% variance, net of time effects	15.9	11.3	11.8
Bootstrapped s.e.	9.6	8.5	7.0

Notes: The table reports components of the variance decomposition laid out in Equation 2.4. The first row reports cross outlet-period variance in time share, the second one does the same, netting out time fixed effects from the time shares. The third row reports the split sample variance of channel-period effects, the fourth row expresses channel effects variance as a share of total variance, net of channel effects. The fifth row reports the standard deviation of bootstrapped shares (100 replications). Rows 6 to 8 do the same for host effects, rows 9 to 11 for the covariance between host and channel-period effects.

Changes in political time shares around the takeover

Table 2.15: Effect of the takeover on the time share of each political group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. With channel fixed effects							
Treated×2015/17	0.00718 (0.00676)	-0.00184 (0.00419)	0.000618 (0.00766)	-0.0153** (0.00670)	-0.000969 (0.00590)	0.00600 (0.00469)	0.00430 (0.00453)
Treated×2017/19	-0.0266* (0.0133)	-0.0200* (0.0103)	-0.0210** (0.00957)	-0.00216 (0.0269)	0.0217 (0.0194)	0.0333 (0.0241)	0.0148*** (0.00475)
Observations	771080	771080	771080	771080	771080	771080	771080
R ²	0.607	0.612	0.620	0.649	0.629	0.622	0.609
\bar{y} (control, post)	.108	.067	.289	.183	.248	.079	.027
Panel B. With host-channel fixed effects							
	Rad. left	Greens	Left	Liberal	Right	Rad. right	Other
Treated×2015/17	0.00443 (0.00619)	0.00277 (0.00401)	-0.00330 (0.00874)	-0.0222*** (0.00697)	-0.00221 (0.00723)	0.0130** (0.00457)	0.00752 (0.00523)
Treated×2017/19	-0.0254* (0.0137)	-0.00780 (0.0114)	-0.0262* (0.0133)	-0.0194 (0.0240)	0.0109 (0.0190)	0.0535*** (0.0171)	0.0143** (0.00604)
Observations	754993	754993	754993	754993	754993	754993	754993
R ²	0.623	0.630	0.635	0.665	0.645	0.637	0.626
\bar{y} (control, post)	.108	.067	.289	.183	.248	.079	.027

Notes: The outcome variable is the time share of distinct political groups: radical left, greens, left, liberals, right, far right, and unclassified. Panel A. estimates correspond to Equation 2.6, Panel B. estimates correspond to Equation 2.7. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

Table 2.16: Effect of the takeover on the time share of political groups, by channel

	(1)	(2)	(3)	(4)	(5)	(6)
	All left-wing parties		All right-wing parties		Radical parties	
C8/D8×2015/17	0.0648*** (0.0186)	0.0466* (0.0244)	-0.0276 (0.0352)	-0.0106 (0.0464)	0.0297 (0.0395)	0.0200 (0.0502)
C8/D8×2017/19	-0.00635 (0.0399)	0.00824 (0.0361)	0.0521** (0.0241)	0.0340 (0.0240)	0.0935** (0.0368)	0.0851* (0.0420)
CNews/I-Télé×2015/17	0.00820 (0.0116)	0.00412 (0.0114)	0.00316 (0.0121)	0.0107 (0.0131)	0.0214* (0.0104)	0.0255** (0.00993)
CNews/I-Télé×2017/19	-0.0804** (0.0358)	-0.0666* (0.0348)	0.0594*** (0.00971)	0.0709*** (0.0149)	0.0130 (0.0406)	0.0349 (0.0279)
Canal+×2015/17	-0.0114 (0.0167)	-0.00368 (0.0183)	0.0176 (0.0126)	0.0154 (0.0136)	-0.00841 (0.00818)	-0.00567 (0.0103)
Canal+×2017/19	-0.0341* (0.0178)	-0.0362 (0.0229)	0.0363** (0.0170)	0.0305 (0.0217)	-0.0360** (0.0143)	-0.0367** (0.0153)
Observations	771080	754993	771080	754993	771080	754993
R^2	0.623	0.638	0.621	0.637	0.619	0.635
Channel FE	Yes	No	Yes	No	Yes	No
Channel-host FE	No	Yes	No	Yes	No	Yes

Notes: The outcome variable is the time share of distinct political groups: left-wing parties (radical left, greens and left) in Columns (1)-(2), right-wing parties (right and radical right) in Columns (3)-(4), radical parties (radical left and radical right) in Columns (5)-(6). Estimates in odd-numbered columns correspond to Equation 2.6, estimates in even-numbered columns correspond to Equation 2.7. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

Table 2.17: Effect of the takeover on left-wing guests time share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. With channel fixed effects							
	Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated × 2015/17	0.00597 (0.0107)	-0.00323 (0.00831)	0.00445 (0.00803)	0.00247 (0.0112)	0.00740 (0.00979)	-0.0249 (0.0148)	0.000808 (0.0102)	0.00763 (0.0116)
Treated × 2017/19	-0.0676*** (0.0227)	-0.0856*** (0.0234)	-0.0606** (0.0271)	-0.0671*** (0.0230)	-0.0603** (0.0220)	-0.0542*** (0.0129)	-0.0640** (0.0301)	-0.0671*** (0.0223)
Observations	771080	779770	761962	743473	771080	695226	688174	691457
R ²	0.623	0.146	0.729	0.624	0.623	0.634	0.650	0.610
	Panel B. With host-channel fixed effects							
	Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated × 2015/17	0.00389 (0.0100)	-0.000306 (0.00778)	0.0000733 (0.00818)	0.00192 (0.0108)	0.00429 (0.00923)	-0.0206 (0.0132)	0.000716 (0.0113)	0.00734 (0.0110)
Treated × 2017/19	-0.0594** (0.0245)	-0.0608** (0.0253)	-0.0545* (0.0294)	-0.0580** (0.0244)	-0.0519** (0.0233)	-0.0445** (0.0164)	-0.0464 (0.0320)	-0.0588** (0.0242)
Observations	754993	764170	745622	727648	754993	679542	648636	676434
R ²	0.638	0.187	0.739	0.640	0.638	0.650	0.666	0.626

Notes: The outcome variable is the time share of all left-wing politicians (radical left, greens, left). Column 1 reports the baseline specification. In Column 2, time effects are date fixed effects while in Column 3, they are date-hour-platform fixed effects. In Column 4, days during which the ARCOM prescribes equal speaking time are excluded. In Column 5, we take the inverse hyperbolic sine of the outcome variable. In Column 6, the time share of political forces excludes members of the government, in Column 7, they exclude non-professional politicians (PENOPs). In Column 8, we exclude summer months (July and August). Panel A. estimates correspond to Equation 2.6, Panel B. estimates correspond to Equation 2.7. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

Table 2.18: Effect of the takeover on right-wing guests time share

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. With channel fixed effects							
Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated×2015/17	0.00504 (0.00914)	0.0118 (0.00762)	0.00804 (0.00892)	0.00619 (0.00783)	0.0386*** (0.0127)	0.00473 (0.00831)	0.00401 (0.00981)
Treated×2017/19	0.0550*** (0.00954)	0.0161** (0.00634)	0.0556*** (0.00970)	0.0543*** (0.0101)	0.0494*** (0.00995)	0.0909*** (0.0144)	0.0507*** (0.00986)
Observations	771080	779770	761962	771080	695226	688174	691457
R^2	0.621	0.146	0.725	0.623	0.634	0.647	0.610
Panel B. With host-channel fixed effects							
Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated×2015/17	0.0108 (0.00971)	0.0110 (0.00875)	0.0117 (0.0104)	0.00779 (0.00988)	0.0101 (0.00828)	0.0382*** (0.0130)	0.00127 (0.00869)
Treated×2017/19	0.0645*** (0.0111)	0.0443*** (0.0151)	0.0672*** (0.0127)	0.0642*** (0.0114)	0.0591*** (0.00965)	0.0967*** (0.0221)	0.0459*** (0.0116)
Observations	754993	764170	745622	727648	754993	679542	648636
R^2	0.637	0.189	0.736	0.639	0.638	0.651	0.663

Notes: The outcome variable is the time share of all right-wing politicians (right, radical right). Other notes as in Table 2.17.

Table 2.19: Effect of the takeover on radical parties guests time share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. With channel fixed effects							
	Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated \times 2015/17	0.0132* (0.00719)	0.00994 (0.00812)	0.00777 (0.00597)	0.0135* (0.00731)	0.0136* (0.00650)	0.0183** (0.00797)	0.0131* (0.00723)	0.0159** (0.00738)
Treated \times 2017/19	0.00668 (0.0339)	-0.0251* (0.0142)	-0.00349 (0.0364)	0.00814 (0.0338)	0.00747 (0.0320)	0.0146 (0.0323)	-0.00353 (0.0251)	0.0102 (0.0319)
Observations	771080	779770	761962	743473	771080	695226	688174	691457
R^2	0.619	0.141	0.729	0.619	0.621	0.636	0.649	0.611
	Panel B. With host-channel fixed effects							
	Baseline	Date FE	D-H-P FE	Excl. equal-time	IHS	Excl. govt	Excl. PENOPs	Excl. summer
Treated \times 2015/17	0.0174** (0.00783)	0.0101 (0.00767)	0.00918 (0.00668)	0.0175** (0.00793)	0.0171** (0.00712)	0.0253*** (0.00834)	0.0122 (0.00856)	0.0210** (0.00834)
Treated \times 2017/19	0.0281 (0.0276)	0.0102 (0.0158)	0.0129 (0.0290)	0.0293 (0.0274)	0.0278 (0.0262)	0.0347 (0.0265)	0.00532 (0.0199)	0.0310 (0.0270)
Observations	754993	764170	745622	727648	754993	679542	648636	676434
R^2	0.635	0.187	0.739	0.636	0.637	0.652	0.663	0.627

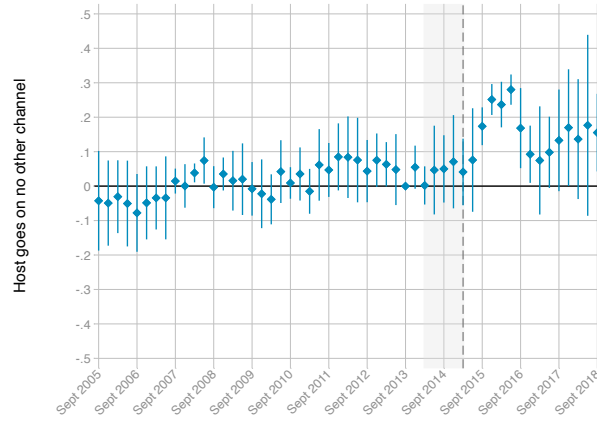
Notes: The outcome variable is the time share of all radical parties politicians (radical left, radical right). Other notes as in Table 2.17.

Hosts staying or leaving around the takeover

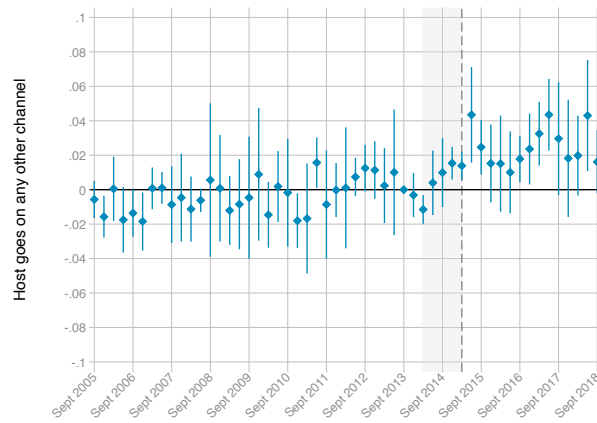
Table 2.20: Hosts staying or leaving after the takeover, by channel

	(1)	(2)
	Stays	Stays
Treated \times 2015/17	-0.154*** (0.0429)	
Treated \times 2017/19	-0.151 (0.0883)	
C8/D8 \times 2015/17		-0.0671*** (0.0133)
C8/D8 \times 2017/19		-0.321*** (0.0295)
CNews/I-Télé \times 2015/17		-0.268*** (0.0124)
CNews/I-Télé \times 2017/19		-0.295*** (0.0286)
Canal+ \times 2015/17		-0.126*** (0.0134)
Canal+ \times 2017/19		-0.0448 (0.0290)
Observations	263832	263832
R^2	0.468	0.469
\bar{y} (control, post)	0.379	0.379

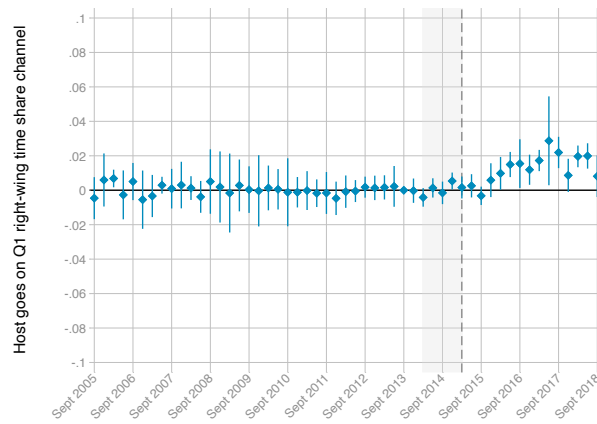
Notes: The outcome variable is an indicator for whether a given host-channel pair existing in quarter t is still existing in quarter $t + 4$. Column (1) presents the baseline specification. Column (2) reports estimates by channel. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.



(a) Host on no other channel in quarter $t + 4$



(b) Host on any other channel in quarter $t + 4$



(c) Host on other channel in Q1 right-wing share in $t + 4$

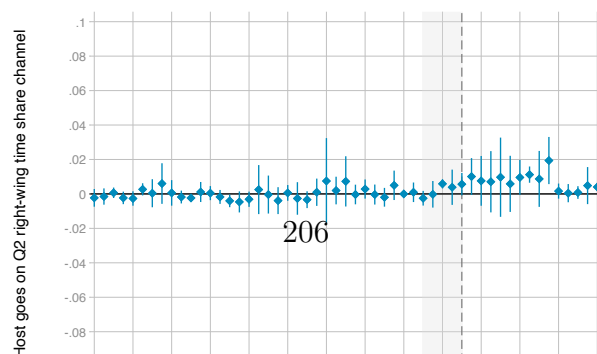


Table 2.21: Hosts observed on another outlet

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	1(Pol guests)	1(Many pol)	1(Journalist)	1(Newscast)	1(Male)	1(LesBios)	1(2y ago)	1(Prime)	1(Ratings)	1(Res ratings)
Treated × 2015/17	0.0229*** (0.00456)	0.0124 (0.00852)	0.0245*** (0.00476)	0.0186*** (0.00658)	0.0179*** (0.00503)	0.0194 (0.0133)	0.0163*** (0.00684)	0.00843 (0.00590)	0.0238*** (0.00380)	0.0497*** (0.00621)	0.0377*** (0.0131)
Treated × 2017/19	0.0222** (0.00832)	0.0239*** (0.00512)	0.0235** (0.0111)	0.0173* (0.00936)	0.0187* (0.00941)	0.0213 (0.0173)	0.0241** (0.00987)	0.0204*** (0.00534)	0.0216** (0.00897)	0.0460** (0.0177)	0.0368 (0.0228)
Treated × 2015/17 × 1.Inter	0.0179** (0.00651)	0.0179** (0.00651)	0.0185** (0.00727)	0.0108 (0.0241)	0.0283 (0.0564)	0.00423 (0.0149)	0.0540*** (0.0135)	0.0200** (0.00953)	-0.00662 (0.0229)	-0.0194* (0.00931)	-0.0109 (0.0156)
Treated × 2017/19 × 1.Inter	-0.00442 (0.0103)	-0.00442 (0.0103)	-0.000327 (0.0104)	0.0224 (0.0132)	0.0705*** (0.0156)	0.000367 (0.0246)	-0.0379 (0.0430)	-0.00438 (0.00872)	0.000874 (0.0168)	-0.0270*** (0.00751)	-0.0125 (0.0186)
Observations	263832	263832	143131	263832	263832	263832	263832	263832	263832	146100	154436
R ²	0.463	0.464	0.460	0.463	0.463	0.463	0.464	0.464	0.463	0.451	0.447
\bar{y} (control, post)	0.0377										
\bar{y} (control, post, inter=0)	0.0379	0.0379	0.0406	0.0386	0.0409	0.0302	0.0338	0.0473	0.0383	0.0337	0.0361
\bar{y} (control, post, inter=1)	0.0375	0.0375	0.0343	0.0367	0.0313	0.0426	0.0801	0.0256	0.0365	0.0281	0.0257

Notes: The outcome variable is an indicator for whether a given host-channel pair existing in quarter t is no longer exists in quarter $t + 4$ and the host is observed on another outlet in the sample. Column (1) presents the baseline specification. Column (2) includes an indicator for whether the host has guests who are politically classified. In Column (3), the indicator is for, among hosts who have political guests, those that have an above channel-quarter specific median share of political guests. In Column (4) the indicator is for whether the host is credited as a journalist for the show. The dummy in Column (5) indicates whether the host's show is a newscast. In Column (6), the variable indicates whether the host is male and in Column (7) whether he has a 'Les Biographies' entry. The indicator variable in Column (8) is for whether the host was already on the channel two years ago. The indicator variable in Column (9) is for whether the host's shows are during prime time (7:00-9:00am for radio, 19:00-21:00 for TV). In Column (10), the indicator is for whether the host has above median viewership (within channel-quarters). The indicator in Column (11) is similar, except that the viewership share is residualized on date-hour FEs and channel-season FEs, to measure whether the host tends to over- or under-perform. The last rows report the mean of the outcome variable on control channels for the period ranging from April 2015 to August 2019. Standard errors are clustered at the outlet level and indicate significance 1, 5, and 10% with ***, **, and *, respectively.

Table 2.22: Hosts observed on no other outlet

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	1(Pol guests)	1(Many pol)	1(Journalist)	1(Newscast)	1(Male)	1(LesBios)	1(2y ago)	1(Prime)	1(Ratings)	1(Res ratings)
Treated × 2015/17	0.131*** (0.0446)	0.0698*** (0.0254)	0.137*** (0.0543)	0.0947*** (0.0385)	0.0950*** (0.0400)	0.227*** (0.0577)	0.132*** (0.0456)	0.0847*** (0.0253)	0.114** (0.0487)	0.119*** (0.0333)	0.118*** (0.0325)
Treated × 2017/19	0.128 (0.0813)	0.116* (0.0663)	0.150 (0.100)	0.0715 (0.0812)	0.100 (0.0807)	0.275*** (0.0873)	0.140 (0.0830)	0.169** (0.0625)	0.131 (0.0873)	0.199 (0.131)	0.161* (0.0808)
Treated × 2015/17 × 1.Inter		0.104*** (0.0230)	0.0662*** (0.0212)	0.107*** (0.0141)	0.225*** (0.0298)	-0.129*** (0.0295)	-0.0112 (0.0204)	0.0463 (0.0320)	0.0601 (0.0483)	0.0484 (0.0794)	-0.0250 (0.0380)
Treated × 2017/19 × 1.Inter		0.0171 (0.0336)	0.00948 (0.0276)	-0.209*** (0.0399)	0.267*** (0.0709)	-0.193*** (0.0312)	-0.137*** (0.0363)	-0.112*** (0.0239)	-0.0220 (0.0353)	-0.0280 (0.0640)	-0.0390*** (0.0140)
Observations	263832	263832	143131	263832	263832	263832	263832	263832	263832	146100	154436
R ²	0.478	0.479	0.479	0.479	0.479	0.479	0.478	0.481	0.478	0.502	0.499
\bar{y} (control, post)	0.583										
\bar{y} (control, post, inter=0)	0.682	0.682	0.560	0.574	0.621	0.590	0.595	0.711	0.613	0.668	0.627
\bar{y} (control, post, inter=1)	0.507	0.507	0.453	0.593	0.507	0.578	0.453	0.422	0.526	0.527	0.571

Notes: The outcome variable is an indicator for whether a given host-channel pair existing in quarter t is no longer exists in quarter $t + 4$ and the host is observed on no other outlet in the sample.

3

The Far-Right Donation Gap

This chapter is based on a paper co-authored with Julia Cagé (Sciences Po Paris) and Yuchen Huang (Paris School of Economics).

Abstract

We document a widespread decline in the share of donors to charities in Western countries over the past decade, and show that this can be in part explained by a lower propensity to donate among far-right voters. Focusing on France, we first conduct a large-scale survey ($N = 12,600$) and show that far-right voters are significantly less likely to report a charitable donation than the rest of the population, conditional on a rich set of controls. Second, using administrative tax data for the universe of French municipalities ($N \simeq 33,000$) combined with electoral results, we find that the negative relationship between vote shares for the far right and charitable donations holds in a broad range of specifications, at both the extensive and the intensive margin, and controlling for municipality fixed effects. Third, we exploit unique geo-localized donation data from several charities and document similar patterns. All evidence points towards a drop in the propensity to donate driven by a shift in social norms that threatens general acceptance of the charitable sector.

Keywords: charitable giving, political donations, far-right, social norms, underlying preferences, communal moral values, universalist moral values.

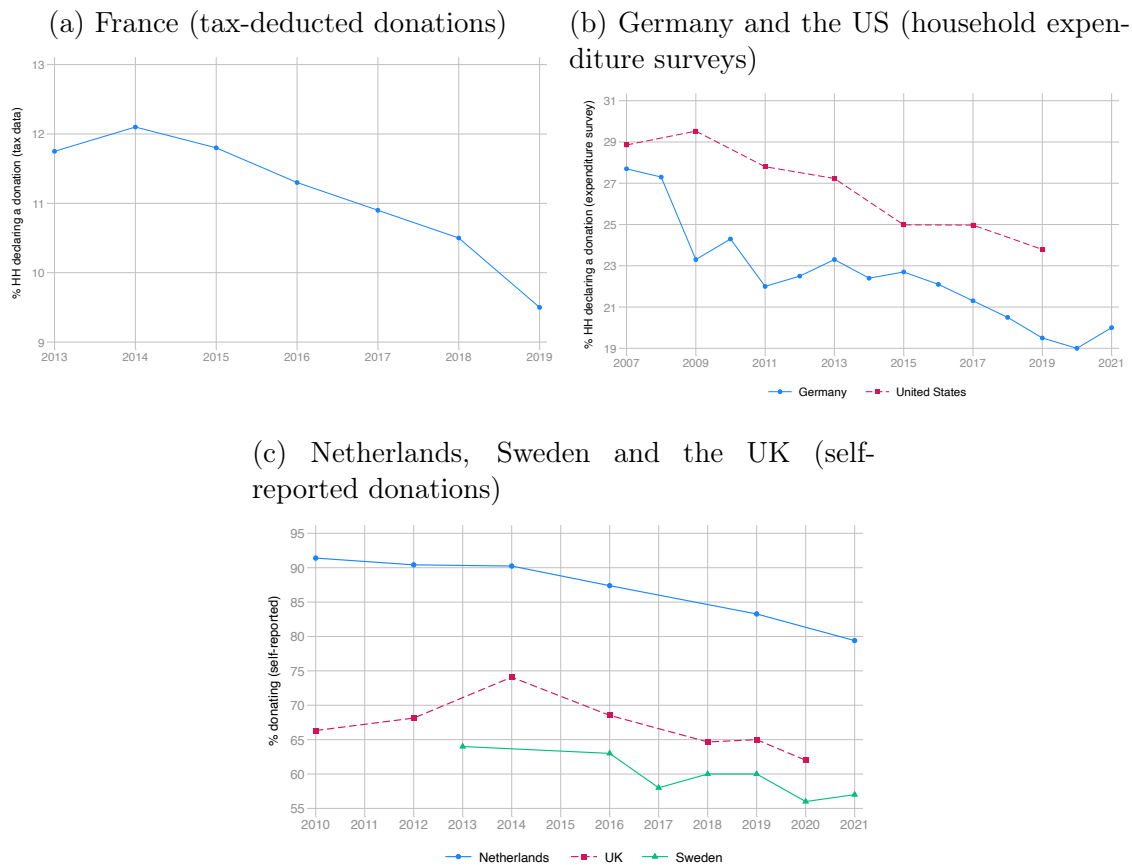
JEL No: H24, H31, L38.

1 Introduction

Although the 21st century is often being presented as the “age of philanthropy”¹ with an unprecedented increase in the amount of charitable giving, the *share* of the population donating to charities is declining in many Western democracies (see Figure 3.1). This drop –

¹See e.g. <https://www.theguardian.com/world/2021/jun/18/a-million-dollars-a-minute-the-rise-and-rise-of-philanthropy>.

concomitant with the electoral rise of far-right parties in many of these countries – poses a threat to the charitable business, as giving increasingly relies on a small number of individuals. In this paper, we provide novel evidence on the relationship between political ideology and charitable donations. Specifically, drawing on insights from rich survey data, geo-localized tax data, and charity records, we show a significant and persistent donation gap among individuals who align themselves with far-right political ideologies. We investigate whether this gap may lead to a further reduction in charities’ donor base in the years to come.



Notes: The figure plots the evolution of the share of households making a donation to charities. Sub-figure 3.1a reports this share for France using administrative tax data from Cagé and Guillot (2021) on the share of donors declaring a charitable donation on their tax return. Sub-figure 3.1b reports this share for Germany and the US using respectively the Deutscher Spendenrat and the Panel Study on Income Dynamics. Sub-figure 3.1c reports this share using household survey data for Netherlands (GINPS), Sweden (Giva Sveriges) and the UK (GSS).

Figure 3.1: An overall decline in the share of donors to charities

To document what we call the “far-right donation gap” – the fact that far-right voters are significantly less likely to donate to charities than other citizens, even relative to people who abstain – we proceed in three steps. First, we run a large-scale pre-registered survey

($N = 12,600$) one week before the 2022 presidential elections in France, where we ask respondents about their past and future donations. According to our findings, Marine Le Pen’s (far-right) voters are 6 percentage points less likely to make a charitable donation than citizens who abstain, and Eric Zemmour’s (far-right) voters are 4 percentage points less likely. On the contrary, both Jean-Luc Mélenchon (left) and Emmanuel Macron’s (center) voters as well as supporters of all the other parties on the left and right of the political spectrum are more likely than abstainers – by 6 to 20 percentage points – to contribute money to a charity. Thus, while voting is generally associated with a higher propensity to donate relative to abstention, the reverse is true for far-right voters (Yen and Zampelli, 2014).²

On top of income, these findings are robust to controlling for a large number of demographic observables, such as the age of the surveyed individuals, their gender, marital status, religion, life satisfaction, trust, pessimism, as well as the size of the city where they live. It is also robust to using the surveyed individuals’ self-placement on a left-right scale, furthermore showing that the negative relationship between far-right voting and donations we document is specific to right-wing extremism and not to political extremism in general. More importantly, the size of the far-right effect does not vary when we control for additional observables, suggesting that the far-right donors gap is structural. We are also able to reproduce the same finding in similar survey data for Germany, which shows that the far-right donation gap is not specific to France.

Survey data may suffer from a number of concerns, in particular regarding social desirability bias in reporting. To address these concerns, we leverage detailed administrative data on tax-deducted charitable contributions (Cagé and Guillot, 2021) for 33,037 French municipalities³ between 2013 and 2019, and compare them with the vote shares obtained by each of the candidates in these municipalities in the first round of the presidential elections, controlling for a large set of city-level socio-demographic variables, including the local supply of charities. We find that a 10% increase in the vote share obtained by Le Pen in a municipality compared to abstention is associated with a 1.9% decrease in the share of households declaring a charitable donation on their tax return. Importantly, the magnitude of the estimated effect is consistent with the one we obtain when using the survey data; furthermore, we show that this effect happens at both the intensive and the extensive margin. We also find that this finding is robust to using the panel dimension of the administrative tax data – with two

²Unfortunately, we do not have information on whether far-right voters devote more or less time (e.g. through volunteering) to charities compared to other voters. We indeed only have information on monetary contributions. However, in the last section of the paper, when dealing with external validity, we show that the probability of making a blood donation is also lower for far-right voters in Germany.

³This represents nearly the universe of French municipalities ($\simeq 36,000$), except the very small ones due to statistical secrecy.

presidential elections that took place during our period of interest (in 2012 and 2017) – and thus to controlling for municipality fixed effects.

While most individuals declare their donations on their tax form to benefit from tax deductions, not all of them do so. To overcome this limitation of the administrative data, we finally obtain detailed information on donations received (with the precise date of the donation and the location of the donor) by three large charities in France: “Action Contre la Faim” (ACF), SOS Méditerranée (SOSM), and Oxfam. Using these data – similarly merged with the electoral results at the local level – we find that a 10% increase in Le Pen’s vote share in a municipality compared to abstention is associated with a 1.9% (ACF) to 0.1%5 (Oxfam) percent decrease in the amount donated to these charities per household. In other words, even if the magnitude of the elasticity of the donations with respect to Le Pen’s vote is smaller for smaller charities like Oxfam and SOSM, the far-right donation gap remains both economically and statistically significant independently of the data source or of the estimation methods used.

What explains the negative relationship between far-right ideology and charitable giving? As highlighted above, our results are robust to controlling for pessimism, unhappiness, and (the lack of) trust at the individual level. Thus, while these factors have been associated with far-right voting (see e.g. [Algan et al., 2017, 2019](#); [Giuliano and Wacziarg, 2020](#); [Guriev and Papaioannou, 2022](#)), they cannot drive our findings. This suggests that there may be some unobservable characteristics that are simultaneously associated with far-right voting and a lower probability of making a charitable donation.

Inspired by the far-right criticism of charities as “*universalism without borders*”, we hypothesize – in the spirit of [Enke \(2020\)](#) – that far-right voting is associated with a lower propensity to donate to charities through the underlying moral values of far-right voters, specifically a sense of “communal” morality, which allocates more altruism to the in-group members than to the out-group members of society.⁴ On the one hand, an individual displaying communal morality would be less likely to donate to “distant” charities, since the identity of the recipient is by definition unknown and likely to be out-group; on the other hand, the individual would also be more likely to identify with the far-right parties.⁵

We provide evidence in support of the communal morality hypothesis using information from the supply side of charity. Using the National Directory of Associations, we locate all

⁴Moral values correspond to people’s deep beliefs about what is right and wrong. See also [Enke et al. \(2020\)](#).

⁵In the context of multi-party election systems such as the ones we observe in the majority of the Western democracies – and contrarily to the US, we think that it is the far right that appeals to people holding communal morality rather than the traditional right. We come back to this point below.

the existing charities in France. We separate the charities into “global” and “non-global” using their stated purpose: a charity is categorized “global” if its purpose explicitly mentions places in a foreign country or contains keywords such as “global” or “universal”. We show that the interaction between the far-right effect and the percentage of global charities in a municipality is statistically significant and negative: in municipalities where charities have a globalist outlook, the far-right donors gap is wider. On the contrary, in municipalities that contain more non-global (local) charities, far-right voters are less hesitant to donate, with the reverse being true for more centrist voters.

In addition, we discuss anecdotal evidence that far-right politicians have become increasingly critical of the charitable sector and its “universalism without borders,” in particular compared to other parties. We next show for a sub-sample of our tax data that the elasticity of charitable donations with respect to political donations is negatively associated with the far-right vote. In other words, the higher the far-right vote, the more political and charitable donations are negatively associated, suggesting that far-right voters perceive them more as substitutes than the rest of the population. This is in line with the reading that far-right voters substitute charitable donations with financing far-right politicians that push their communal values through policy.

We also show that the far-right donation effect is not driven by social desirability. It is indeed stronger in municipalities where the share of far-right voters is smaller (i.e. in municipalities where voters should theoretically suffer from more stigma if they do not donate).

Finally, we discuss the decomposition and implications of the far-right donation gap. While the far right donation gap persists when we exploit the panel dimension of our tax data and control for municipality fixed effects, it is smaller in magnitude. Hence, we show that the donation gap is not only driven by people who newly converted to the far-right ideology but also by those whose affiliation with the far-right is more deep-rooted. In particular, we show that cities that voted more for the far right in 2012 experienced a much sharper drop between 2013 and 2019 in the share of households donating unexplained by changes in city-level characteristics. We provide additional evidence using survey data and show that the 2022 far-right donation gap is largely driven by people who were already voting for Le Pen in the previous 2017 presidential elections. These results point toward the fact that the decreasing trend in charitable contributors is driven by both the *intensification* of the existing preferences of people with communal morality, as well as by the *adoption* of people who did not possess communal morality before. Given the increasingly persistent electoral success of far-right parties, this poses a threat to the charitable sector in two ways. First, the stronger and more persistent the electoral success of far-right parties, the greater the

chance that donations will decline. Second, a shrinking donor base at the extensive margin undermines the democratic legitimacy of large public subsidies that benefit charities in most countries.

Literature review This paper contributes to the nascent literature that highlights the role of political preferences in the decision to give money to a non-profit organization. While there is a large literature investigating the determinants of charitable donations (among others [Andreoni, 1989](#); [Andreoni et al., 2017](#); [Dawood, 2015](#)), the focus has mostly been on the overall rise of the charitable sector, with little attention to the fact that the *share of donors* among citizens has actually been decreasing in recent years. At the same time, a number of important determinants of charitable donations have been raised, both theoretically and empirically: pure altruism versus a warm-glow effect ([Andreoni, 1990](#)), reputation concerns ([Tirole and Bénabou, 2006](#)), price of giving ([Randolph, 1995](#); [Fack and Landais, 2010, 2016](#)), and others.⁶

From a political economy perspective, we still know little about the relationship between charitable giving and political preferences of voters. Some papers have started to address this relationship between political affiliation and charitable giving. [Alzuabi et al. \(2022\)](#) use a large-scale household longitudinal survey in the UK to show that aligning with Labour vs the Conservative party is negatively associated with both the probability of donating money to charities and the proportion of income donated to charities. [Yen and Zampelli \(2014\)](#) use panel data to document the relationship between political preferences, tax burden and charitable contributions in the US; they show that Republican counties tend to report higher contributions than Democrats. [Paarlberg et al. \(2019\)](#), however, find no evidence of a positive relationship between political conservatism and the probability of giving in the US, suggesting that multi-party systems such as France may provide a better ground to understand the relationship between political preferences and giving. These papers link the relationship between political preferences and the donation behavior to three main factors: religious identity, preferences for government redistribution, and communication of economic status (see e.g. [Brooks, 2007](#); [Margolis and Sances, 2017](#); [Yang and Liu, 2021](#)). We are, to the extent of our knowledge, the first to provide evidence – using both survey and tax data – on the evolution of the share of charitable donors and its relationship to the electoral rise of the far right and of communal moral values. This is particularly relevant given that a shrinking donor base echoed by a rise of the far right might undermine public support for

⁶On the role played by the price of a donation, see also [Karlán and List \(2007\)](#) and [Rondeau and List \(2008\)](#) using lab-field experiments. [Karlán and List \(2020\)](#) investigate the role played by leading donors, and [Eckel and Grossman \(2003\)](#), [Eckel and Grossman \(2006\)](#), and [Eckel and Grossman \(2008\)](#) investigate the relative efficiency of matching vs rebate.

the generous subsidies that are granted to charities, often in the form of tax advantages that overproportionally benefit richer donors (Reich, 2018).

Finally, compared to Enke (2020) and Enke et al. (2020) who first emphasized the difference between universalist and communal moral values, our contribution is threefold. First, we exploit the multi-party electoral system in France, which allows us to show that the donation gap is specific to the far right as opposed to the right or to extreme parties in general. Second, we make an important methodological contribution by highlighting the need to combine survey data with administrative and charity-level data to cross-validate results and overcome various measurement errors related to social desirability and reporting. Third, we leverage the time series of our data to compare the co-evolution of donation behavior and political preferences over time.

The rest of the paper is organized as follows. In Section 2 below, we present the three novel databases we build for this study. Section 4 presents our main findings on the negative relationship between far-right voting and political donations. In Section 4, we discuss several mechanisms that could explain this result and highlight in particular the role played by communal values. Section 5 discusses the policy implications of our results as well as their external validity, and Section 6 concludes.

2 Data

In this section, we briefly introduce the different data sources we use in this paper. Summary statistics and a more detailed description of the data are provided in the Online Appendix.

2.1 Survey data

We ran a pre-registered survey between April 2 and April 4, 2022, as a part of the 2022 French Electoral Survey ("*L'Enquête Electorale Française*"), a monthly panel run from September 2021 to June 2022 jointly by the survey company Ipsos, the newspaper *Le Monde* and the CEVIPOF at Sciences Po Paris.⁷

The data contains 12,600 individuals representative of the French voting-age population, for which we have detailed socio-demographic characteristics (including gender, age, education,

⁷The survey was pre-registered at the AER RCT Registry: AEARCTR-0009023. The first part of the survey is about the reported donations (past and future) of the surveyed individuals and is at the core of this research paper. The second part is an experiment aimed at understanding the role played by tax deductions and belongs to a different research project.

location, profession, religion, and income; see online Appendix Table 3.6 for descriptive statistics). Respondents are also asked about their political preferences, such as their projected vote in the 2022 presidential elections and their self-reported vote in the 2017 presidential elections (see online Appendix Table 3.7 for summary statistics). As part of this research project, we added to this standard electoral survey a novel module on past and future charitable and political donations. Specifically, we introduce the following questions:⁸

- Of the following organizations, have you made a donation in the last 12 month to [*a non-profit organization/a foundation/the Téléthon/A political party or movement/An electoral campaign*]?
- If yes, what was the overall amount of your donations?
- If yes, did you report this donation on your income tax return?
- Do you plan to make a donation in the next 12 months to [*a non-profit organization/a foundation/the Téléthon/A political party or movement/An electoral campaign*]?
- If yes, how much do you plan to donate?

Table 3.1 reports summary statistics on these variables. 43% of the surveyed individuals report a charitable donation in the past 12 months, while about 5% of the individuals report a political donation. Among those who report a donation, the average amount donated (combining both charitable and political donations⁹) is €249. 28% of the surveyed respondents in our sample also report having declared this donation on their income tax return.¹⁰

Our data may suffer from a reporting bias that has been well-documented in the existing literature; because of social desirability, surveyed individuals indeed tend to over-report donations (see e.g. Bekkers and Wiepking, 2011). Indeed, only about 10 to 12% of households in France report on average a donation every year on their income tax return as observed in the fiscal data (Cagé and Guillot, 2021). Note, however, that part of the gap between the fiscal data and the reported donations in the survey can also come from the fact that

⁸In the questionnaire, we distinguish non-profit organizations (in French, “*associations*”) from foundations (in French, “*fondations*”) given that they are formally two different legal forms of non-profits, which may create confusion (e.g. citizens may think that they make a donation to a “foundation” that formally is an “association” and the reverse; the main differences between the two come from the bylaws as well as from somehow different tax deductions – in particular with respect to the wealth tax). We ask specifically about the “Téléthon” given it is the most famous French non-profit organization.

⁹Unfortunately, given the strong space (and monetary) constraints associated with the fact of adding a new module to the existing “*Enquête électorale*” (with 12,600 surveyed individuals), we were not able to ask individuals separately for the amount of their charitable donations vs. the amount of their political donations. Hence, when presenting the results using the survey data, we will focus on the extensive margin, i.e. on the probability of making a donation. However, the administrative tax data allows us to also consider the intensive margin (i.e. the average amount given).

¹⁰Charitable and political giving can indeed benefit in France from a nonrefundable income tax credit equal to 66% of the gift (see e.g. Fack and Landais, 2010; Cagé and Guillot, 2021).

Table 3.1: Summary statistics: Past and future (self-reported) donations

	Mean	St.Dev
Have you made a donation in the past 12 months to		
A non-profit organization	0.31	0.46
A foundation	0.15	0.36
The telethon	0.10	0.30
A charitable donation (overall)	0.43	0.49
A political party	0.04	0.21
An electoral campaign	0.02	0.14
I have not made any donation	0.55	0.50
Amount given in the past 12 months		
Amount of donations (including the 0s)	113	422
Amount of donations (cond. on giving)	249	600
=1 if declared the donation(s)	0.63	0.48
Have you declared a donation in the past 12 months to		
A non-profit organization	0.22	0.41
A foundation	0.10	0.30
The telethon	0.07	0.25
A charitable donation (overall)	0.28	0.45
A political party	0.03	0.17
An electoral campaign	0.01	0.11
Do you plan to make a donation in the next 12 months to		
A non-profit organization	0.31	0.46
A foundation	0.15	0.36
The telethon	0.10	0.30
A political party	0.03	0.18
An electoral campaign	0.01	0.11
Total amount I plan to give	228.13	501.22
I don't plan to make a donation	0.57	0.50
Observations	12,600	

Notes: The table reports summary statistics for the surveyed individuals as part of the *Enquête Electorale Française* (see the text for more details). An observation is an individual.

most households (as encompassed in the tax data) include several individuals, while the survey data is at the individual level. Importantly, note also that the magnitude of the total amount of donations implied by the survey declaration matches approximately the official numbers (€5.9 billions vs €5.1 billions).¹¹ To address the bias that may come from social desirability, we nonetheless complement our survey data with administration tax data and charitable-level donation information.

2.2 Administrative tax data

We use administrative tax data from [Cagé and Guillot \(2021\)](#), which include the total amount of charitable donations declared by households aggregated at the municipality/year level for nearly all municipalities in France between 2013 and 2019 – our data include 33,037 municipalities, which represents nearly the universe of French municipalities ($\simeq 36,000$), except the very small ones due to statistical secrecy. Specifically, the data include all charitable donations that benefit from a 66% tax deduction, as well as the “Coluche” donations, which are donations to charities that help people in need and benefit from a non-refundable income tax credit of 75%. For our analysis, we focus on the general charitable donations deductions, which is the most commonly available and economically more relevant measure.

These administrative tax data also include municipality/year information on the number of tax households, the reference tax income of the households, the total amount of tax paid, the numbers of retired persons and the total pension. We complement it with census data that allow us to control for the demographics of the municipalities, including the age of the population, the average education, the share of foreigners, etc. Online Appendix Table 3.9 presents summary statistics on these variables.

The administrative tax data contain information on the aggregate amount donated, but do not report the precise destination of the donations.¹² Furthermore, some households may decide not to report their donations, in particular if they are not eligible to the tax deductions (see e.g. [Fack and Landais, 2010](#)). To overcome these limitations, we thus finally reach out to individual charities to obtain their donation data.

¹¹According to *France Générosité* (<https://www.francegenerosites.org/chiffres-cles/>), charitable donations in France in 2021 are around €5 billions; political donations could reach around €100 million during election years ([Cagé, 2018](#); [Cagé, 2022](#)), leading to a total amount of individual-level donations of €5.1 billions.

¹²Apart from setting apart the charities that help people in need under the “Coluche” label (see above), and disentangling between charitable and political donations.

2.3 Charity-level data

We obtained access to detailed donation data from three different charities: *Action Contre la Faim* (ACF), *SOS Méditerranée* (SOSM), and *Oxfam*.

Action Contre la Faim (“Action Against Hunger”) is a non-governmental organization that fights hunger in the world.¹³ It provided us with data on all the donations it received from France between 2010 and 2022, with detailed information on the date of the donation as well as on the location of the donor. Overall, the dataset includes more than 4 million observations, and accounts for around €40-50 millions in donations every year .

SOS Méditerranée is a European, maritime-humanitarian search and rescue organization established in 2015, currently operating in the Mediterranean sea in international waters north of Libya.¹⁴ It provided us with annual information on all donations received since its creation in 2015, with information on the city of the donors. We focus on all donations coming from Metropolitan France.

Oxfam is a British-founded confederation of 21 independent charitable organizations focusing on the alleviation of global poverty, founded in 1942 and led by Oxfam International. We obtain data from Oxfam France, the French section of Oxfam International¹⁵, covering 2009-2022.

As we did for the administrative tax data, we merge these data from ACF, SOSM and Oxfom with municipality/year-level covariates, and compute the amount of donations made per tax household (see online Appendix Table 3.10 for summary statistics). Online Appendix Figure 3.8 plots the temporal evolution of these donations.

¹³According to its website, “*its mission is to save lives eradicating hunger through the prevention, detection, and treatment of malnutrition, in particular during and after emergency situations caused by conflicts and natural disasters.*” The organization was created in 1979 by a number of French intellectuals, and is structured on an international network. It provides a coordinated response in nearly 50 countries (see online Appendix Figure 3.7 for an illustration).

¹⁴The organization chartered the Aquarius and more recently the Ocean Viking in order to rescue people fleeing by sea from Libya who are at risk of drowning. It was founded by German former captain Klaus Vogel and Frenchwoman Sophie Beau after the Italian navy ended the rescue Operation Mare Nostrum in 2014. It has headquarters in Marseille (France), Milan (Italy), Frankfurt (Germany), and Geneva (Switzerland).

¹⁵Oxfam France, was founded in 1988 – under the name “Agir ici pour un monde solidaire” – and became part of Oxfam International in 2003 (first as an observer and then as a member in 2006).

2.4 Additional data

Electoral data For each municipality in our data, we obtain the election results for the first round of the 2012, 2017 and 2022 presidential elections from the Interior Ministry. We label the political ideology of each candidate in accordance with the political party for which they ran. Throughout the analysis, we focus on the vote shares obtained by each candidate as a share of the total number of registered voters, thus taking abstention as the reference category. We focus on presidential elections for the sake of comparison between municipalities (contrarily to other elections, the same candidates are indeed running in all the municipalities during presidential elections).

National Directory of Associations To gain insights into the supply side of charities, we rely on the French national directory of associations (*“Répertoire National des Associations,”* RNA), the repository of all the non-profit organizations. By law, all French non-profit organizations are included in this dataset, which contains a unique identifier for each of them, as well as their stated purpose.

While an association has to make a declaration to the RNA at the time of its creation, many associations that cease to be active do not report their dissolution. As a result, the RNA contains 2 million observations, of which 1.5 million non-profit organizations marked active in 2018, i.e. significantly more than the INSEE’s estimation of 1.2 million active non-profit organizations.¹⁶ Hence, while the RNA can be used as a proxy for the overall stock of global vs. local non-profits at the municipality level in France (see Section 4.1 below), it cannot be used to accurately measure the annual variation in the local supply of associations.

3 Empirical estimation: The far-right donation gap

In this section, we document a negative relationship between support for the far-right and donations to charities. We first consider the extensive margin, i.e. the propensity to donate to charities, and then turn to the intensive margin.

3.1 Far-right ideology and the propensity to donate to charities

Evidence from survey data

To estimate the relationship between electoral support for the far right and the propensity to donate to charities, we first rely on the individual-level survey data described in Section

¹⁶<https://www.insee.fr/fr/statistiques/5365639>.

2.1. We estimate the following linear probability model:

$$Donation_i = \pi_0 + \mathbf{Vote}_i\pi_1 + \mathbf{X}_i'\pi_2 + \epsilon_i \quad (3.1)$$

where i indexes the surveyed individuals, and $Donation_i$ is an indicator variable equal to one if the respondent reports that she has made a charitable donation in the past 12 months, and to zero otherwise.

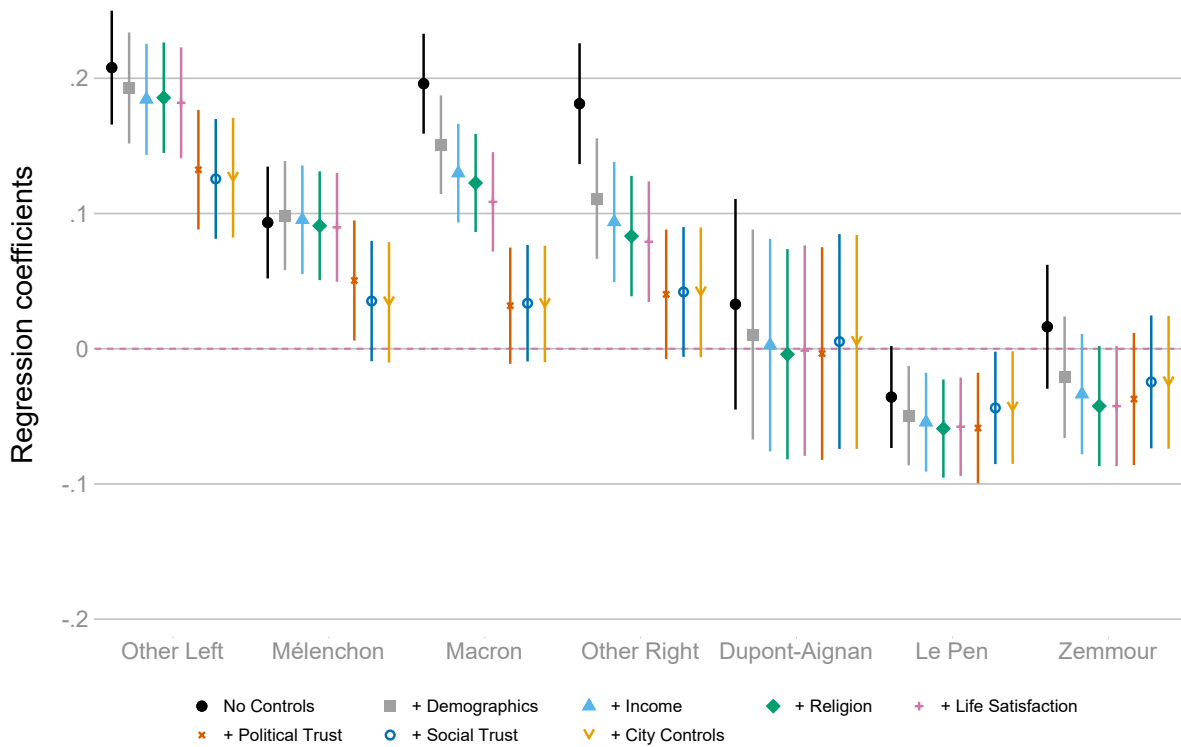
\mathbf{Vote}_i' is a vector of indicator variables that represent the candidate that the respondent intends to vote for in the 2022 presidential elections. 12 candidates ran in the first round of the elections, out of which three can be classified as far-right: Nicolas Dupont-Aignan (who obtained 2.06% of the casted votes), Eric Zemmour (7.07%) and Marine Le Pen (23.15%). The omitted category is abstention.

\mathbf{X}_i' is a vector of controls including demographics (gender, age, marital status, residential area), the respondent's income bracket, her religion, life satisfaction, trust in political actors (such as the president, the mayor of the municipality where she lives, the media and the political parties) and trust in various members of the society (such as family members, strangers, and people of different nationalities and religions) (see Section 2.1 above and Appendix Table 3.8). We include these controls one at a time.

Figure 3.2 reports the results of the estimation (see online Appendix Table 3.11 for the associated regression table).¹⁷ We first report the raw relationship between far-right support and the propensity to donate (black dots) and then progressively introduce the controls. As highlighted above, the omitted category is abstention. We find that respondents who intend to vote for Le Pen report on average a 4 to 6% lower probability of having made a donation than people who abstained. This result is significant at the 5% level. Zemmour's voters also tend to give less than abstainers – and than supporters of other candidates (except Le Pen) – but to a lower extent. We find no statistically significant effect for Nicolas Dupont-Aignan's supporters (not reported), but there are very few (Dupont-Aignan only obtained 2.06% of the votes in the first round of the elections).

Importantly, this gap in the propensity to donate between far-right citizens and other voters does not disappear or change in magnitude when we add controls. On the other hand, for other candidates, we see a drop in the conditional propensity to donate, in particular when we control for income. In other words, the observable characteristics such as income and life

¹⁷Figure 3.2 reports the coefficients we obtain when estimating an OLS model. We show in the Appendix that the results are robust to rather using a Probit model (given the use of indicator variables); see below.



Notes: The figure reports the results of the estimation of equation (3.1), using an OLS model. An observation is an individual ($N = 12,600$) and the corresponding regression coefficients are reported in the online Appendix Table 3.11. Error bars show 95% confidence intervals. The “other left” candidates include the candidate from the French communist party (Fabien Roussel, 2.28% of the votes), the candidate of the *Nouveau Parti Anticapitaliste* (Philippe Poutou, 0.77%), the candidate of the Socialist party (Anne Hidalgo, 1.75%), and the candidate of *Lutte Ouvrière* (Nathalie Arthaud, 0.56%). The “other right” candidates include the candidate of *Les Républicains* (Valérie Pécresse, 4.78%).

Figure 3.2: The far-right donation gap: Evidence from self-reported donations (2022 electoral survey)

satisfaction can partly explain why supporters of other parties donate more than abstainers, but cannot rationalize why far-right supporters contribute less.

Robustness We find similar results if, rather than using the expected votes, we use a self-evaluated political preference scale from 0 (Left) to 10 (Right) as the independent variable; the results are reported in the online Appendix Table 3.12.

Moreover, the results are robust to using a probit specification rather than the linear probability model (online Appendix Tables 3.14) and to using intended future donations instead of reported past donations (3.15).

Finally, we investigate the external validity of this finding using the German Socioeconomic Panel, a large household panel in Germany that records voting intentions and various self-reported donation behavior between 2010 and 2020. Besides donations to charities, we can investigate donations for refugees following to 2015 and blood donations. We run the same specification as for the French survey data which is reported in Online Appendix Figure 3.12 and Table 3.16. We find that supporters of the far-right *Alternative für Deutschland* (AfD) party differ similarly from other voters and are 10-25 % less likely to report donations, although the difference is not significantly different from unaligned voters. Perhaps unsurprisingly, AfD voters are even less likely to have donated – money or in kind – for refugees following the arrival of large numbers of refugees in 2015. The right panel of figure 3.12 suggests that AfD voters are even less likely to donate blood than other voters. Blood donations are a good proxy of a time-intensive, pro-social, in-kind donations with little to no direct interaction with the beneficiary.

However, since the survey data rely on self-reported donations, there could be a concern over misreporting, in particular due to the social desirability bias.¹⁸ To deal with this, we turn to the use of the administrative tax data.

Evidence from administrative tax data

We validate the survey analysis with administrative tax data on the annual share of households in a municipality that report a charitable donation in their tax declaration. We merge this share with the municipality-level electoral results. Specifically, we estimate the following model:

¹⁸Especially given the comparison between our data and administrative tax data discussed in Section 2 above.

$$Donors_{c(d),t} = \pi_0 + \mathbf{Elections}'_{c(d)}\pi_1 + \mathbf{X}'_{c(d),t}\pi_2 + \gamma_d + \omega_t + \epsilon_{c(d),t} \quad (3.2)$$

where c indexes the municipalities, d the departments and t the years.

The dependent variable, $Donors_{c(d),t}$, is the share of households that deducted a charitable donation on their tax return in city c in year t . The tax data is available annually from 2013 to 2019. Two presidential elections took place around this time period – in 2012 and in 2017. We take the average share of households donating between 2013 and 2016 and compare it to the 2012 election results; and between 2017 and 2019 and relate it to the 2017 election results.¹⁹ In our preferred estimation strategy, we use the inverse hyperbolic sine (IHS) transformation of the share of households and the vote share to obtain elasticities.²⁰ Standard errors are clustered at the level of the 96 departments in France.

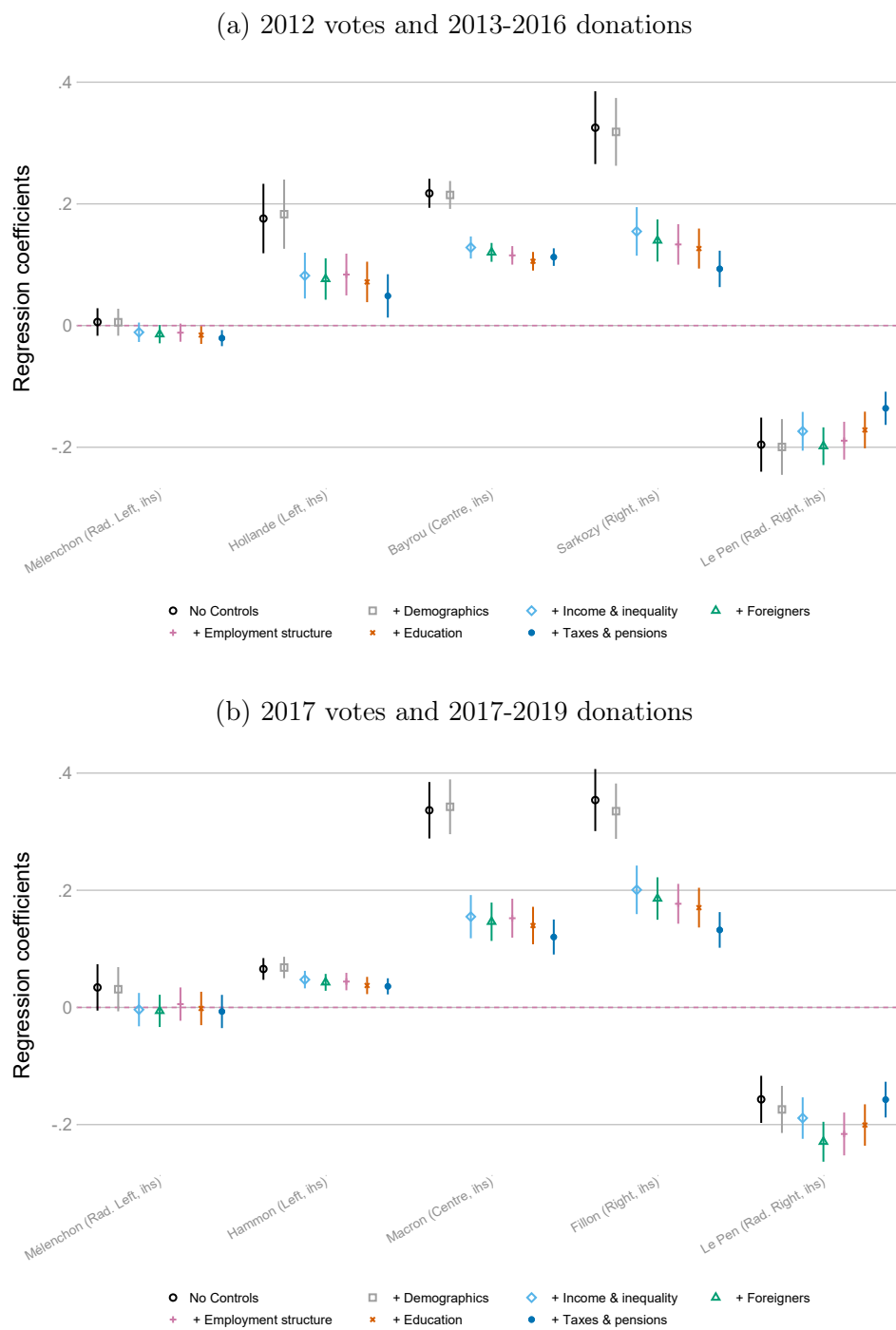
The main independent variable, $\mathbf{Elections}'_{c(d)}$, is a vector of the (IHS transformations of the) vote shares obtained by the candidates in the 2012 and 2017 presidential elections, with the share of the registered voters abstaining as the reference category. $\mathbf{X}'_{c(d),t}$ is a time-varying vector of municipality-level controls – that, as before, we introduce sequentially – and include measures of demographics, median income, inequality, the share and structure of the foreign population, the local employment structure, the average education levels as well as the number of tax households and pensions (see Section 2.2 for details on the set of controls included and Appendix Table 3.9 for summary statistics). Finally, we control for time (ω_t) and department (γ_d) fixed effects.

Figure 3.3 reports the results of the estimation of equation (3.2) separately for the 2012 presidential elections (sub-Figure 3.3a) and for the 2017 presidential elections (sub-Figure 3.3b).²¹ The patterns we obtain are consistent with the survey-level results: both in 2012 and 2017, municipalities with a higher vote share for Le Pen also have a lower share of households declaring a donation to charities. In terms of magnitude, with respect to abstention, a one-percent increase in the vote shares for Le Pen in 2012 is associated with a 0.19 to 0.17% decrease in the share of donors. This finding is robust to controlling for the local socioeconomic conditions, and the magnitude is roughly similar for the 2017 elections (0.19 to 0.21% decrease).

¹⁹In the robustness section below, we show that our results hold if we take instead the sum of the share of donors, or if we consider each year separately. In Section 3.2, we present the results of the estimation if we introduce municipality fixed effects.

²⁰This accounts for outliers and zero values, which we do observe in the voting data for some candidates given the high granularity of the municipality data

²¹The corresponding regression tables are reported in the online Appendix (respectively Table 3.17 and 3.18).



Notes: The figure reports with 95% confidence intervals the results of the estimation of equation (3.2), using an OLS model. An observation is a city and the corresponding regression coefficients are reported in the online Appendix Tables 3.17 (sub-Figure 3.3a) and 3.18 (sub-Figure 3.3b). Error bars show 95% confidence intervals. The omitted category is abstention.

Figure 3.3: The far-right donation gap: Evidence from administrative tax data and electoral results

Note also that the magnitude of the estimates is consistent with the one we obtain when using the survey data. As reported in Figure 3.3, a 1% increase in the vote share for Le Pen is associated with a decrease of around 0.2% in the share of donors, which implies that moving from abstaining to voting Le Pen (i.e. a 100% increase in the vote share for Le Pen) leads to a 20% decrease in the share of donors. According to the survey data, the share of donors among abstainers is equal to 35.4%; hence, a 20% drop would decrease this share to 28.3%, i.e. a drop of 7 percentage points in the share of donors, consistent with the fact that, according to our survey estimations, Le Pen’s voters are around 6 percentage points less likely to make a charitable donations than abstainers. Hence, it is thus unlikely that the reported difference between far-right voters and the rest of the population comes from a reporting bias.

Robustness The estimates presented in Figure 3.3 are robust to using the level (rather than the IHS transformation) of both the dependent and independent variables (see online Appendix Tables 3.19 and 3.20). They are also robust to using the sum rather than the mean of the share of donors between two presidential elections (online Appendix Tables 3.21 and 3.22), and hold when breaking the tax data down annually (online Appendix Table 3.23). We further show that our results are robust to dropping the municipalities in the eight electoral districts that had elected a far-right representative in the 2017 election to remove any potential direct influence of elected politicians on our estimates (online Appendix Table 3.24). Finally, as we will discuss in more detail in section 5.1, this finding is qualitatively similar when controlling for municipality fixed characteristics.

3.2 Far-right ideology and the overall amount of donations

Until now, we have focused on the extensive margin of charitable donations which motivated this study with figure 3.1, considering whether individuals have made (or reported) a donation independently of the amount of this donation. We now turn to the intensive margin, i.e. investigate how much individuals contributed conditional on making a donation.

To do so, we estimate equation (3.2), but use as the dependent variable the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. First, we use the overall amount reported in the tax data (covering the time period 2013-2019); second, we proceed similarly but instead consider alternatively the donations received by ACF (2012-2022), Oxfam (2012-2022), and SOSM (2015-2022²²).

²²SOS Méditerranée was indeed created in 2015 and we thus do not have donation data before this date.

Table 3.2 presents the results. A 1% increase in Le Pen’s vote share is associated with a 0.2% decrease in the total amount of charitable donations declared on the tax forms (Column (1)). The magnitude of the results is roughly similar if we consider the donations made to “Action Contre La Faim” (Column (2)). We also observe a statistically significant drop for donations received by Oxfam (Column (3)) and SOSM (Column (4)), but the magnitude is much smaller.

This lower magnitude is in part due to the fact that both SOSM and Oxfam (France) are relatively small charities, which have less variability in the donations they receive. Online Appendix Table 3.27 reports the standardized coefficients. A one standard-deviation increase in the vote share for Le Pen is associated with a 0.05 standard-deviation decrease in the total donations made to Oxfam and SOSM. While smaller, this is qualitatively similar to a 0.09 standard-deviation decrease observed for the donations declared on the tax form.

Finally, we are well aware of the fact that both SOSM and Oxfam, and to a certain extent ACF, may be perceived as rather left-wing associations. It would be of interest to investigate in the future whether these findings based on charity records also hold with more right-wing ones. Unfortunately, very few charities are willing to share their donation-level information, and we leave this question for future research.

4 Mechanisms

In the section above, we show that far-right voters are consistently less likely to donate to charities than other citizens, including those who abstain in elections. Not only is this donation gap robust to controlling for a wide range of covariates (including demographics, income, religion, and various measures of social capital), but the magnitude of the far-right effect – in stark contrast to other political affiliations – is nearly unchanged when we introduce these controls. This seems to suggest that the link between far-right voting and a lower propensity to donate is structural and exhibits something deeper about voters’ beliefs.

In this section, we suggest that the far-right donation gap may be partly explained by differences in people’s moral views: far-right ideologies appeal more to people who hold a “communal” morality that emphasizes in-group identity (as defined by Enke, 2020) and are thus averse to donating to “distant” charities where the identity of the recipient cannot be controlled. We show evidence for this in two ways. First, we leverage data on the *supply-side of charities*, by exploiting city-level variations in the number of charities that focus on global issues. Second, we present evidence on the *supply side of politics*, and document that in recent years far-right politicians have increasingly attacked the charitable sector. We then

Table 3.2: Far-right ideology and the overall amount of donations

	(1)	(2)	(3)	(4)
	Tax data	ACF	Oxfam	SOSM
LFI (Rad. Left)	-0.032** (0.013)	0.026*** (0.010)	0.006*** (0.002)	0.012** (0.005)
PS (Left)	0.048*** (0.011)	0.051*** (0.009)	0.010*** (0.002)	0.009*** (0.003)
LREM (Center)	0.108*** (0.011)	0.052*** (0.011)	0.004* (0.002)	0.007 (0.005)
LR (Right)	0.142*** (0.019)	0.043*** (0.014)	-0.004 (0.003)	-0.000 (0.005)
DLF (Rad. Right)	-0.007 (0.007)	-0.007 (0.006)	-0.003* (0.002)	-0.013*** (0.004)
FN (Rad. Right)	-0.219*** (0.021)	-0.198*** (0.017)	-0.017*** (0.004)	-0.031*** (0.007)
Department FEs	✓	✓	✓	✓
Election FEs	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	57,349	57,349	57,349	27,561
Clusters (Departments)	101	101	101	95
Mean DepVar	4.23	0.58	0.03	0.04
Sd DepVar	0.59	0.49	0.11	0.14

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level between parentheses). An observation is a city/election, and all the specifications include city and election fixed effects. Controls are city-level controls and include local demographics, income, share of foreigners, employment, education and taxes. The dependent variable is the (IHS transformation of) the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. In Column (1), we consider the amount declared in the tax data; in Column (2), the donations received by *Action contre la Faim* (ACF); in Column (3), the donations received by *Oxfam*; and in Column (4), the donations received by *SOS Méditerranée* (SOSM). The independent variable is a vector of the (IHS transformations of the) vote shares obtained by the different candidates in the 2012 and 2017 presidential elections, except for column (4) since SOSM was founded only in 2015. The omitted category is abstention.

show that the higher the far-right vote share in a municipality, the higher the substitutability between charitable and political donations, suggesting that far-right voters instead donate to politicians that claim to support communal values through policy.

We also address and reject the hypotheses that the far-right donation gap is driven by the social desirability of donating, which makes us confident that it is indeed changes in underlying social norms that is driving the gap.

4.1 Local vs. global charities

The concept of universalist vs. communal moral values is described in details in [Enke \(2020\)](#), who also shows that universalist moral values are often strongly positively correlated to donations to "global" charities. On the contrary, voters with communal moral values seem to value authority and in-group identity and to be inherently averse to contributing to "distant" charities. In a multi-party political system such as France, such values would more likely serve as the psychological foundation for voting far-right.

To investigate whether the universalist vs. communal morality cleavage may explain the far-right donation gap, we provide evidence based on the composition of the supply of charities. Information about the supply of charitable associations at the municipality level come from the national directory of associations (see [Section 2](#) above for a brief description of the data). We characterize charitable associations by their mission statement, i.e. whether they are "global" charities that aim to help people in different parts of the world (which will typically be the case of ACF) or "non-global" charities, which might focus more on local issues.

We adopt a simple characterization method: we take the stated purpose of each of the charitable organizations and categorize them as a "global charity" based on the presence of "global keywords." Global keywords are selected from the (stemmed) list of all high-frequency words in the stated purpose of charities.²³ We manually choose the words that refer to foreign places²⁴ or those that contains a globalist thinking such as "global," "European," or "international." The complete list of the global keyword dictionary can be found in the online [Appendix Section A.3](#), where we also provide additional details on the classification method.

We categorize a charity as global if its stated purpose contains at least one global keyword.

²³High-frequency words refers here to words that showed up more than 10 times in all stated purposes of charities; in total, there are about 5,000 words that are high-frequency, out of which we selected 434 global keywords. We used the stemming function in French from the `nlTK.corpus`.

²⁴Foreign places include all the foreign continents, countries, regions and city names in adjective or noun form in the high-frequency list.

For example, a charity whose stated purpose is “distribution of school supplies in Morocco, Africa”²⁵ is categorized as global, while a charity whose stated purpose is “feed, heal and sterilize the stray cats of Saint-Médard”²⁶ is not categorized as global. Overall, about 18% of the charities are categorized as global, with a slightly higher concentration in larger cities.

To investigate whether the supply of global charities affects our findings, we estimate the following model:

$$\begin{aligned}
 Donors_{c(d),t} = & \pi_0 + \mathbf{Elections}'_{c(d)t} \pi_1 + \pi_2 \text{Number of global nonprofits}'_{c(d)t} \\
 & + \mathbf{Elections}'_{c(d)t} \times \text{Number of global nonprofits}'_{c(d)t} \pi_3 \\
 & + \pi_4 \text{Total number of nonprofits}_{c(d)t} + \mathbf{X}'_{c(d),t} \pi_5 + \gamma_d + \omega_t + \epsilon_{c(d)t} \quad (3.3)
 \end{aligned}$$

where $Donors_{c(d),t}$ is, as before, the (IHS transformation of the) share of households that have deducted a charitable donation in year t in city c , and $\mathbf{Elections}'_{c(d)t}$ is a vector of the (IHS transformation of the) vote shares obtained by each of the candidates in the presidential elections. $\text{Number of global nonprofits}'_{c(d)t}$ is the number of global nonprofit organizations per 1,000 inhabitants in city c and year t . We also control for the overall number of nonprofit organizations per 1,000 inhabitants in the city ($\text{Total number of nonprofits}_{c(d)t}$) (we use the IHS transformation of both variables). The other control variables included in the vector $\mathbf{X}'_{c(d),t}$ are the same as in Section 3.1.

We are interested in the sign of π_3 . Indeed, one might indeed expect the likelihood of donating to a charity to vary with the supply of charities. In particular, while a citizen with communal morality might dislike donating to charity by default, she might be less reluctant to donate to charities that focus exclusively on local issues. Table 3.3 reports the results for the 2012 and 2017 presidential elections pulled together.²⁷ In Column (2), we introduce the number of charities and the number of global charities, where the latter is more predictive of the share of households that donate without affecting the estimated elasticities of the share of donors with respect to vote shares.

Column (3) shows that the negative relationship between far-right voting and the propensity to donate is stronger in places with more global charities. The effect is both statistically and economically significant: holding the total number of charities per 1,000 inhabitants

²⁵This is the case for example of the nonprofit organization “TEAM VW GOLF 2 BOSTON.”

²⁶Saint-Médard is a small city in France. This charity is called ‘*L’Ile aux chats*’ (Island for cats).

²⁷In the online Appendix, we report the results for these two elections considered separately: see Table 3.28 for the 2012 presidential elections and Table 3.29 for the 2017 ones.

Table 3.3: The propensity to donate and local supply of global charities

	(1)		(2)		(3)	
	% HHs donating		% HHs donating		% HHs donating	
Mélenchon	-0.033***	(0.009)	-0.033***	(0.009)	-0.033***	(0.009)
PS	0.082***	(0.007)	0.082***	(0.007)	0.081***	(0.007)
LREM	0.074***	(0.008)	0.073***	(0.008)	0.074***	(0.008)
LR	0.136***	(0.013)	0.136***	(0.013)	0.137***	(0.013)
Le Pen	-0.152***	(0.014)	-0.151***	(0.014)	-0.148***	(0.014)
Total # of charities			0.003*	(0.001)	0.003*	(0.001)
glob. charities			0.026***	(0.006)	0.176	(0.214)
glob. charities × Mélenchon					0.001	(0.020)
glob. charities × PS					0.030***	(0.011)
glob. charities × LREM					-0.017	(0.020)
glob. charities × LR					-0.018	(0.027)
glob. charities × Le Pen					-0.052**	(0.024)
Department FEs	✓		✓		✓	
Election FEs	✓		✓		✓	
Controls	✓		✓		✓	
Observations	55,949		55,949		55,949	
Clusters (Departments)	101		101		101	
Mean DepVar	3.12		3.12		3.12	
Sd DepVar	0.36		0.36		0.36	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level in parentheses). An observation is a city/election, and all the specifications include department and election fixed effects. Controls are city-level controls and include local demographics, income, share of foreigners, employment, education and taxes. The dependent variable is the (IHS transformation of) the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. "Globalist charities" are defined based on a set of keywords in the charities' statement of purpose (see the text for details).

constant, a 1% increase in the local number of global charities increases the size of the far-right donation gap by one fourth (from a baseline of -0.21 to -0.16). On the other hand, the interaction term is not statistically significant for the other parties, and positive for the Socialist party: left-wing voters donate more in cities with a higher number of global charities (although the magnitude of the effect is relatively small). This finding is consistent with our assumption that communal vs universalist moral values partly explain the far-right donation gap.

4.2 Salience and political donations

Charities as a far-right policy issue As early as 2014, Éric Zemmour – one of the far-right candidates in the 2022 presidential elections in France – wrote in his book *Le Suicide français*, in a chapter partly devoted to the charity sector (and in particular to “Les Restos du Coeur,” one of the main French nonprofit organization): everything “*was copied from the Anglo-Saxon models: the charitable objective, but also the number of celebrities (...), the altruistic theme, the universalism without borders, even the images of the studio recording (...). But while this business of charity was a secular tradition in the Anglo-Saxon Protestant world, it was an innovation in a Catholic country like France where the State had ousted the Church in its role of providing social charity since the Revolution. In France, solidarity is guaranteed by taxes and redistribution organizations that avoid the humiliating clash between a donor and his recipient. But it was undoubtedly the historical vocation of this post-Second World War generation to end up in the arms of Anglo-Saxon liberal Protestantism, after it had ruthlessly destroyed the moral and then economic foundations of the French Catholic-social state.*” This criticism has become more salient in recent years, in particular with a number of far-right local politicians (in particular the mayors) openly cutting subsidies to a number of nonprofit organizations. This, for example, was the case in 2015 of the far-right mayor of Mantes-la-Ville who ended public subsidies to the “*Ligue des droits de l’homme*” (the Human Rights League). In 2020, during the mayoral election campaigns, many far-right candidates vowed to end municipal subsidies to nonprofit organizations they presented as “communitarist” or to organizations “that promote mass immigration.”

Data from party manifestos confirms this trend and further suggests that other parties have adopted a more positive tone towards charities. For evidence on this, we turn to the Manifestos project (Lehmann et al., 2023), which details the share of party manifestos that make certain political statements. While it does not contain a measure directly linked to charities, it measures “Support for Civic-mindedness,” which codes all statements “favourable [to] the civil society and volunteering; decrying anti-social attitudes in times of crisis; appeals for public spiritedness and support for the public interest.”

Figure 3.23 plots the share of party manifestos supporting Civic-mindedness by political family in 2012 and 2017. To have sufficiently dense and comparable data in both elections, we aggregate parties by political families, with parties weighted by their vote shares. There is a clear increase in positive mentions of Civic-mindedness in party manifestos across the board, with the decline observed for the far-right as notable exception. This indicates that the far right’s change of tone with regard to civic-mindedness which is out of tune with the political competition.

Political vs. charitable donations With political parties increasing the salience of charities as globalist actors, communal voters might substitute away from charitable donations to political donations that give more direct support to their communal values through exclusionary policies. Thus, political donations present another margin of adjustment that can explain the differential donation behavior of far-right voters.

To test this hypothesis, we focus on the sub-sample of municipalities in our data for which information on the aggregate amount of political donations declared on the households' tax forms is available.²⁸ This sub-sample is richer and more urban than the complete sample of municipalities (online Appendix Table 3.30), a caveat that should be taken into account (even if we control for these observable characteristics). Moreover, more municipalities are included for the 2012 elections (5,632 observations) than for the 2017 ones (3,586 observations), which we thus consider separately. Reassuringly, the number of households donating to charities – our outcome of interest – is very similar in this sub-sample.

In Table 3.4, we estimate equation (3.2) but introduce as a control the share of households in the municipalities that declare a political donation, as well as this share interacted with the vote share obtained by the different political parties. Columns (1) to (3) reports the results for the 2012 presidential elections, and Columns (4) to (6) report the 2017 ones. For the sake of comparison, Columns (1) and (4) report the results without political donations; the magnitude of the estimated effects is similar to the one reported in Table 3.2 (Column (1)).

In Columns (2) and (5), we show that introducing political donations as a control does not affect the estimated far-right donation gap; if anything, the estimated coefficients slightly increase. Interestingly, political donations are positively correlated with the propensity to make a charitable donation. In Columns (3) and (6), we interact the vote share obtained by each candidates with political donations. While the share of political donors is still positively correlated with the share of charitable donors, the far-right donation gap *per se* is no longer significant. Instead, the far-right vote share is negatively associated with the share of charitable donors only in places where the share of political donors is high enough. In terms of magnitude, a 10% increase in the vote share for Le Pen is not significantly associated with a decrease in the share of tax-deducting donors to charity, but it is significantly associated with a 5.6% to 7.5% decrease in the share of charitable donors if the share of political donors increases by 10% as well. This suggests that political donations seem to act as substitutes

²⁸This includes all the municipalities for which there is a high-enough number of households making a political donation so as to guarantee anonymity. The data is from Cagé and Guillot (2021) and was provided to them by the tax administration. Unfortunately, we do not observe the beneficiary of the political donation in this administrative tax data.

for far-right voters.

4.3 Underlying preferences or social pressure?

We now turn to the relationship between one's preference to donate and the local social norms. Charitable giving is often perceived as a pro-social behavior: people donate not only to satisfy their own preferred level of altruism but also to send a signal to others.

Specifically, we want to determine whether social pressure also drives the far-right donation gap. [Perez-Truglia \(2018\)](#) shows for example that individuals are more politically active in more like-minded social environments. Similar social pressure might also apply to donations: even if a far-right supporter would like to donate less, she might still be motivated to donate if she lives in an environment where everyone donates. In addition, social norms can change. An individual might dare to expose her real preference if there is evidence that this opinion is more mainstream than she imagined, as documented by [Bursztyn et al. \(2020\)](#).

To explore this possibility, we look at the size of the far-right donation gap for cities below and above the median far-right vote. We then estimate equation (3.2) (including all the city-level controls), but consider separately cities with relatively low and high support for the far right in the 2012 and 2017 presidential elections.

Figure 3.4 presents the results. First note that, for both the 2012 and the 2017 elections, the far-right donation gap is statistically significant both in municipalities with both high and low electoral support for the far right. In the 2012 elections (sub-Figure 3.4a), we see that contrary to the social norm hypothesis, the far-right voting gap is stronger in municipalities with relatively fewer Le Pen voters. In 2017 (sub-Figure 3.4a), there is no statistically difference in the magnitude of the estimated coefficients between the two kinds of municipalities. Overall, our results show that, if anything, far-right voters are actually less reluctant to donate in a municipality with many far-right voters where they would not suffer from the stigma of not donating. This result lends supports to the hypothesis that the far-right donation gap relates to individual preferences and is not simply related to concerns about violating social norm.

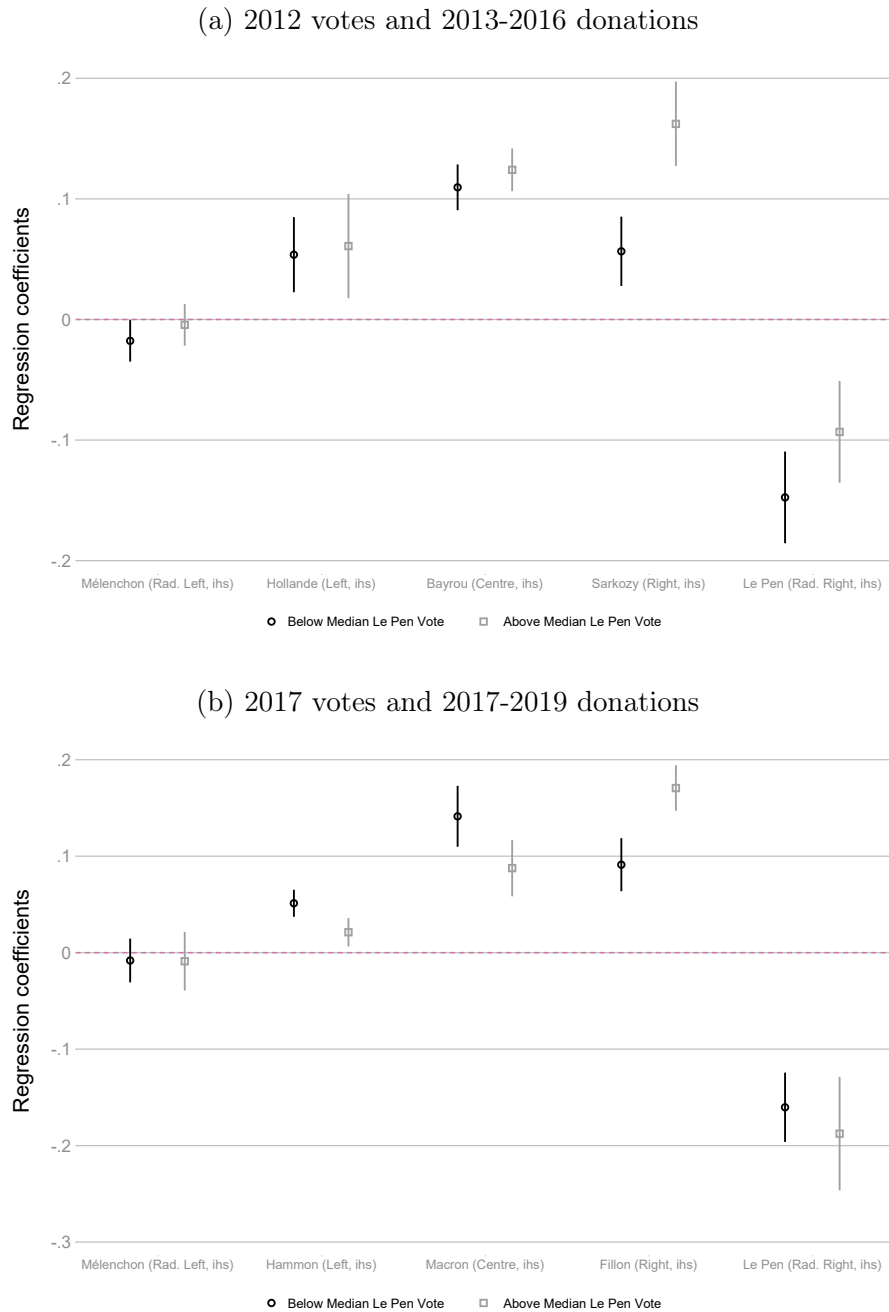
5 Discussion

Given the existence of the far-right donation gap, there is reason to worry that the recent rise in electoral support for the far right will threaten the future of the charitable sector, which may become even more concentrated. Thus, a key question is to what extent and in what

Table 3.4: The impact of political donations on the propensity to make a charitable donation

	2012 Elections			2017 Elections		
	(1)	(2)	(3)	(4)	(5)	(6)
LFI (Rad. Left)	0.20 (0.14)	-0.11 (0.14)	0.14 (0.42)	0.17 (0.14)	0.01 (0.15)	0.40 (0.26)
PS (Left)	0.99*** (0.16)	0.83*** (0.15)	1.55*** (0.30)	2.09*** (0.27)	1.84*** (0.29)	2.94*** (0.54)
LREM (Center)	3.31*** (0.31)	3.44*** (0.33)	4.48*** (0.51)	1.33*** (0.16)	1.37*** (0.17)	1.67*** (0.27)
LR (Right)	0.72*** (0.15)	0.59*** (0.15)	0.91*** (0.25)	0.64*** (0.16)	0.55*** (0.16)	1.14*** (0.26)
DLF (Rad. Right)	1.13** (0.51)	1.05*** (0.40)	2.69* (1.43)	1.51*** (0.42)	1.55*** (0.43)	3.07*** (0.92)
FN (Rad. Right)	-0.27* (0.15)	-0.30** (0.15)	0.22 (0.32)	-0.60*** (0.13)	-0.66*** (0.13)	-0.23 (0.23)
% donating pol.		0.13*** (0.01)	0.62*** (0.17)		0.09*** (0.02)	0.75*** (0.19)
% donating pol. × LFI (Rad. Left)			-0.36 (0.37)			-0.78** (0.35)
% donating pol. × PS (Left)			-0.73*** (0.26)			-1.40*** (0.36)
% donating pol. × LREM (Center)			-1.03** (0.45)			-0.58 (0.35)
% donating pol. × LR (Right)			-0.41* (0.24)			-0.91*** (0.31)
% donating pol. × DLF (Rad. Right)			-1.56 (1.11)			-2.13** (0.85)
% donating pol. × FN (Rad. Right)			-0.56* (0.30)			-0.75** (0.29)
Department FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	5,632	5,632	5,632	3,586	3,586	3,586
Clusters (Departments)	99	99	99	95	95	95
Mean DepVar	3.2	3.2	3.2	3.0	3.0	3.0
Sd DepVar	0.34	0.34	0.34	0.38	0.38	0.38

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level between parentheses). An observation is a city and all the specifications include department fixed effects. Columns (1) to (3) report the results for the 2012 presidential elections Columns (4) to (6) for the 2017 presidential elections. Controls are city-level controls and include local demographics, income, share of foreigners, employment, education and taxes. The dependent variable is the (IHS transformation of) the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. The main independent variable is a vector of the (IHS transformations of the) vote shares obtained by the different candidates in the 2012 and 2017 presidential elections. The omitted category is abstention. In Columns (3) and (6) we interact these shares with the share of households making a political donation in the municipality.



Notes: The figure reports with 95% confidence intervals the results of the estimation of equation (2), using an OLS model (standard errors are clustered at the level of the department). An observation is a city. The upper Figure 3.4a presents the results for the 2012 presidential elections, and the bottom Figure 3.4b for the 2017 presidential elections (the corresponding tables are the online Appendix Tables 3.32 and 3.33). All the estimations control for department fixed effects and the full set of city-level observables. Black bars with dots report the estimates for the municipalities whose vote share for Le Pen is below the median, and gray bars with squares for municipalities whose vote share for Le Pen is above the median. The independent variable is a vector of the (IHS transformation of the) vote shares obtained by candidates at the presidential elections (omitting abstention). The dependent variable is the (IHS transformation of) the share of households declaring a charitable donation in their tax returns.

Figure 3.4: The far-right donation gap: Municipality with high vs. low electoral support for the far-right

way could the share of charitable donors decline further, should the vote share of the far-right continue to rise. We discuss this question in two steps: first, we consider whether “new converts” to the far-right ideology significantly reduce their donations right away; secondly, we examine an “intensification” effect of communal morality for those who already voted for the far right in the past.

5.1 The new far-right voters

Do voters that have recently started voting for the far right but have not done so in the past also contribute less to charities? To tackle this question, we first exploit the panel dimension of the administrative tax data by introducing municipality fixed effects (γ_c) and estimate the following model:

$$Donations_{c,t} = \pi_0 + \mathbf{Elections}'_{c,t}\pi_1 + \mathbf{X}'_{c,t}\pi_2 + \gamma_c + \omega_t + \epsilon_{c,t} \quad (3.4)$$

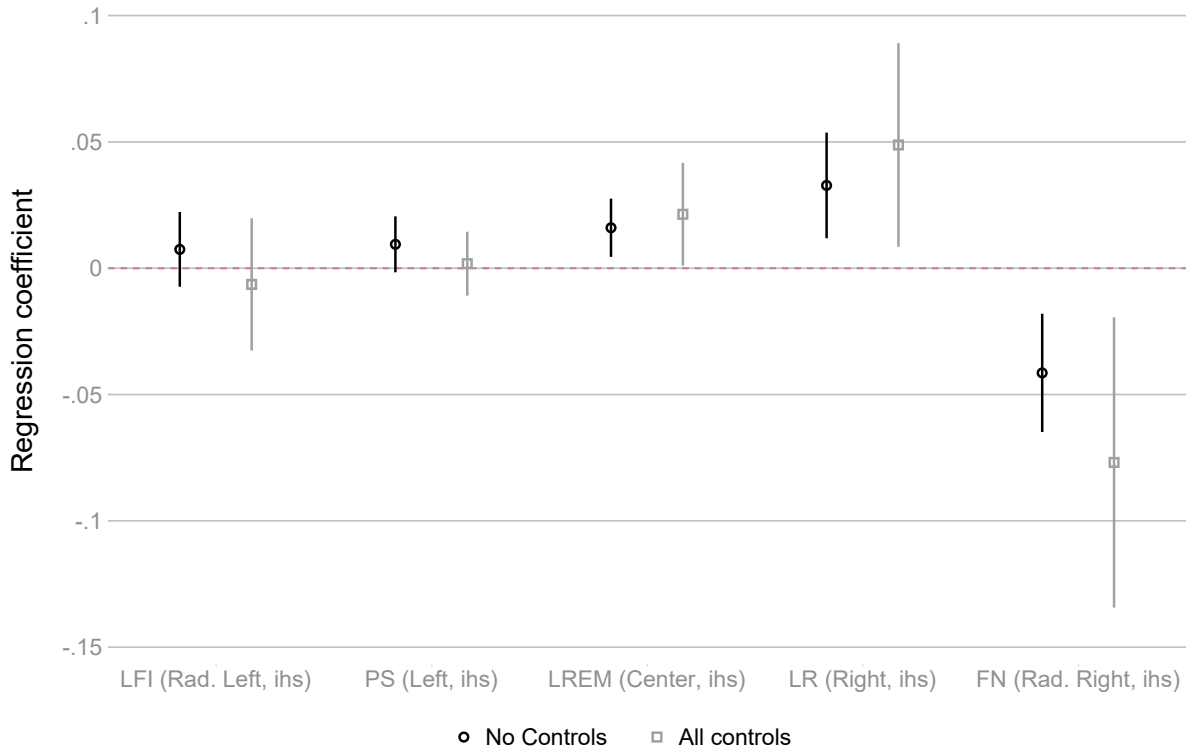
Introducing municipality fixed effects allows us to investigate the impact of the change in far-right vote between 2012 and 2017 on the change in tax-declared charitable donations between these two dates. Standard errors are clustered at the city level and we use as before the IHS transformation of both the share of donors and the vote shares.

Figure 3.5 reports the results.²⁹ A 10% increase in the vote for Le Pen compared to abstention is associated with a 0.4% decrease in the share of households making a charitable donations, holding all other votes constant. The point estimate is statistically significant, but smaller in magnitude than the one we obtain with the cross-sectional estimations, where a 10% increase in the vote for Le Pen is associated with a decrease of about 2% in the share of households that donated. This is robust to adding the full set of controls and looking at the extensive margin only (see online Appendix figure 3.24) Thus, while people stop donating when they start voting for far-right candidates, it seems that some differences in donation patterns between the Le Pen voting cities and other cities are driven by unobservable traits that have intensified the far-right donation gap in recent years.

5.2 Intensification over time

We next turn to our survey data to decompose the 2022 far-right donation gap depending on the respondent’s reported vote in 2017. In Table 3.5, we divide the respondents who report a Le Pen vote in 2022 into “converters” and “faithfuls” depending on whether they

²⁹See online appendix Table 3.26 for the associated regression table.



Notes: The figure reports with 95% confidence intervals the results of the estimation of equation (3.4) (standard errors are clustered at the municipality level.), using an OLS model. An observation is a city/election and the corresponding regression coefficients are reported in the online Appendix Table 3.26. Error bars show 95% confidence intervals. The omitted category is abstention and the estimation control for election and municipality fixed effects.

Figure 3.5: The far-right donation gap, controlling for election and municipality fixed effects

already voted for Le Pen in 2017.³⁰ It is the Le Pen “faithfuls”³¹ who are significantly less likely to report a donation: they are on average 6.7 percentage points less likely to have reported a donation compared to those who abstained, while those who used to vote for other candidates – regardless of which ones – do not report that they donate significantly less than those who abstained (although the point estimate is much smaller than the one observed for other candidates). This hints at an intensifying far-right donation gap for voters with a longer history of voting for far-right movements.

To investigate this further, we next look at the unexplainable share of the drop in the share of households that donate to charities in the administrative tax data depending on the early far-right vote in 2012. We residualize the share of households that donates in the yearly municipality panel with the time-varying municipality-level characteristics and divide municipalities by tercile of their Le Pen vote in 2012. Next, we index the residuals within group by their 2012 level to make changes in residuals over time more comparable.

Figure 3.6 shows the temporal evolution of the indexed residuals by the 2012 Le Pen vote tercile. The overall downward trend already shown in Figure 3.1a in the share of households that declare charitable donations affects all municipalities. However, the drop is much more pronounced for municipalities that voted relatively more for the far right in 2012. As mentioned, this “intensification” cannot be explained by changes in local socio-economic characteristics that are residualized here. Instead, the far-right vote from the early wave of its success (in France) is becoming increasingly important for explaining the decline of the propensity to donate. As many countries have experienced a rise in far-right voting in recent years, this suggests that a similar “intensification” of the far-right electorate’s negative attitudes poses a threat to the donor base on which the charitable sector depends.

6 Conclusion

Can the observed drop in the share of charitable donors be linked to the electoral rise of the far right? In this paper, we take advantage of the French presidential elections to conduct a large-scale survey in order to better understand the drivers of charitable giving. We document a lower propensity of far-right supporters to contribute to charities, which we confirm by combining administrative tax data on donations with electoral results at the

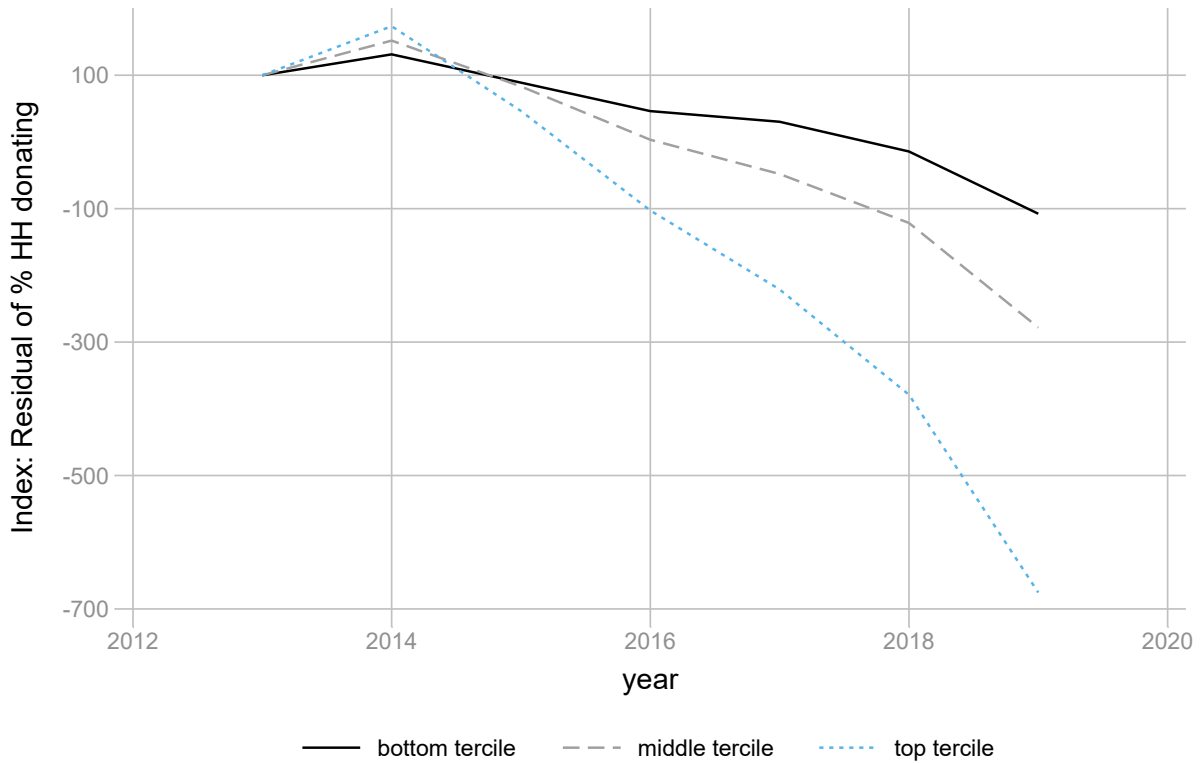
³⁰Summary statistics are reported in the online Appendix Table 3.31: about 60% of Le Pen supporters in 2022 already voted for her in 2017, 4% voted from other far-right candidates, while the others come from a wide range of other positions in the political spectrum (about 7% from the Left, 5% from Macron, 11% from the right, while 7% abstained in 2007).

³¹Here defined as those who voted for Le Pen or another far-right candidate (Dupont-Aignan) in 2017.

Table 3.5: The 2022 far-right donation gap depending on the reported vote in 2017

	(1)	(2)
	Donated	Donated
Other Left	0.126*** (0.023)	0.126*** (0.023)
Mélenchon	0.035 (0.023)	0.035 (0.023)
Macron	0.034 (0.022)	0.034 (0.022)
Other Right	0.042 (0.024)	0.042 (0.024)
Le Pen Converters	0.002 (0.025)	
Le Pen Faithfuls	-0.076*** (0.023)	-0.076*** (0.023)
Zemmour	-0.026 (0.025)	-0.026 (0.025)
Le Pen Converter - Left		-0.003 (0.045)
Le Pen Converter - Macron		-0.054 (0.052)
Le Pen Converter - Right		0.020 (0.032)
Le Pen Converters - Abst		-0.000 (0.037)
Controls	Yes	Yes
N	10,755	10,755
Dep. mean	0.44	0.44
Dep. SD	0.50	0.50

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level between parentheses). An observation is a city and all the specifications include department fixed effects.



Notes: The residuals are obtained from regressing the share of households donating in a municipality on the full set of controls described in Table 3.9. The residuals average to zero over by construction and are indexed to the 2013 average.

Figure 3.6: Trends in the residualised share of households donating (in the administrative tax data) depending on the 2012 vote for Le Pen

local level. According to our findings, the rise of the far-right leads not only to a change in reported donations but also changes the actual behavior of far-right supporters who donate less to charities. Using a number of different empirical strategies, we provide suggestive evidence that this far-right donation gap is most probably linked to a higher demand for communal morality.

Our findings imply a potential (unanticipated) drop in charities' resources in the years to come. Further, not only might the rise in support for far-right politicians pose a threat to the total revenue of the charitable sector through a change in social norms, but also a shrinking base of supporters for charities that may trigger a debate about the democratic legitimacy of the large tax breaks that support them in many countries and thus pose a threat to the charitable sector as a whole.

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A Appendices

A.1 Additional Tables

Table 3.6: Summary statistics: Socio-demographic characteristics of the surveyed individuals

	Mean	St.Dev
Demographics		
=1 if woman	0.52	0.50
Age	50	18
=1 if married/civ. union	0.50	0.50
=1 if College graduate	0.54	0.50
Profession		
=1 if Senior executive	0.10	0.31
=1 if Intermediate profession	0.16	0.36
=1 if Employee	0.17	0.38
=1 if Worker	0.12	0.33
=1 if Retired	0.29	0.45
Location		
=1 if lives in rural area	0.23	0.42
=1 if lives in urban area	0.40	0.49
=1 if lives in the Paris region	0.17	0.38
Religion		
=1 if No religion	0.42	0.49
=1 if Catholic	0.51	0.50
=1 if Muslim	0.03	0.16
Income		
Below €1,250	0.10	0.30
€11,250-€9,999	0.20	0.40
€9,000-€2,499	0.14	0.34
€2,500-€3,499	0.22	0.41
€3,500-€4,999	0.18	0.39
Above €5,000	0.08	0.27
Observations	12,600	

Notes: The table reports summary statistics for the surveyed individuals as part of the *Enquête Electorale Française* (see the text for more details). An observation is an individual.

Table 3.7: Summary statistics: Political preferences of the surveyed individuals

	Mean	St.Dev
2022 elections		
=1 if intended vote E. Macron 2022, 1st round	0.23	0.42
=1 if intended vote M. Le Pen 2022, 1st round	0.20	0.40
=1 if intended vote JL. Melenchon 2022, 1st round	0.15	0.36
=1 if intended vote E. Zemmour 2022, 1st round	0.09	0.28
2017 elections		
=1 voted E. Macron 2017, 1st round	0.20	0.40
=1 if voted M. Le Pen 2017, 1st round	0.18	0.38
=1 voted JL. Melenchon 2017, 1st round	0.16	0.37
Preferences		
Self-reported political preference (0 (left) to 10 (right))	5.63	2.51
Observations	12,600	

Notes: The table reports summary statistics for the surveyed individuals as part of the *Enquête Electorale Française* (see the text for more details). An observation is an individual.

Table 3.8: Summary statistics: Subjective Well-being and Trust of the surveyed individuals

	Mean	Median	SD	Min	Max	N
<i>Life Satisfaction</i>						
Overall life satisfaction	5.91	6	2	0	10	12,600
<i>Political Trust</i>						
Trust in: the President	2.62	2	1	1	4	10,785
Trust in: Deputies	2.82	3	1	1	4	10,782
Trust in: the Mayor of my city	2.32	2	1	1	4	10,784
Trust in: Media	2.91	3	1	1	4	10,783
Trust in: Political Parties	3.13	3	1	1	4	10,782
<i>Social Trust</i>						
Trust in: Family Members	1.34	1	1	1	4	10,785
Trust in: People I personally know	1.57	2	1	1	4	10,785
Trust in: People I meet for the first time	2.77	3	1	1	4	10,779
Trust in: People of different nationalities	2.25	2	1	1	4	10,775
Trust in: People of different religions	2.21	2	1	1	4	10,772

Notes: The table reports summary statistics for the surveyed individuals as part of the *Enquête Electorale Française* (see the text for more details). An observation is an individual.

Table 3.9: Summary statistics: municipality level data

	Mean	SD	Min	Max	N
Tax-deducted donations					
Share of fiscal HHs declaring a charitable donation	12.12	4.85	0.69	53.79	189,491
Amount of charitable donations declared	41.61	56.85	1.55	13133.34	189,491
Demographics					
Population > 14 year-old	2,157	19,102	2	3,755,778	251,367
Share of women	0.50	0.03	0.17	0.76	251,357
Share of population above 25 years old	0.11	0.03	0.01	0.50	250,112
Income					
median annual income	20638.70	3,414.17	14076.00	41460.00	224,298
GINI	0.32	0.17	0.21	9.52	36,513
Share of population below 60 % of median income	19.39	9.33	5.00	71.00	34,657
Foreigners					
Share of the population that is foreigner	0.05	0.05	0.00	0.92	256,732
Share of foreigners that are unemployed	0.08	0.12	0.00	1.00	238,967
Employment					
Unemployment rate	0.11	0.05	0.01	0.67	248,485
Share of the active pop working in agriculture	0.07	0.11	0.00	1.00	251,048
Share of the active pop working in public administration	0.02	0.05	0.00	0.99	251,280
Share of white-collar workers	0.07	0.10	0.00	1.00	240,659
Share of blue-collar workers	0.24	0.20	0.00	1.00	240,659
Education					
Share of the adult population with a bachelor	0.30	0.15	0.00	1.00	249,912
Share of the adult population with a Master degree	0.14	0.12	0.00	1.00	249,912
Other admin tax data information					
Reference tax income of tax households	23215.78	73136.43	404.51	1,598,188.52	246,704
Total net tax	1,397.81	5,596.12	5.42	128,336.21	246,704
# of retirees	322	923	11	18,934	246,201
Total pensions	7,399.65	21778.37	204.04	467,093.34	244,193
Charities					
Stock Charities Per 1,000 Inhabitants	0.49	1.69	0.00	142.86	209,640
New Charities Per 1,000 Inhabitants	0.03	0.34	0.00	40.00	209,640
Global Charities per 1,000 inhabitants	0.08	0.56	0.00	32.26	209,640
Percentage Global, Stock	0.17	0.31	0.00	1.00	57,978

Notes: The table reports summary statistics for the city-level observable (see the text for more details). An observation is a city/year. Data on tax-deducted donations are from [Cagé and Guillot \(2021\)](#); election results in the 2012 and 2017 presidential elections are from the *Ministère de l'Intérieur*; data on the supply of charities were built from the National Directory of Associations ("*Répertoire National des Association*") provided by the *Ministère de l'Intérieur*; other control variables such as demographics, income, foreigners, employment and education come from census data provided by the *Institut national de la statistique et des études économiques*.

Table 3.10: Summary statistics: Donations received by Action Contre la Faim, Oxfam and SOS Méditerranée

	Mean	SD	Min	Max	N
2012					
Total amount donated to ACF (€/hh)	0.69	1.86	0.00	219.03	177,186
Total amount donated to Oxfam (€/hh)	0.03	0.20	0.00	18.10	177,186
Total amount donated to SOSM (€/hh)	0
2017					
Total amount donated to ACF (€/hh)	0.69	1.71	0.00	236.08	177,341
Total amount donated to Oxfam (€/hh)	0.03	0.29	0.00	29.17	177,341
Total amount donated to SOSM (€/hh)	0.05	0.84	0.00	194.44	177,341
2022					
Total amount donated to ACF (€/hh)	0.65	1.72	0.00	202.49	104,636
Total amount donated to Oxfam (€/hh)	0.06	0.37	0.00	29.17	104,636
Total amount donated to SOSM (€/hh)	0.08	1.06	0.00	194.44	104,636
Total					
Total amount donated to ACF (€/hh)	0.68	1.78	0.00	236.08	459,163
Total amount donated to Oxfam (€/hh)	0.04	0.28	0.00	29.17	459,163
Total amount donated to SOSM (€/hh)	0.06	0.93	0.00	194.44	281,977

Notes: The table reports summary statistics for the donations received by the three charities – Action Contre la Faim, Oxfam and SOS Méditerranée – for which we have donation-level information (see the text for more details). Each observation is a city/year. The summary statistics are aggregated around the closest Presidential elections: the 2012 Presidential elections for the 2010-2014 donations, the 2017 elections for the 2015-2019 donations, and the 2022 elections for the 2020-2022 donations. Data on donations were provided to us directly by the charities.

Table 3.11: The far-right donation gap: Evidence from self-reported donations (2022 electoral survey)

	Donated to charity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mélenchon	0.09*** (0.02)	0.10*** (0.02)	0.10*** (0.02)	0.09*** (0.02)	0.09*** (0.02)	0.05** (0.02)	0.04 (0.02)	0.03 (0.02)
Other Left	0.21*** (0.02)	0.19*** (0.02)	0.18*** (0.02)	0.19*** (0.02)	0.18*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.13*** (0.02)
Macron	0.20*** (0.02)	0.15*** (0.02)	0.13*** (0.02)	0.12*** (0.02)	0.11*** (0.02)	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)
Other Right	0.18*** (0.02)	0.11*** (0.02)	0.09*** (0.02)	0.08*** (0.02)	0.08*** (0.02)	0.04* (0.02)	0.04* (0.02)	0.04* (0.02)
Dupont-Aignan	0.03 (0.04)	0.01 (0.04)	0.00 (0.04)	-0.00 (0.04)	-0.00 (0.04)	-0.00 (0.04)	0.01 (0.04)	0.01 (0.04)
Le Pen	-0.04* (0.02)	-0.05*** (0.02)	-0.05*** (0.02)	-0.06*** (0.02)	-0.06*** (0.02)	-0.06*** (0.02)	-0.04** (0.02)	-0.04** (0.02)
Zemmour	0.02 (0.02)	-0.02 (0.02)	-0.03 (0.02)	-0.04* (0.02)	-0.04* (0.02)	-0.04 (0.02)	-0.02 (0.03)	-0.02 (0.03)
Demographics		✓	✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓	✓
Religion				✓	✓	✓	✓	✓
Life satisfaction					✓	✓	✓	✓
Trust on Pol.						✓	✓	✓
Trust on Society							✓	✓
City Controls								✓
Observations	12,600	12,600	12,600	12,600	12,600	10,778	10,755	10,755
Mean DepVar	0.43	0.43	0.43	0.43	0.43	0.44	0.44	0.44
Sd DepVar	0.49	0.49	0.49	0.49	0.49	0.50	0.50	0.50

Notes: * p<0.10, ** p<0.05, *** p<0.01. Models are estimated using an OLS (robust standard errors in parentheses). An observation is an individual. Our sample of analysis include all the surveyed individuals who are part of the part of the 2022 French Electoral Survey ($N = 12,600$; the lower number of observations in Columns (6) to (8) comes from the fact that some individuals did not answer the questions on trust). The dependent variable is an indicator variable equal to one if the respondent reports that she has made a charitable donation in the past 12 months, and to zero otherwise. The main explanatory variable is a vector of indicator variables that represent the candidate that the respondent intends to vote for in the 2022 presidential elections. The omitted category is abstention. The "other left" candidates include the candidate from the French communist party (Fabien Roussel, 2.28%) of the votes, the candidate of the *Nouveau Parti Anticapitaliste* (Philippe Poutou, 0.77%), the candidate of the Socialist party (Anne Hidalgo, 1.75%), and the candidate of *Lutte Ouvrière* (Nathalie Arthaud, 0.56%). The "other right" candidates include the candidate of *Les Républicains* (Valérie Pécresse, 4.78%). More details are provided in the text.

Table 3.12: Far-right Donation Gap: Self-reported donations, On Self-Reported Left-Right Scale

	Donated to charity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0 - Very to the Left	0.02 (0.04)	0.05 (0.04)	0.07* (0.04)	0.07* (0.04)	0.08** (0.04)	0.06 (0.04)	0.04 (0.04)	0.06 (0.05)
1	0.08** (0.04)	0.08** (0.04)	0.08** (0.04)	0.09** (0.04)	0.09** (0.04)	0.10*** (0.04)	0.09** (0.04)	0.08* (0.04)
2	0.08*** (0.02)	0.08*** (0.02)	0.08*** (0.02)	0.09*** (0.02)	0.09*** (0.02)	0.08*** (0.03)	0.07*** (0.03)	0.03 (0.03)
3	0.09*** (0.02)	0.10*** (0.02)	0.10*** (0.02)	0.10*** (0.02)	0.10*** (0.02)	0.11*** (0.02)	0.10*** (0.02)	0.08*** (0.02)
4	0.14*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.11*** (0.02)	0.10*** (0.02)	0.10*** (0.02)
6	0.10*** (0.02)	0.08*** (0.02)	0.07*** (0.02)	0.07*** (0.02)	0.06*** (0.02)	0.05*** (0.02)	0.06*** (0.02)	0.06*** (0.02)
7	0.09*** (0.02)	0.06*** (0.02)	0.05** (0.02)	0.04** (0.02)	0.03* (0.02)	0.03* (0.02)	0.04** (0.02)	0.04* (0.02)
8	0.05*** (0.02)	0.02 (0.02)	0.00 (0.02)	-0.01 (0.02)	-0.01 (0.02)	0.00 (0.02)	0.01 (0.02)	-0.00 (0.02)
9	0.02 (0.03)	-0.01 (0.02)	-0.02 (0.02)	-0.02 (0.02)	-0.03 (0.02)	0.00 (0.03)	0.02 (0.03)	-0.00 (0.03)
10 - Very to the Right	-0.10*** (0.02)	-0.09*** (0.02)	-0.09*** (0.02)	-0.09*** (0.02)	-0.09*** (0.02)	-0.03 (0.03)	-0.01 (0.03)	-0.02 (0.03)
99 - No Response	-0.14*** (0.02)	-0.12*** (0.02)	-0.12*** (0.02)	-0.11*** (0.02)	-0.11*** (0.02)	-0.08*** (0.02)	-0.07*** (0.02)	-0.06** (0.03)
Demographics		✓	✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓	✓
Religion				✓	✓	✓	✓	✓
Life satisfaction					✓	✓	✓	✓
Trust on Pol.						✓	✓	✓
Trust on Society							✓	✓
City Controls								✓
Observations	12,600	12,600	12,600	12,600	12,600	10,778	10,755	8,138
Mean DepVar	0.43	0.43	0.43	0.43	0.43	0.44	0.44	0.45
Sd DepVar	0.49	0.49	0.49	0.49	0.49	0.50	0.50	0.50

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (robust standard errors in parentheses). An observation is an individual. Our sample of analysis include all the surveyed individuals who are part of the part of the 2022 French Electoral Survey ($N = 12,600$; the lower number of observations in Columns (6) to (8) comes from the fact that some individuals did not answer the questions on trust). The dependent variable is an indicator variable equal to one if the respondent reports that she has made a charitable donation in the past 12 months, and to zero otherwise. The main explanatory variable is a vector of indicator variables that represent the respondent's self-reported position on the political spectrum from 0 (very on the left) to 10 (very on the right). The omitted category is 5. 6.9% of the respondents choose to not respond to the question. We are unable to observe what they would have chosen, but we can observe the intended vote for this group. Among those who choose not to answer, 28.9% intended to vote Le Pen, 25.4% will choose abstention, 16.5% intended to vote Macron, 9.1% other right-wing candidates, 9.0% other left-wing candidates and 5.2% Zemmour.

Table 3.13: Far-right Donation Gap, Robustness Checks: Logit Regression, Self-reported donations, French Election Panel, 2022

	Donated to charity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Donated								
Other Left	2.37*** (0.22)	2.28*** (0.21)	2.23*** (0.21)	2.25*** (0.21)	2.22*** (0.21)	1.80*** (0.19)	1.76*** (0.18)	1.73*** (0.22)
Mélenchon	1.49*** (0.14)	1.55*** (0.14)	1.53*** (0.14)	1.51*** (0.14)	1.50*** (0.14)	1.26** (0.13)	1.18 (0.13)	1.12 (0.14)
Macron	2.26*** (0.19)	1.90*** (0.16)	1.75*** (0.15)	1.70*** (0.14)	1.60*** (0.14)	1.13 (0.11)	1.14 (0.12)	1.11 (0.13)
Other Right	2.13*** (0.21)	1.60*** (0.16)	1.49*** (0.15)	1.43*** (0.15)	1.40*** (0.14)	1.18 (0.13)	1.20 (0.13)	1.11 (0.15)
Le Pen	0.85* (0.08)	0.79*** (0.07)	0.78*** (0.07)	0.76*** (0.07)	0.76*** (0.07)	0.76*** (0.08)	0.81** (0.08)	0.75** (0.09)
Zemmour	1.08 (0.11)	0.91 (0.10)	0.86 (0.09)	0.82* (0.09)	0.82* (0.09)	0.84 (0.10)	0.89 (0.11)	0.80 (0.11)
Demographics		✓	✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓	✓
Religion				✓	✓	✓	✓	✓
Life satisfaction					✓	✓	✓	✓
Trust on Pol.						✓	✓	✓
Trust on Society							✓	✓
City Controls								✓
Observations	12,600	12,600	12,600	12,600	12,600	10,778	10,755	8,135
Mean DepVar	0.43	0.43	0.43	0.43	0.43	0.44	0.44	0.45
Sd DepVar	0.49	0.49	0.49	0.49	0.49	0.50	0.50	0.50

Exponentiated coefficients

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using a logit regression and we report odd ratios (robust standard errors in parentheses). An observation is an individual. Our sample of analysis include all the surveyed individuals who are part of the part of the 2022 French Electoral Survey ($N = 12,600$; the lower number of observations in Columns (6) to (8) comes from the fact that some individuals did not answer the questions on trust). The dependent variable is an indicator variable equal to one if the respondent reports that she has made a charitable donation in the past 12 months, and to zero otherwise. The main explanatory variable is a vector of indicator variables that represent the candidate that the respondent intends to vote for in the 2022 presidential elections. The omitted category is abstention. The “other left” candidates include the candidate from the French communist party (Fabien Roussel, 2.28%) of the votes, the candidate of the *Nouveau Parti Anticapitaliste* (Philippe Poutou, 0.77%), the candidate of the Socialist party (Anne Hidalgo, 1.75%), and the candidate of *Lutte Ouvrière* (Nathalie Arthaud, 0.56%). The “other right” candidates include the candidate of *Les Républicains* (Valérie Pécresse, 4.78%). More details are provided in the text.

Table 3.14: Far-right Donation Gap, Robustness Checks: Probit Regression, Self-reported donations, French Election Panel, 2022

	Donated to charity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Donated to charity								
Mélenchon	1.28*** (0.07)	1.31*** (0.08)	1.31*** (0.08)	1.30*** (0.08)	1.29*** (0.07)	1.16** (0.07)	1.11 (0.07)	1.11 (0.07)
Other Left	1.71*** (0.10)	1.67*** (0.10)	1.65*** (0.10)	1.66*** (0.10)	1.64*** (0.10)	1.44*** (0.09)	1.42*** (0.09)	1.42*** (0.09)
Macron	1.66*** (0.08)	1.49*** (0.08)	1.42*** (0.07)	1.39*** (0.07)	1.34*** (0.07)	1.09 (0.07)	1.09 (0.07)	1.09 (0.07)
Other Right	1.60*** (0.10)	1.34*** (0.08)	1.29*** (0.08)	1.26*** (0.08)	1.24*** (0.08)	1.12 (0.08)	1.12* (0.08)	1.12* (0.08)
Dupont-Aignan	1.09 (0.12)	1.04 (0.11)	1.02 (0.11)	1.00 (0.11)	1.01 (0.11)	0.99 (0.11)	1.02 (0.12)	1.02 (0.12)
Le Pen	0.90* (0.05)	0.87** (0.05)	0.86*** (0.05)	0.85*** (0.05)	0.85*** (0.05)	0.85*** (0.05)	0.89* (0.06)	0.89* (0.06)
Zemmour	1.05 (0.07)	0.95 (0.06)	0.92 (0.06)	0.89* (0.06)	0.89* (0.06)	0.90 (0.07)	0.93 (0.07)	0.93 (0.07)
Demographics		✓	✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓	✓
Religion				✓	✓	✓	✓	✓
Life satisfaction					✓	✓	✓	✓
Trust on Pol.						✓	✓	✓
Trust on Society							✓	✓
City Controls								✓
Observations	12,600	12,600	12,600	12,600	12,600	10,778	10,755	10,755
Mean DepVar	0.43	0.43	0.43	0.43	0.43	0.44	0.44	0.44
Sd DepVar	0.49	0.49	0.49	0.49	0.49	0.50	0.50	0.50

Exponentiated coefficients

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using a probit regression and we report odd ratios (robust standard errors in parentheses). An observation is an individual. Our sample of analysis include all the surveyed individuals who are part of the part of the 2022 French Electoral Survey ($N = 12,600$; the lower number of observations in Columns (6) to (8) comes from the fact that some individuals did not answer the questions on trust). The dependent variable is an indicator variable equal to one if the respondent reports that she has made a charitable donation in the past 12 months, and to zero otherwise. The main explanatory variable is a vector of indicator variables that represent the candidate that the respondent intends to vote for in the 2022 presidential elections. The omitted category is abstention. The "other left" candidates include the candidate from the French communist party (Fabien Roussel, 2.28%) of the votes, the candidate of the *Nouveau Parti Anticapitaliste* (Philippe Poutou, 0.77%), the candidate of the Socialist party (Anne Hidalgo, 1.75%), and the candidate of *Lutte Ouvrière* (Nathalie Arthaud, 0.56%). The "other right" candidates include the candidate of *Les Républicains* (Valérie Pécresse, 4.78%). More details are provided in the text.

Table 3.15: Far-right Donation Gap, Robustness Checks: Intended donations Next Year, French Election Panel, 2022

	Donated to charity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mélenchon	0.11*** (0.02)	0.11*** (0.02)	0.11*** (0.02)	0.10*** (0.02)	0.10*** (0.02)	0.07*** (0.02)	0.05** (0.02)	0.05** (0.02)
Other Left	0.23*** (0.02)	0.22*** (0.02)	0.21*** (0.02)	0.21*** (0.02)	0.21*** (0.02)	0.17*** (0.02)	0.16*** (0.02)	0.16*** (0.02)
Macron	0.20*** (0.02)	0.16*** (0.02)	0.14*** (0.02)	0.13*** (0.02)	0.12*** (0.02)	0.06*** (0.02)	0.06*** (0.02)	0.06*** (0.02)
Other Right	0.16*** (0.02)	0.09*** (0.02)	0.08*** (0.02)	0.07*** (0.02)	0.06*** (0.02)	0.04* (0.02)	0.05** (0.02)	0.05** (0.02)
Dupont-Aignan								
Le Pen	-0.05** (0.02)	-0.06*** (0.02)	-0.06*** (0.02)	-0.07*** (0.02)	-0.07*** (0.02)	-0.07*** (0.02)	-0.05** (0.02)	-0.05** (0.02)
Zemmour	0.01 (0.02)	-0.03 (0.02)	-0.04* (0.02)	-0.05** (0.02)	-0.05** (0.02)	-0.02 (0.02)	-0.00 (0.02)	-0.00 (0.02)
Demographics		✓	✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓	✓
Religion				✓	✓	✓	✓	✓
Life satisfaction					✓	✓	✓	✓
Trust on Pol.						✓	✓	✓
Trust on Society							✓	✓
City Controls								✓
Observations	12,600	12,600	12,600	12,600	12,600	10,778	10,755	10,755
Mean DepVar	0.41	0.41	0.41	0.41	0.41	0.43	0.43	0.43
Sd DepVar	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (robust standard errors in parentheses). An observation is an individual. Our sample of analysis include all the surveyed individuals who are part of the part of the 2022 French Electoral Survey ($N = 12,600$; the lower number of observations in Columns (6) to (8) comes from the fact that some individuals did not answer the questions on trust). The dependent variable is an indicator variable equal to one if the respondent reports that she will make a charitable donation in the next 12 months, and to zero otherwise. The main explanatory variable is a vector of indicator variables that represent the candidate that the respondent intends to vote for in the 2022 presidential elections. The omitted category is abstention. The "other left" candidates include the candidate from the French communist party (Fabien Roussel, 2.28%) of the votes, the candidate of the *Nouveau Parti Anticapitaliste* (Philippe Poutou, 0.77%), the candidate of the Socialist party (Anne Hidalgo, 1.75%), and the candidate of *Lutte Ouvrière* (Nathalie Arthaud, 0.56%). The "other right" candidates include the candidate of *Les Républicains* (Valérie Pécresse, 4.78%). More details are provided in the text.

Table 3.16: External Validity: Results from the German Socio-Economic Panel (SOEP)

	Donated		
	(1) Donation	(2) Donated Blood	(3) Donated for Refugees
LINKE	0.12*** (0.01)	0.02*** (0.01)	0.10*** (0.01)
SPD	0.10*** (0.01)	0.03*** (0.00)	0.08*** (0.01)
Grüne	0.25*** (0.01)	0.04*** (0.01)	0.19*** (0.01)
FDP	0.14*** (0.01)	0.02** (0.01)	0.05*** (0.01)
CDU/CSU	0.15*** (0.01)	0.03*** (0.00)	0.05*** (0.01)
AfD	-0.01 (0.01)	-0.00 (0.01)	-0.06*** (0.01)
Controls	✓	✓	✓
Year FE	✓	✓	✓
State FE	✓	✓	✓
Observations	93,705	51,250	75,716
Mean DepVar	0.44	0.11	0.23
Sd DepVar	0.50	0.32	0.42

Notes: The plotted coefficients are reported in table 3.16. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at individual level (panel data). The dependent variable is a dummy for having reported a donation in the survey and the independent variable is the individual's reported party preference. The omitted category is abstention. Controls include year and state FE, demographics (gender, marital status), log of income, trust for society in general, religion and employment status.

Table 3.17: Far-right Donation Gap: Tax-declared donations, 2012 Election and 2013-2016 Donation, Elasticity

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	0.01 (0.01)	0.01 (0.01)	-0.01 (0.01)	-0.01* (0.01)	-0.01 (0.01)	-0.02** (0.01)	-0.02*** (0.01)
Hollande (Left, ihs)	0.18*** (0.03)	0.18*** (0.03)	0.08*** (0.02)	0.08*** (0.02)	0.08*** (0.02)	0.07*** (0.02)	0.05*** (0.02)
Bayrou (Centre, ihs)	0.22*** (0.01)	0.21*** (0.01)	0.13*** (0.01)	0.12*** (0.01)	0.12*** (0.01)	0.11*** (0.01)	0.11*** (0.01)
Sarkozy (Right, ihs)	0.33*** (0.03)	0.32*** (0.03)	0.15*** (0.02)	0.14*** (0.02)	0.13*** (0.02)	0.13*** (0.02)	0.09*** (0.02)
Dupont-Aignan (Rad. Right, ihs)	0.00 (0.01)	0.00 (0.01)	-0.01* (0.00)	-0.01*** (0.00)	-0.01*** (0.00)	-0.01** (0.00)	-0.00 (0.00)
Le Pen (Rad. Right, ihs)	-0.20*** (0.02)	-0.20*** (0.02)	-0.17*** (0.02)	-0.20*** (0.02)	-0.19*** (0.02)	-0.17*** (0.02)	-0.14*** (0.01)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	29,989	29,989	29,989	29,989	29,989	29,989	29,989
Mean DepVar	3.18	3.18	3.18	3.18	3.18	3.18	3.18
Sd DepVar	0.35	0.35	0.35	0.35	0.35	0.35	0.35

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of (the IHS transformation of) votes obtained by presidential candidates in the 2012 and 2017 election as share of the total electorate (omitting abstention). The dependent variable is the IHS transformation of share of households deducting a charitable donation in tax returns. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.18: Far-right Donation Gap: Tax-declared donations, 2017 Election and 2017-2019 Donation, Elasticity

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	0.03*	0.03	-0.00	-0.01	0.01	-0.00	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Hammon (Left, ihs)	0.07***	0.07***	0.05***	0.04***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Macron (Centre, ihs)	0.34***	0.34***	0.15***	0.15***	0.15***	0.14***	0.12***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Fillon (Right, ihs)	0.35***	0.34***	0.20***	0.19***	0.18***	0.17***	0.13***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Dupont-Aignan (Rad. Right, ihs)	0.07***	0.06***	0.02**	0.01*	0.01*	0.01*	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Le Pen (Rad. Right, ihs)	-0.16***	-0.17***	-0.19***	-0.23***	-0.22***	-0.20***	-0.16***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	27,561	27,561	27,561	27,561	27,561	27,561	27,561
Mean DepVar	3.05	3.05	3.05	3.05	3.05	3.05	3.05
Sd DepVar	0.36	0.36	0.36	0.36	0.36	0.36	0.36

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of (the IHS transformation of) votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the IHS transformation of share of households deducting a charitable donation in tax returns. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.19: Far-right Donation Gap: Tax-declared donations, 2012 Election and 2013-2016 Donation, Shares

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left)	0.17*** (0.01)	0.16*** (0.01)	0.10*** (0.01)	0.09*** (0.01)	0.09*** (0.01)	0.08*** (0.01)	0.04*** (0.01)
Hollande (Left)	0.26*** (0.02)	0.26*** (0.02)	0.16*** (0.01)	0.15*** (0.01)	0.15*** (0.01)	0.14*** (0.01)	0.10*** (0.01)
Bayrou (Centre)	0.48*** (0.02)	0.48*** (0.02)	0.33*** (0.02)	0.31*** (0.02)	0.30*** (0.01)	0.28*** (0.01)	0.26*** (0.01)
Sarkozy (Right)	0.33*** (0.01)	0.32*** (0.01)	0.20*** (0.01)	0.18*** (0.01)	0.18*** (0.01)	0.17*** (0.01)	0.12*** (0.01)
Dupont-Aignan (Rad. Right)	0.19*** (0.04)	0.18*** (0.03)	0.08*** (0.03)	0.06** (0.03)	0.06* (0.03)	0.06** (0.03)	0.05** (0.03)
Le Pen (Rad. Right)	0.01 (0.02)	0.01 (0.02)	-0.02 (0.02)	-0.05*** (0.01)	-0.05*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	29,989	29,989	29,989	29,989	29,989	29,989	29,989
Mean DepVar	12.75	12.75	12.75	12.75	12.75	12.75	12.75
Sd DepVar	4.78	4.78	4.78	4.78	4.78	4.78	4.78

Notes: * p<0.10, ** p<0.05, *** p<0.01. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2012 election as share of the total electorate (omitting abstention). The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.20: Far-right Donation Gap: Tax-declared donations, 2017 Election and 2017-2019 Donation, Shares

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left)	0.15*** (0.01)	0.14*** (0.01)	0.08*** (0.01)	0.07*** (0.01)	0.08*** (0.01)	0.07*** (0.01)	0.04*** (0.01)
Hammon (Left)	0.36*** (0.02)	0.35*** (0.02)	0.27*** (0.02)	0.24*** (0.02)	0.24*** (0.02)	0.22*** (0.02)	0.17*** (0.01)
Macron (Centre)	0.34*** (0.01)	0.34*** (0.01)	0.20*** (0.02)	0.18*** (0.01)	0.19*** (0.01)	0.18*** (0.01)	0.14*** (0.01)
Fillon (Right)	0.36*** (0.02)	0.34*** (0.02)	0.23*** (0.02)	0.22*** (0.01)	0.20*** (0.01)	0.20*** (0.01)	0.15*** (0.01)
Dupont-Aignan (Rad. Right)	0.28*** (0.03)	0.27*** (0.03)	0.17*** (0.03)	0.14*** (0.03)	0.14*** (0.03)	0.13*** (0.03)	0.13*** (0.02)
Le Pen (Rad. Right)	0.03*** (0.01)	0.02* (0.01)	-0.02 (0.02)	-0.05*** (0.01)	-0.05*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	27,561	27,561	27,561	27,561	27,561	27,561	27,561
Mean DepVar	11.27	11.27	11.27	11.27	11.27	11.27	11.27
Sd DepVar	4.44	4.44	4.44	4.44	4.44	4.44	4.44

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.21: Far-right Donation Gap: Tax-declared donations, 2012 Election and Sum of Donation 2013-2016, ihs

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	0.07*** (0.02)	0.08*** (0.03)	0.04** (0.02)	0.04* (0.02)	0.03* (0.02)	0.02 (0.02)	-0.01 (0.02)
Hollande (Left, ihs)	0.52*** (0.06)	0.49*** (0.06)	0.27*** (0.04)	0.26*** (0.04)	0.24*** (0.04)	0.22*** (0.04)	0.08** (0.04)
Bayrou (Centre, ihs)	0.32*** (0.02)	0.34*** (0.02)	0.19*** (0.02)	0.19*** (0.02)	0.20*** (0.02)	0.18*** (0.02)	0.17*** (0.02)
Sarkozy (Right, ihs)	0.55*** (0.05)	0.56*** (0.06)	0.24*** (0.04)	0.24*** (0.04)	0.25*** (0.04)	0.23*** (0.04)	0.14*** (0.04)
Dupont-Aignan (Rad. Right, ihs)	0.03*** (0.01)	0.04*** (0.01)	0.02* (0.01)	0.01 (0.01)	0.01 (0.01)	0.02* (0.01)	0.01 (0.01)
Le Pen (Rad. Right, ihs)	-0.29*** (0.05)	-0.26*** (0.05)	-0.20*** (0.04)	-0.22*** (0.04)	-0.22*** (0.04)	-0.18*** (0.04)	-0.18*** (0.03)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	29,989	29,989	29,989	29,989	29,989	29,989	29,989
Mean DepVar	4.11	4.11	4.11	4.11	4.11	4.11	4.11
Sd DepVar	0.68	0.68	0.68	0.68	0.68	0.68	0.68

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2012 election as share of the total electorate (omitting abstention). The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one.

Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate.

Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.22: Far-right Donation Gap: Tax-declared donations, 2017 Election and Sum of Donation 2017-2019, ihs

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	0.15*** (0.03)	0.15*** (0.03)	0.10*** (0.02)	0.09*** (0.02)	0.08*** (0.02)	0.07*** (0.02)	0.02 (0.03)
Hammon (Left, ihs)	0.17*** (0.02)	0.17*** (0.02)	0.13*** (0.01)	0.13*** (0.01)	0.13*** (0.01)	0.12*** (0.01)	0.09*** (0.01)
Macron (Centre, ihs)	0.63*** (0.03)	0.63*** (0.03)	0.29*** (0.03)	0.29*** (0.03)	0.28*** (0.03)	0.26*** (0.03)	0.12*** (0.03)
Fillon (Right, ihs)	0.48*** (0.03)	0.50*** (0.03)	0.29*** (0.03)	0.28*** (0.03)	0.29*** (0.03)	0.28*** (0.03)	0.19*** (0.03)
Dupont-Aignan (Rad. Right, ihs)	0.07*** (0.02)	0.08*** (0.02)	0.03* (0.01)	0.02 (0.01)	0.02* (0.01)	0.02* (0.01)	0.02 (0.01)
Le Pen (Rad. Right, ihs)	-0.24*** (0.04)	-0.21*** (0.04)	-0.18*** (0.03)	-0.21*** (0.03)	-0.21*** (0.03)	-0.19*** (0.03)	-0.19*** (0.03)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	27,561	27,561	27,561	27,561	27,561	27,561	27,561
Mean DepVar	4.19	4.19	4.19	4.19	4.19	4.19	4.19
Sd DepVar	0.68	0.68	0.68	0.68	0.68	0.68	0.68

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one.

Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate.

Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.23: Far-right Donation Gap: Tax-declared donations, yearly breakdown, ihs

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	-0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)	0.02* (0.01)	0.02 (0.01)	0.02 (0.01)
PS (Left, ihs)	0.05*** (0.02)	0.05*** (0.02)	0.05*** (0.02)	0.05*** (0.02)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)
LREM (Center, ihs)	0.11*** (0.01)	0.11*** (0.01)	0.11*** (0.01)	0.10*** (0.01)	0.12*** (0.01)	0.11*** (0.01)	0.12*** (0.01)
LR (Right, ihs)	0.09*** (0.01)	0.09*** (0.01)	0.08*** (0.01)	0.08*** (0.01)	0.12*** (0.01)	0.13*** (0.01)	0.12*** (0.01)
Dupont-Aignan (Rad. Right, ihs)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	0.02*** (0.00)	0.02*** (0.00)	0.03*** (0.01)
Le Pen (Rad. Right, ihs)	-0.15*** (0.01)	-0.14*** (0.01)	-0.14*** (0.01)	-0.14*** (0.01)	-0.16*** (0.01)	-0.17*** (0.01)	-0.16*** (0.02)
Election year	2012	2012	2012	2012	2017	2017	2017
Year of tax declarations	2013	2014	2015	2016	2017	2018	2019
Demographics	✓	✓	✓	✓	✓	✓	✓
Income	✓	✓	✓	✓	✓	✓	✓
Foreigners	✓	✓	✓	✓	✓	✓	✓
Employment	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓
Local Taxes	✓	✓	✓	✓	✓	✓	✓
Observations	31,134	31,041	31,258	30,047	29,757	29,248	28,399
Mean DepVar	3.55	3.54	3.55	3.50	3.47	3.42	3.35
Sd DepVar	0.35	0.35	0.35	0.36	0.36	0.37	0.38

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.24: Far-right Donation Gap: Tax-declared donations, 2017 Election and 2017-2019 Donation, Removing Cities with Far-right Representatives

	Share of donors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mélenchon (Rad. Left, ihs)	0.03 (0.02)	0.03 (0.02)	-0.00 (0.01)	-0.01 (0.01)	0.00 (0.01)	-0.00 (0.01)	-0.01 (0.01)
Hammon (Left, ihs)	0.06*** (0.01)	0.07*** (0.01)	0.05*** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.04*** (0.01)
Macron (Centre, ihs)	0.33*** (0.02)	0.34*** (0.02)	0.15*** (0.02)	0.14*** (0.02)	0.15*** (0.02)	0.14*** (0.02)	0.12*** (0.01)
Fillon (Right, ihs)	0.35*** (0.03)	0.33*** (0.02)	0.20*** (0.02)	0.18*** (0.02)	0.17*** (0.02)	0.17*** (0.02)	0.13*** (0.01)
Dupont-Aignan (Rad. Right, ihs)	0.07*** (0.01)	0.06*** (0.01)	0.02** (0.01)	0.01* (0.01)	0.01* (0.01)	0.01* (0.01)	0.03*** (0.01)
Le Pen (Rad. Right, ihs)	-0.16*** (0.02)	-0.18*** (0.02)	-0.19*** (0.02)	-0.23*** (0.02)	-0.22*** (0.02)	-0.20*** (0.02)	-0.16*** (0.02)
Demographics		✓	✓	✓	✓	✓	✓
Income			✓	✓	✓	✓	✓
Foreigners				✓	✓	✓	✓
Employment					✓	✓	✓
Education						✓	✓
Local Taxes							✓
Observations	27,394	27,394	27,394	27,394	27,394	27,394	27,394
Mean DepVar	3.05	3.05	3.05	3.05	3.05	3.05	3.05
Sd DepVar	0.36	0.36	0.36	0.36	0.36	0.36	0.36

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention), removing cities in the 8 electoral districts that elected a far-right representative in the 2017 parliamentary elections. The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.25: Far-right vote and Share of Donors (ihs), Panel with municipality Fixed Effects

	Share of donors	
	(1)	(2)
LFI (Rad. Left, ihs)	0.01* (0.00)	-0.02*** (0.01)
PS (Left, ihs)	0.02*** (0.00)	0.01** (0.00)
LREM (Center, ihs)	-0.00* (0.00)	-0.05*** (0.00)
LR (Right, ihs)	0.04*** (0.00)	0.08*** (0.01)
FN (Rad. Right, ihs)	-0.03*** (0.00)	-0.02 (0.01)
Commune Fixed effects	✓	✓
Full Controls		✓
Observations	188,197	188,197
Mean DepVar	3.12	3.12
Sd DepVar	0.37	0.37

*Notes:** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Yearly municipality panel from 2013-2019. Including municipality fixed effect and SE clustered at the municipality level. Each coefficient can be interpreted as an elasticity. The dependent variable is the IHS transformation of the share of donors. The plotted coefficients are the the IHS transformation of the party vote shares of candidates in the Presidential elections in the 2012 and 2017 as share of the total electorate (omitting abstention). The full set of controls includes demographics (population, share of women, under 24-year old), income (log median income, share of population in poverty, GINI), foreigners (share of foreign-born, foreigners unemployment rate), employment (unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture), education (share with master degree, share with bachelor's degree), taxes (log total tax revenue, number of fiscal households, total pensions).

Table 3.26: Far-right vote and Donations (ihs), Panel with municipality Fixed Effects

	Share of donors	
	(1)	(2)
LFI (Rad. Left, ihs)	0.01 (0.01)	-0.01 (0.01)
PS (Left, ihs)	0.01* (0.01)	0.00 (0.01)
LREM (Center, ihs)	0.02*** (0.01)	0.02** (0.01)
LR (Right, ihs)	0.03*** (0.01)	0.05** (0.02)
FN (Rad. Right, ihs)	-0.04*** (0.01)	-0.08*** (0.03)
Commune Fixed effects	✓	✓
Full Controls		✓
Observations	188,197	188,197
Mean DepVar	4.22	4.22
Sd DepVar	0.61	0.61

*Notes:** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Yearly municipality panel from 2013-2019. Including municipality fixed effect and SE clustered at the municipality level. Each coefficient can be interpreted as an elasticity. The dependent variable is the IHS transformation of the declared donations per tax household. The plotted coefficients are the the IHS transformation of the party vote shares of candidates in the Presidential elections in the 2012 and 2017 as share of the total electorate (omitting abstention). The full set of controls includes demographics (population, share of women, under 24-year old), income (log median income, share of population in poverty, GINI), foreigners (share of foreign-born, foreigners unemployment rate), employment (unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture), education (share with master degree, share with bachelor's degree), taxes (log total tax revenue, number of fiscal households, total pensions).

Table 3.27: Far-right ideology and the overall amount of donations (standardized coefficients)

	(1) Tax data	(2) ACF	(3) Oxfam	(4) SOSM
LFI (Rad. Left)	-0.017 (0.013)	0.038*** (0.010)	0.050*** (0.011)	0.071*** (0.016)
PS (Left)	0.109*** (0.018)	0.112*** (0.017)	0.101*** (0.019)	0.288*** (0.058)
LREM (Center)	0.087*** (0.012)	0.070*** (0.013)	0.042*** (0.011)	0.036** (0.016)
LR (Right)	0.126*** (0.015)	0.057*** (0.012)	0.006 (0.010)	0.025 (0.016)
DLF (Rad. Right)	-0.019** (0.009)	-0.016** (0.008)	-0.016** (0.008)	-0.023** (0.010)
FN (Rad. Right)	-0.098*** (0.015)	-0.111*** (0.013)	-0.045*** (0.011)	-0.051*** (0.015)
Department FEs	✓	✓	✓	✓
Election FEs	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	57,349	57,349	57,349	27,561
Clusters (Departments)	101	101	101	95
Mean DepVar	0.00	0.00	0.00	-0.00
Sd DepVar	1.00	1.00	1.00	1.00

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression of the standardised vote shares obtained by presidential candidates in the 2012 and 2017 election as share of the total electorate (omitting abstention). Column (1) regresses on the tax-deducted donations per fiscal household, column (2) on standardised donations to *Action contre la Faim*, column (3) for *Oxfam* and column (4) for *SOS Méditerranée*. All outcomes and regressors are standardised and winsorised at the 99th percentile to account large outliers in small municipalities in the Oxfam and SOSM data. Controls include department FE, **election FE**, local demographics, income, share of foreigners, employment, education and taxes.

Table 3.28: The propensity to donate and local supply of global charities, 2012

	(1)		(2)		(3)	
	% HHs donating		% HHs donating		% HHs donating	
Mélenchon	-0.020***	(0.007)	-0.020***	(0.007)	-0.019**	(0.007)
Hollande	0.046**	(0.019)	0.046**	(0.019)	0.044**	(0.019)
Bayrou	0.118***	(0.008)	0.118***	(0.008)	0.118***	(0.008)
Sarkozy	0.091***	(0.016)	0.092***	(0.016)	0.093***	(0.017)
Le Pen	-0.136***	(0.014)	-0.134***	(0.014)	-0.132***	(0.014)
Total # of charities			0.005***	(0.001)	0.004***	(0.001)
glob. charities			0.025***	(0.007)	0.200	(0.373)
glob. charities × Mélenchon					-0.027	(0.023)
glob. charities × Hollande					0.061	(0.041)
glob. charities × Bayrou					-0.021	(0.029)
glob. charities × Sarkozy					-0.036	(0.043)
glob. charities × Le Pen					-0.046	(0.028)
Controls	✓		✓		✓	
Observations	28,686		28,686		28,686	
Mean DepVar	3.18		3.18		3.18	
Sd DepVar	0.34		0.34		0.34	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level in parentheses). The independent variable is the (IHS transformation of) the share of households declaring charitable donations. The dependent variables are the (IHS transformation of) vote shares in 2012. An observation is a city/year. All the specifications include department fixed effects. Controls are city-level variables and include local demographics, income, share of foreigners, employment, education and taxes. The dependent variable is the (IHS transformation of) the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. "Globalist charities" are defined based on a set of keywords in the charities' statement of purpose (see the text for details).

Table 3.29: The propensity to donate and local supply of global charities, 2017

	(1)		(2)		(3)	
	% HHs donating		% HHs donating		% HHs donating	
Mélenchon	-0.012	(0.014)	-0.013	(0.014)	-0.013	(0.014)
Hamon	0.034***	(0.007)	0.034***	(0.007)	0.032***	(0.007)
Macron	0.112***	(0.015)	0.112***	(0.015)	0.113***	(0.015)
Fillon	0.129***	(0.014)	0.129***	(0.014)	0.129***	(0.015)
Le Pen	-0.165***	(0.016)	-0.163***	(0.016)	-0.160***	(0.015)
Total # of charities			0.003*	(0.001)	0.003*	(0.001)
glob. charities			0.022***	(0.006)	0.025	(0.311)
glob. charities × Mélenchon					0.017	(0.030)
glob. charities × Hamon					0.048***	(0.016)
glob. charities × Macron					-0.022	(0.031)
glob. charities × Fillon					0.008	(0.027)
glob. charities × Le Pen					-0.051*	(0.031)
Controls	✓		✓		✓	
Observations	27,263		27,263		27,263	
Mean DepVar	3.05		3.05		3.05	
Sd DepVar	0.36		0.36		0.36	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors clustered at department level in parentheses). The independent variable is the (IHS transformation of) the share of households declaring charitable donations. The dependent variables are the (IHS transformation of) vote shares in 2017. An observation is a city/year. All the specifications include department fixed effects. Controls are city-level variables and include local demographics, income, share of foreigners, employment, education and taxes. The dependent variable is the (IHS transformation of) the total amount of charitable donations reported in municipality c in year t , normalized by the number of households in the municipality. "Globalist charities" are defined based on a set of keywords in the charities' statement of purpose (see the text for details).

Table 3.30: Communes with and without data on political donations due to statistical secrecy

	No pol. data			Pol. data			Diff
	n	mean	sd	n	mean	sd	
% donating to politics	64047	-99.00	0.00	9311	0.99	0.50	99.987***
Political donations (€/hh)	64047	-99.00	0.00	9311	2.47	2.34	101.473***
Election (2012, 2017)	64047	2014.58	2.50	9311	2013.93	2.43	-0.601***
Charitable donations (€/hh)	48371	40.34	43.33	9311	48.10	33.45	7.725***
% donating to charity	48371	12.00	4.73	9311	12.14	4.51	0.138
LFI (Rad. Left)	62600	11.28	5.53	9224	11.46	4.46	0.183
PS (Left)	62600	12.93	10.34	9224	16.59	10.34	3.663***
LREM (Center)	62600	12.20	5.97	9224	11.88	6.50	-0.323
LR (Right)	62600	19.54	7.62	9224	19.39	6.85	-0.149
DLF (Rad. Right)	62600	3.29	2.39	9224	2.41	1.38	-0.879***
FN (Rad. Right)	62600	19.94	7.00	9224	16.15	5.43	-3.792***
Population > 14 year-old	62286	786.96	2092.62	9292	12255.10	54228.82	10,543.695**
Share of women	62283	0.50	0.03	9292	0.52	0.02	0.023***
Share above 25 years old	61931	0.11	0.03	9292	0.13	0.03	0.023***
median annual income	54854	20514.35	3243.27	9080	21812.78	4182.65	1,296.694**
Share below 60 % of median	2300	18.36	8.75	7609	19.69	9.47	1.972***
GINI	2525	0.29	0.05	7920	0.33	0.19	0.033***
Share that is foreigner	64043	0.04	0.04	9309	0.08	0.07	0.035***
Share foreigners unemployed	58788	0.08	0.13	9292	0.11	0.05	0.026***
Unemployment rate	61480	0.10	0.05	9292	0.12	0.05	0.022***
Share in agriculture	62206	0.07	0.11	9291	0.02	0.04	-0.053***
Share in public sector	62265	0.02	0.04	9290	0.05	0.06	0.035***
Share white-collar jobs	59222	0.06	0.10	9218	0.12	0.07	0.067***
Share blue-collar jobs	59222	0.24	0.21	9218	0.23	0.10	-0.011**
Share with a bachelor	61927	0.30	0.16	9219	0.35	0.13	0.059***
Share with a Master degree	61927	0.14	0.12	9219	0.19	0.11	0.051***
Reference tax income	61956	8143.96	9384.63	9241	120448.47	168162.86	111,993.360**
Total net tax	62226	392.08	557.36	9246	7751.82	13469.66	7,359.742**
Total pensions	61269	2656.36	3023.45	9243	37575.21	49226.64	34,918.853**
Global Charities p. 1,000	60636	0.07	0.58	9244	0.10	0.17	0.029***
Percentage Global, Stock	12670	0.16	0.34	7575	0.17	0.25	0.003

Notes:* p<0.10, ** p<0.05, *** p<0.01. Compares municipalities with and without data on charitable donations.

Table 3.31: Decomposition of 2017 Votes for 2022 Le Pen voters

	Converters Breakdown	
	Frequency	Observations
N. Arthaud	0.00	2
P. Poutou	0.00	8
JL Mélenchon	0.06	102
B. Hamon	0.01	47
E. Macron	0.05	144
J. Lassalle	0.01	17
F. Fillon	0.11	210
N. Dupont-Aignan	0.04	101
M. Le Pen	0.61	1328
J. Cheminade	0.00	2
F. Asselineau	0.00	9
Abst.	0.07	208
B&N	0.02	63
Total	1.00	2241

Notes: The table reports the reported votes in the 2017 presidential election of survey respondents who have reported to vote Le Pen in 2022. “Observations” reported the raw number of respondents who said they voted for this candidate in 2017 and “Frequency” refers to the (weighted) frequency among 2022 Le Pen voters. Respondents who voted Le Pen or Dupont-Aignant in 2017 are categorized as “faithful”, while the others are categorized as “converters”.

Table 3.32: Far-right Donation Gap By Median Le Pen Votes, Tax-declared Donations, 2012 Elections and 2013-2016 Donations

	Share of donors	
	(1) Below Median Le Pen Vote	(2) Above Median Le Pen Vote
Mélenchon (Rad. Left, ihs)	-0.02** (0.01)	-0.00 (0.01)
Hollande (Left, ihs)	0.05*** (0.02)	0.06*** (0.02)
Bayrou (Centre, ihs)	0.11*** (0.01)	0.12*** (0.01)
Sarkozy (Right, ihs)	0.06*** (0.01)	0.16*** (0.02)
Dupont-Aignan (Rad. Right, ihs)	-0.01 (0.01)	0.01 (0.01)
Le Pen (Rad. Right, ihs)	-0.15*** (0.02)	-0.09*** (0.02)
Demographics	✓	✓
Income	✓	✓
Foreigners	✓	✓
Employment	✓	✓
Education	✓	✓
Local Taxes	✓	✓
Observations	15,102	14,887
Mean DepVar	3.24	3.12
Sd DepVar	0.32	0.35

*Notes:** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression for half of the sample whose vote for the far-right candidate in the 2012 election was above/below the median. The independent variable is (IHS transformation of) votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the (IHS transformation of) share of households deducting a charitable donation in tax returns. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

Table 3.33: Far-right Donation Gap By Median Le Pen Votes, Tax-declared Donations, 2017 Elections and 2017-2019 Donation

	Share of donors	
	(1) Below Median Le Pen Vote	(2) Above Median Le Pen Vote
Mélenchon (Rad. Left, ihs)	-0.01 (0.01)	-0.01 (0.02)
Hammon (Left, ihs)	0.05*** (0.01)	0.02*** (0.01)
Macron (Centre, ihs)	0.14*** (0.02)	0.09*** (0.01)
Fillon (Right, ihs)	0.09*** (0.01)	0.17*** (0.01)
Dupont-Aignan (Rad. Right, ihs)	0.03*** (0.01)	0.03*** (0.01)
Le Pen (Rad. Right, ihs)	-0.16*** (0.02)	-0.19*** (0.03)
Demographics	✓	✓
Income	✓	✓
Foreigners	✓	✓
Employment	✓	✓
Education	✓	✓
Local Taxes	✓	✓
Observations	14,143	13,418
Mean DepVar	3.13	2.97
Sd DepVar	0.34	0.36

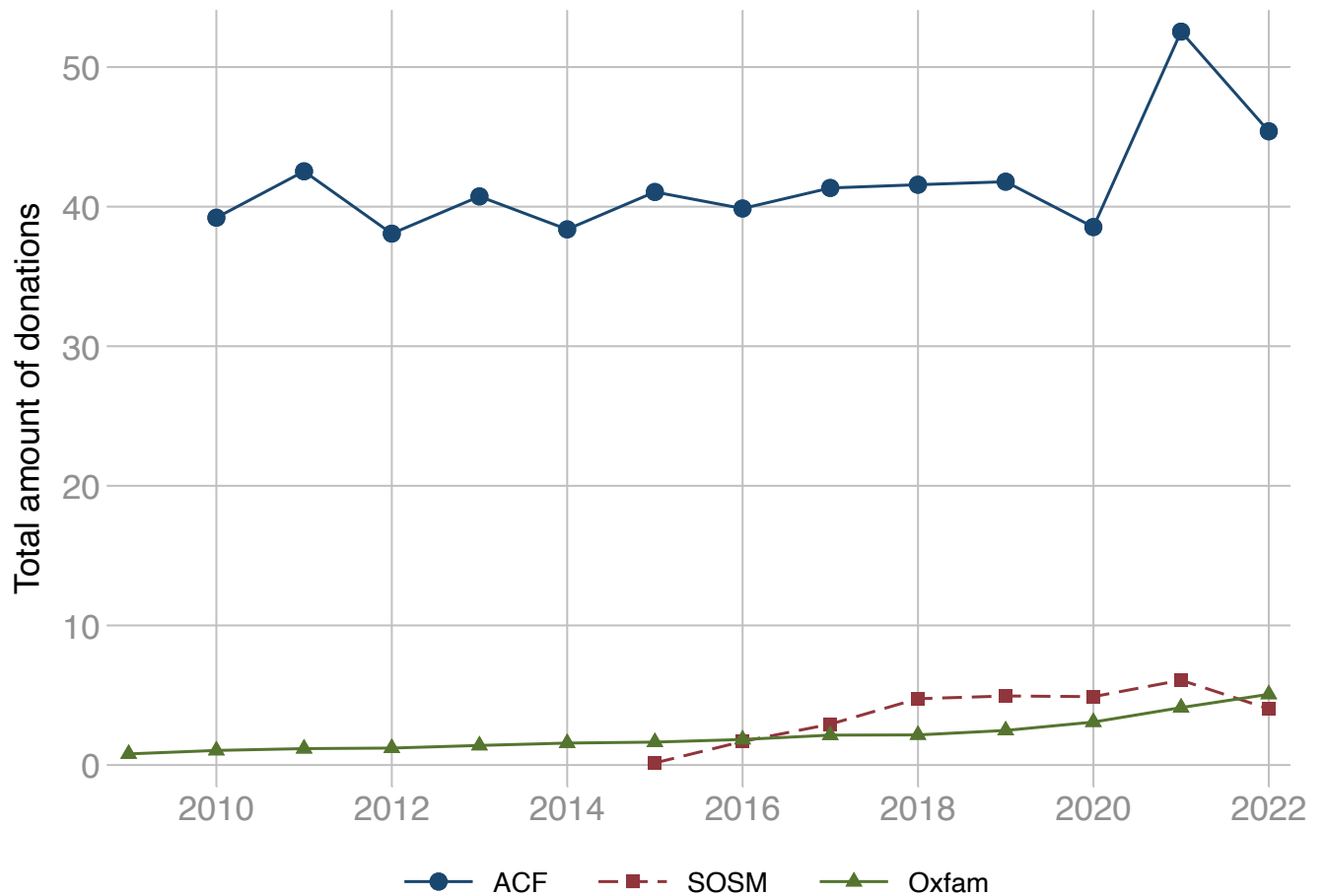
*Notes:** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at department level. Each column reports a multivariate regression for half of the sample whose vote for the far-right candidate in the 2017 election was above/below the median. The independent variable is (IHS transformation of) votes obtained by presidential candidates in the 2017 election as share of the total electorate (omitting abstention). The dependent variable is the (IHS transformation of) share of households deducting a charitable donation in tax returns. The dependent variable is the share of households deducting a charitable donation in tax returns standardized to have zero mean and standard deviation one. Demographics: Population, share of women, under 24-year old. Income: Log median income, share of population in poverty, GINI. Foreigners: Share of foreign-born, foreigners unemployment rate. Employment: Unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture. Education: Share with master degree, share with bachelor's degree. Taxes: Log total tax revenue, number of fiscal households, total pensions.

A.2 Additional Figures



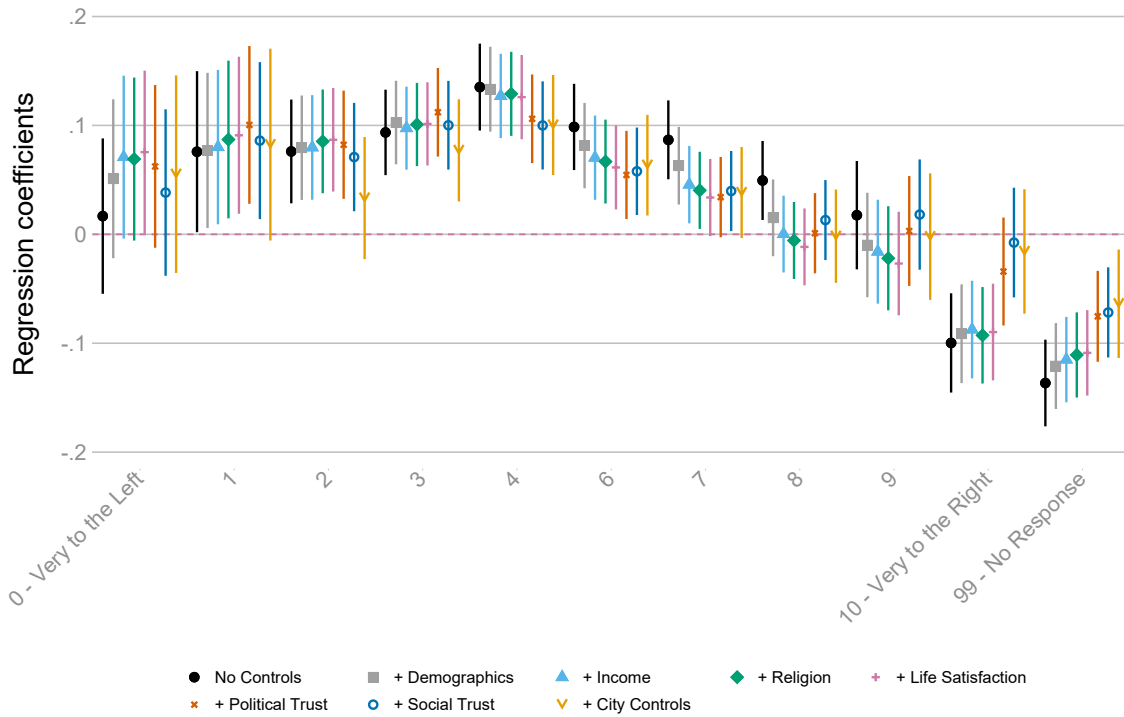
Notes: The Figure reports a screen shot of a map provided on the Action contre la faim’s website illustrating the interventions of the non-profit organization in the world.

Figure 3.7: “Action contre la faim”’s interventions in the world



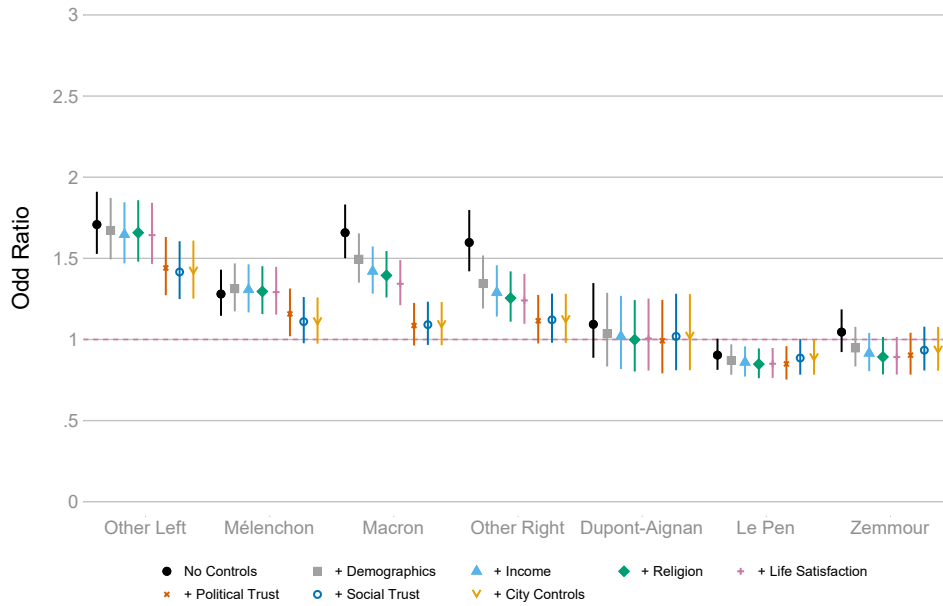
Notes: The Figure plots the annual amount of donations received from donors located in France for three non-profit organizations: “Action Contre la Faim” (ACF, blue lines with dots), “SOS Méditerranée” (SOSM, dashed red line with shares), and Oxfam (green line with triangles).

Figure 3.8: Action contre la faim, SOS Méditerranée and Oxfam: Annual amount of donations received



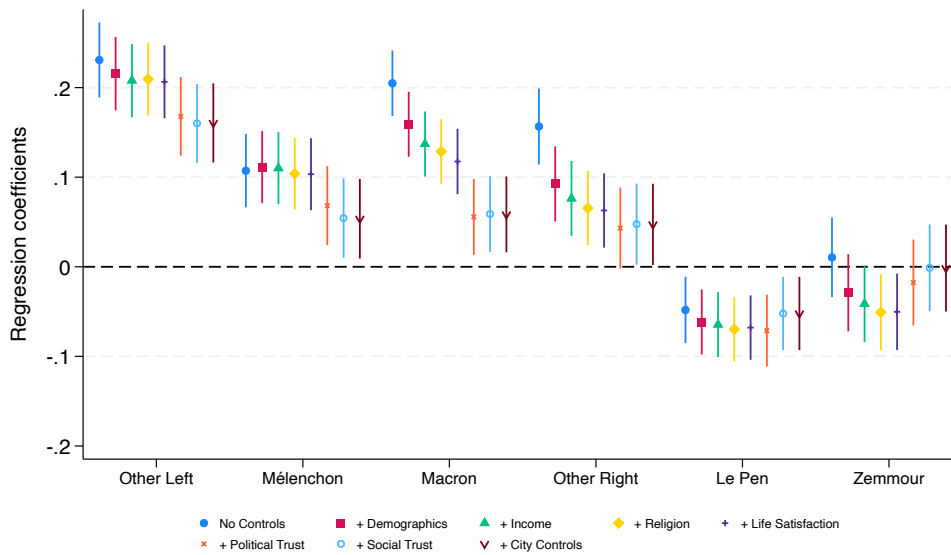
Notes: This figure visualizes the regression coefficients from table 3.12. The dependent variable is a dummy representing answering having made a donation to charitable organization in the last year in the survey and the right hand size is a continuous variable for self-reported political position on 0-10. The reference category is people placing themselves at 5. Demographics: Gender, age. Income: 14-scale income bracket. Religion: Categorical variable for religion. Life satisfaction: Overall satisfaction with life on a 0-10 scale. Trust in politics: four-point scale for trust in the President, MPs, mayors, the media and parties. Trust in society: four-point scale for trust in the family, acquaintances, strangers, people of other nationality and of other religion. City-level controls: Local socio-economic conditions of the city they live in, if available (see municipality level data).

Figure 3.9: Far-right Donation Gap: by Self-reported position on left-right scale



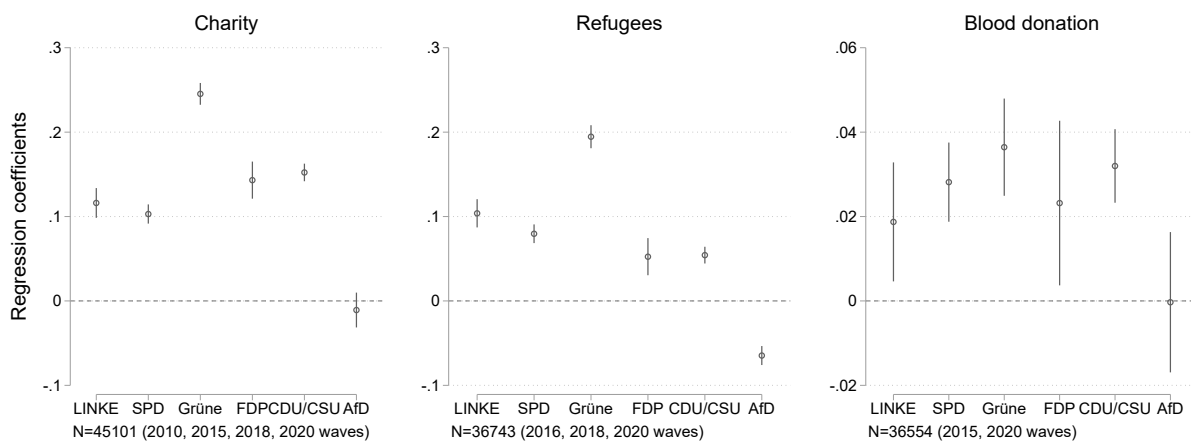
Notes: This figure visualizes the regression coefficients from table 3.14, which is the main regression in table 3.11 in probit specification. The coefficients are odd ratios.

Figure 3.10: Far-right Donation Gap: Robustness Checks, Probit Regression, Self-reported charitable donations: French Election Panel, 2022



Notes: This figure visualizes the regression coefficients from table 3.15, which is the main regression in table 3.11 but using the intention to donate next year as dependent variable.

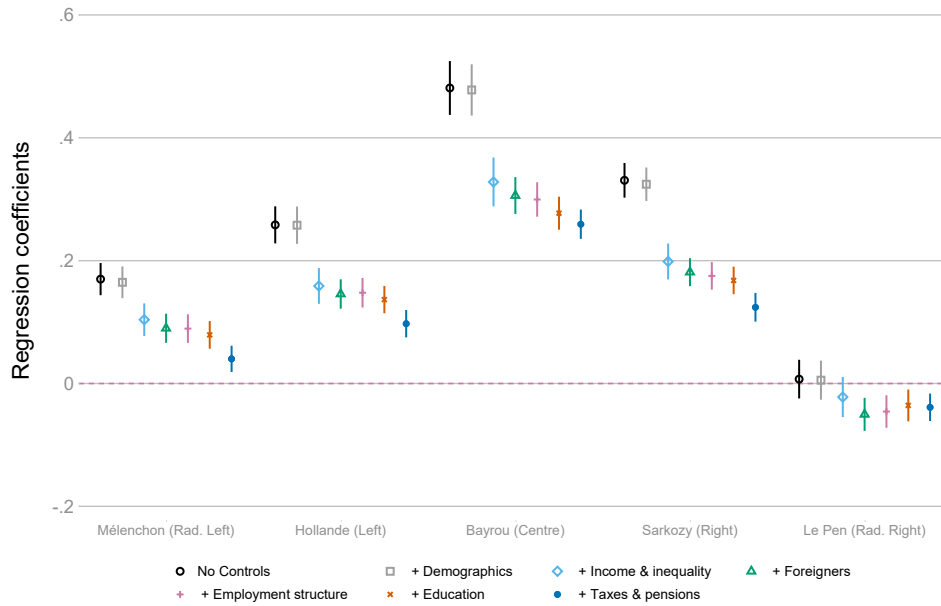
Figure 3.11: Far-right Donation Gap: Robustness Checks, Intention to Donate Next Year: French Election Panel, 2022



German Socioeconomic Panel. Baseline: Non-aligned. Parties from left to right, with AfD as only far-right party. Donations: dummy if donated past year (charities, refugees) or past five years (blood) SE clustered at the individual level and control for state & year FEs, gender, age, employment, income, marital status, religion, trust and subjective well-being.

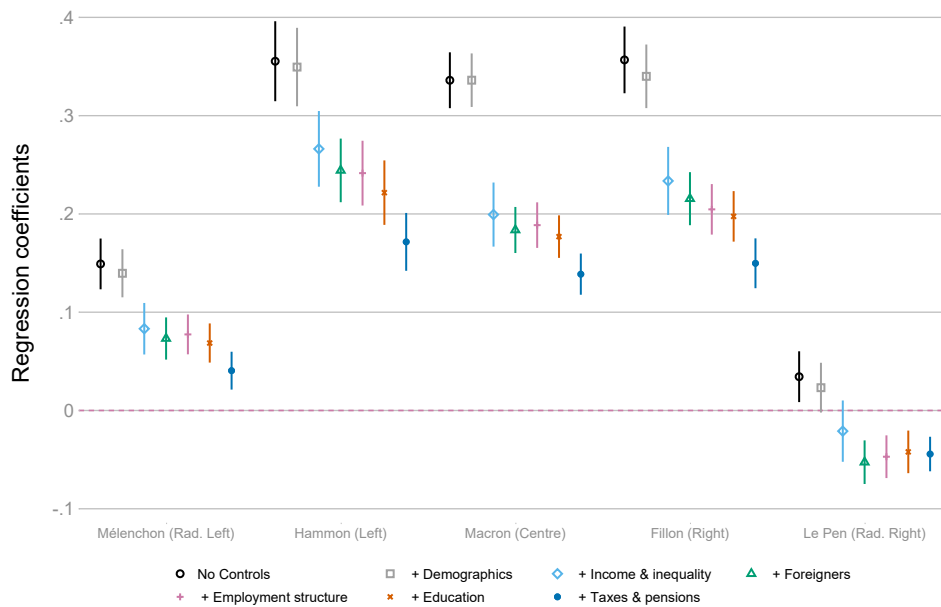
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. SE clustered at individual level (panel data). The dependent variable is a dummy for having reported a donation in the survey and the independent variable is the individual's reported party preference. The omitted category is abstention. Controls include year and state FE, demographics (gender, marital status), log of income, trust for society in general, religion and employment status.

Figure 3.12: External Validity: Results from the German Socio-Economic Panel (SOEP, 2010-2020)



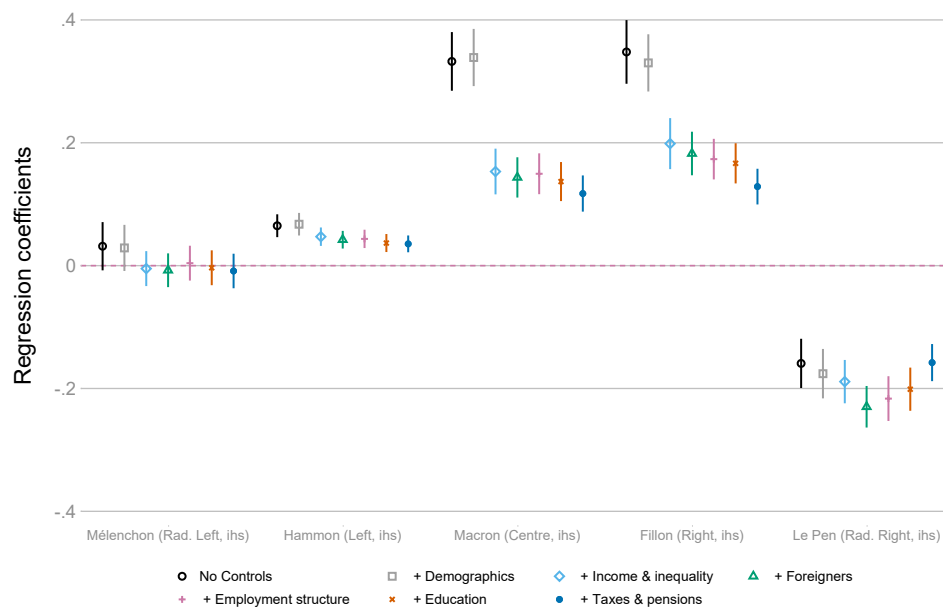
Notes: This figure visualizes the regression coefficients from table 3.19.

Figure 3.13: Far-right Donation Gap: Robustness Checks, Regression in levels, 2012 Votes and 2013-2016 Donations



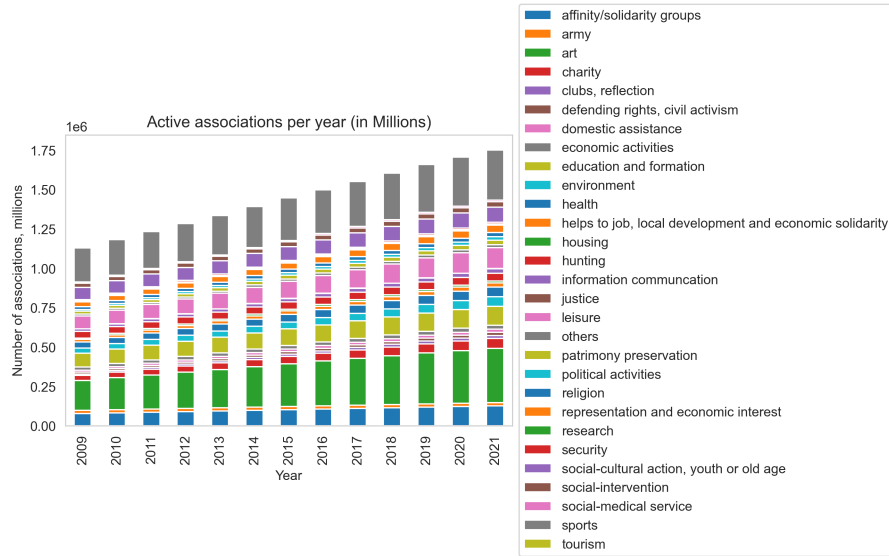
Notes: This figure visualizes the regression coefficients from table 3.20.

Figure 3.14: Far-right Donation Gap: Robustness Checks, Regression in levels, 2017 Votes and 2017-2019 Donations



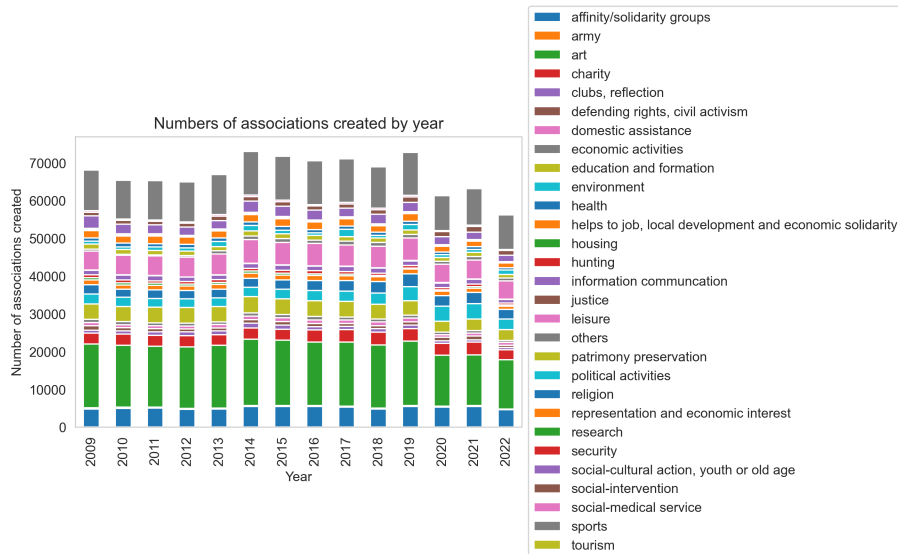
Notes: The Figure reports the regression results of table 3.24, which is the same multivariate regression in 3.18 without the cities that belong to the 8 electoral districts that elected a far-right representative in the 2017 parliamentary elections.

Figure 3.15: Far-right Donation Gap: Robustness Checks, Tax-declared Donations Data without Cities that Elected Far-right Representatives, 2017 Votes and 2017-2019 Donations



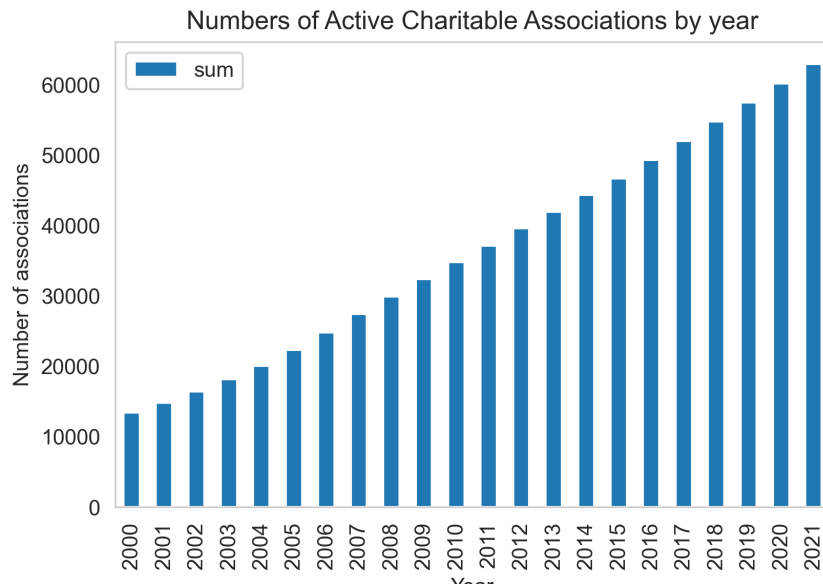
Notes: The Figure plots the number of associations in the National Directory of Associations that report a creation date before year x and are not dissolved in year x by the first two digit of a five-digit categorization code.

Figure 3.16: National Directory of Associations, Stock of Associations by Category



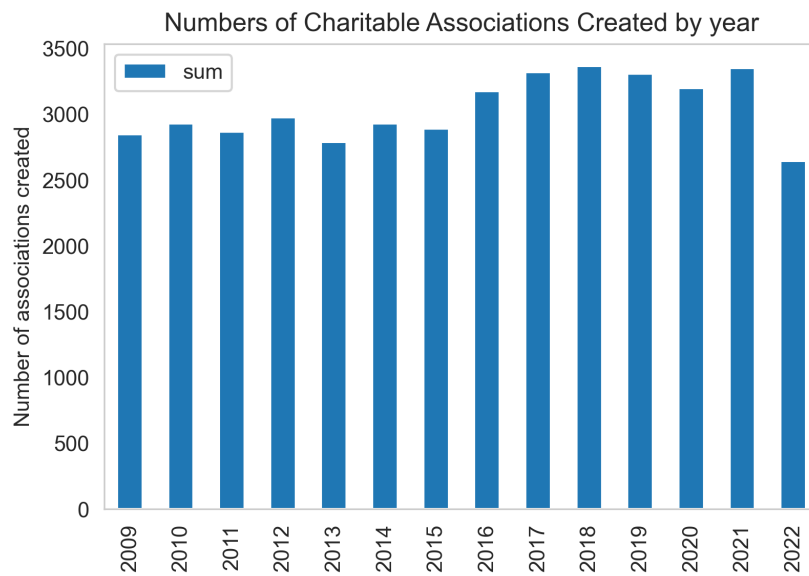
Notes: The Figure plots the number of new associations in the National Directory of Associations that reports a creation date in year x by the first two digit of a five-digit categorization code.

Figure 3.17: National Directory of Associations, Flow of New Associations by Category



Notes: The Figure plots the number of charitable associations in the National Directory of Associations that report a creation date before year x and are not dissolved in year x. A charitable association is defined as an association assigned a category code that begin with "20 - charitable associations, humanitarians, aid to development and volunteering".

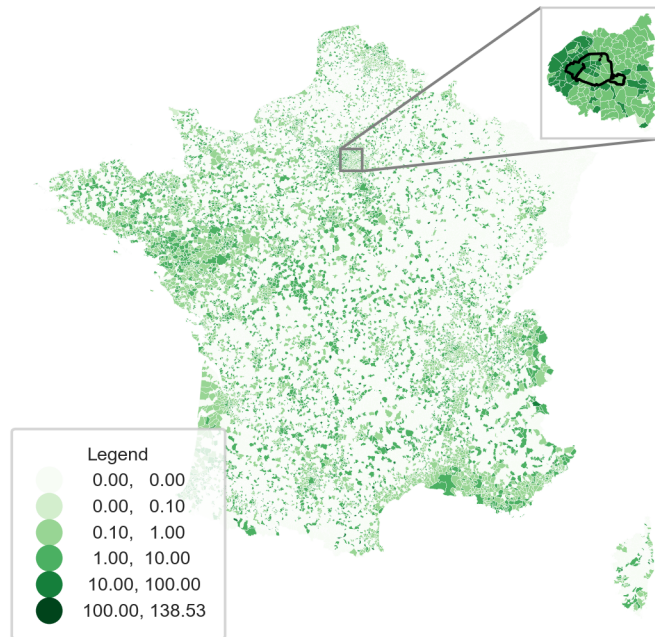
Figure 3.18: National Directory of Associations, Stock of Charitable Associations



Notes: The Figure plots the number of charitable associations in the National Directory of Associations that are created in each year between 2009 and 2022. A charitable association is defined as an association assigned a category code that begin with "20 - charitable associations, humanitarians, aid to development and volunteering".

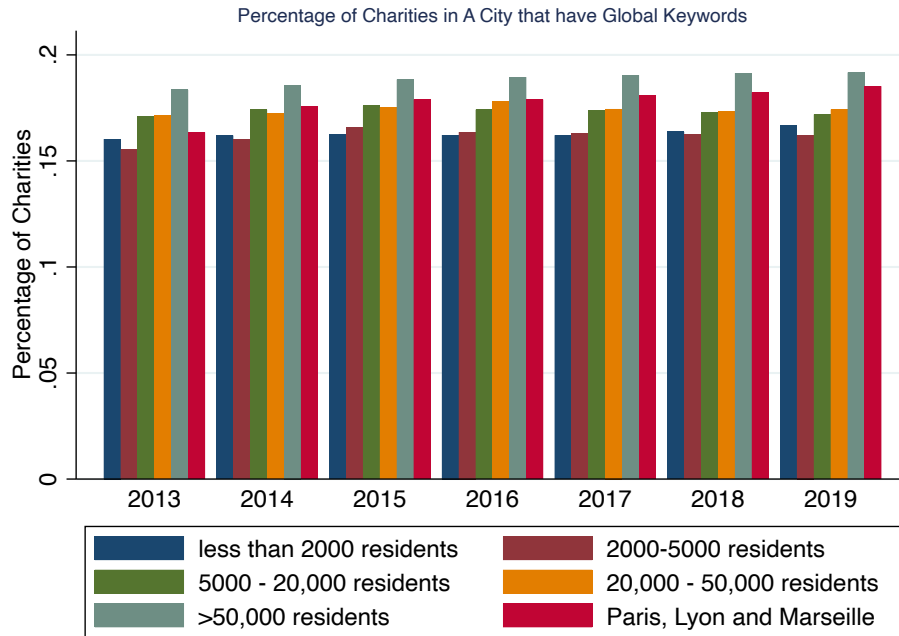
Figure 3.19: National Directory of Associations, Flow of New Charitable Associations

Numbers of charitable associations per 1,000 people 2019



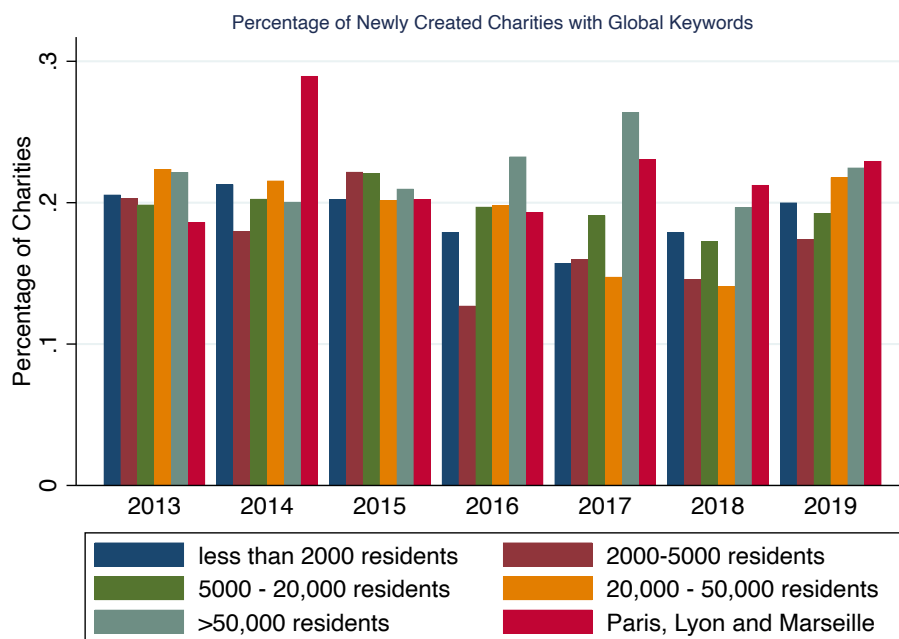
Notes: The Figure plots the number of charitable associations in the National Directory of Associations that report a creation date before 2019 and are not dissolved in 2019 by municipality. The population data come from the census ran by the National Bureau of Statistics and Economic Study (INSEE).

Figure 3.20: National Directory of Associations, Number of Charitable Associations in 2019



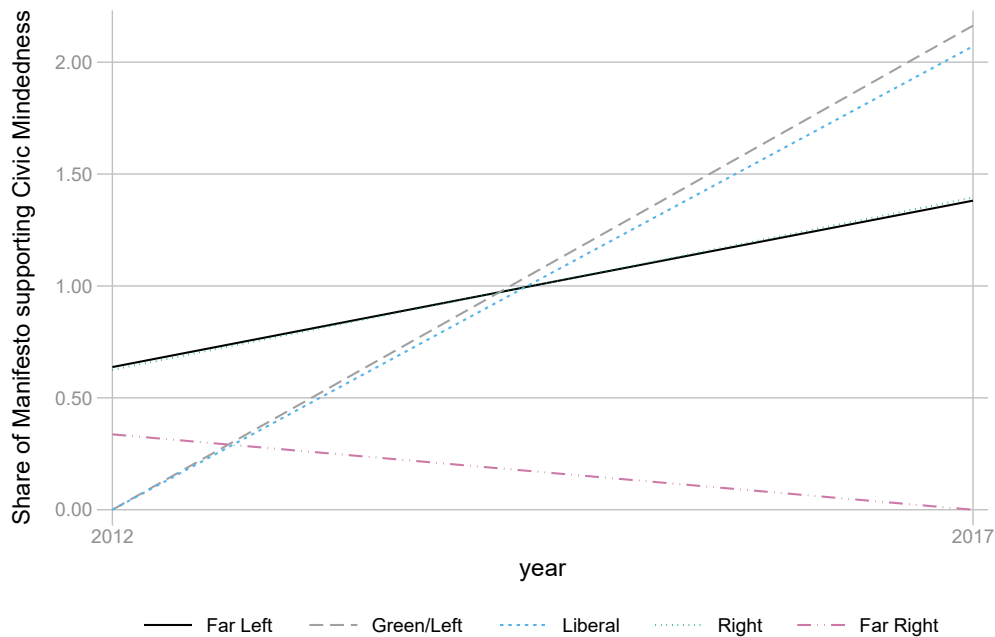
Notes: The Figure plots the average of percentage of charities that are characterised as “global” among all charties in a city by city size. For each level of cities, there are between 15% to 18% charties in a city that is “global” but the percentage of global charties is larger in larger cities.

Figure 3.21: Percentage of Charitable Associations that Contain Global Keywords, Stock



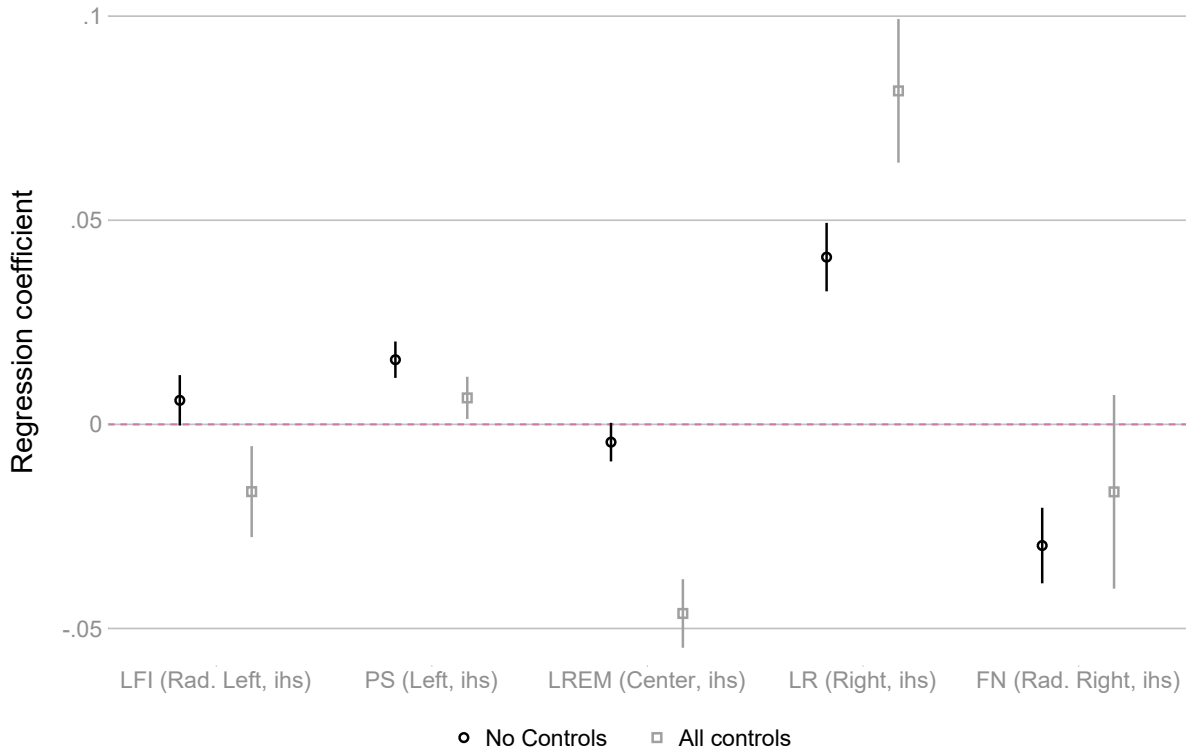
Notes: The Figure plots the average of percentage of charities that are characterised as “global” among new charties created each year in a city by city size.

Figure 3.22: Percentage of Charitable Associations that Contain Global Keywords, Flow



Notes: The Figure plots the importance of civic mindfulness in party manifestos by political family for the 2012 and 2017 presidential election from the Manifestos project [Lehmann et al. \(2023\)](#). It gives the share of sentences that express support for civic mindfulness as a fraction of the overall number of coded sentences per manifesto. A sentence is counted as supportive if it contains *"appeals for national solidarity and the need for society to see itself as united [or calls] for solidarity with and help for fellow people, familiar and unfamiliar. This may include favourable mention of the civil society, decrying anti-social attitudes in times of crisis, appeal for public spiritedness and support for the public interest."* For example, a value of 2% indicates that two percent of coded sentences in a manifesto express positive attitude towards civic mindedness. Parties are aggregated into political families by their vote share on the presidential election. The parties are classified in accordance with [Lehmann et al. \(2023\)](#): LFI, FDG, PCF, PRG as "Far Left"; EÉLV, Les Verts and PS as "Green/Left"; LREM, PR and UDI as "Liberal"; AC, MoDem, NC, LR and UMP as "Right"; and FN (Le Pen's party) as "Far right".

Figure 3.23: Party support Civic Mindedness, by political family



Notes: This figure visualizes the regression coefficients from table 3.25, which uses the yearly municipality panel from 2013-2019. The specification includes municipality fixed effect and SE are clustered at the municipality level. Each coefficient can be interpreted as an elasticity. The dependent variable is the IHS transformation of the share of donors. The plotted coefficients are the the IHS transformation of the party vote shares of candidates in the Presidential elections in the 2012 and 2017 as share of the total electorate (omitting abstention). The full set of controls includes demographics (population, share of women, under 24-year old), income (log median income, share of population in poverty, GINI), foreigners (share of foreign-born, foreigners unemployment rate), employment (unemployment rate, share of white and blue collar workers, share employed in public sector and agriculture), education (share with master degree, share with bachelor's degree), taxes (log total tax revenue, number of fiscal households, total pensions).

Figure 3.24: Far-right vote and Share of Donors (ihs), Panel with municipality Fixed Effects

A.3 Other additional information

RNA and Global keywords

Definition of a “charity” The RNA contains about 2 million observations of associations, including active, dissolved and inactive ones. In this paper, we analysis only a subset of non-profit organizations: those categorized as “charities” by the RNA.

We follow the WALDEC nomenclature of associations built in the RNA provided by the *Ministère de l’Intérieur et des Outre-Mer*. The codebook could be found on [Data.gouv.fr](https://data.gouv.fr).

The nomenclature is a 5-digit code that separate the charities into 27 small categories (on a 2-digit level) and more than 300 small categories. See below for a table that compares the original French description of the 2-digit categories and a short English translation used for the breakdown of associations as listed in 3.17.

In this project, we define “charity” as the associations that fits in the category “20 - *associations caritatives, humanitaires, aide au développement, développement du bénévolat* ”. We choose this board definition to follow the categorization by RNA itself; while it is true that some sub-categories of other categories have a charitable flavor (such as “02005-*associations philanthropiques*” , which is included in the boarder cateogry of “02-clubs, reflection”), we would like to refrain from subjectively cherry-picking smaller categories. There are roughly 73k “active” associations in this category as of end of 2022. ³²

Procedure to Generate the Global Key Words List We use the following procedures to generate the global key word list.

1. We take first all the active charitable associations in the RNA (about 73k,less than 5% of total active associations). Active here is defined as created before the end of 2022 without having been annulled.
2. Then we take all words in the statements of purpose, remove the stopwords, de-root them and list by frequency. The de-rooting and removing stopwords are based in the original French using the natural language processing package *nltk.corpus*.
3. We then take the top 5,000 words in frequency (covers all words that shows up more than 10 times in the description of all charities) and manually mark the words that satisfy the following characteristics

³²Active here is defined as non-dissolved. As we mentioned in the paper, the RNA data has a major caveat that it doesn’t accurately report the dissolutions: many associations do not report their inactivity or dissolution. So the stock of associations should be considered a proxy - a cumulated flow rather than an accurate estimation of active charities.

- Name of a foreign country or location and their adjective forms, such as "Mali", "Moroccan", "Indonesia", "Asia"
- Synonyms of "global", "European" and "international"
- Word that is unlikely to be linked with the situation in France or developed countries, such as "war" "famine" "refugees"
- Word that indicates a form of exchange, such as "cultures" or "peoples"

4. We save this list as the global dictionary.

See the next page for the complete list of global keywords by frequency and the distribution of frequency for top 30 words.

Table 3.34: 2-digit level categories in the RNA with approximate English translation

Code Objet Social	Objective - French	Short English name
1000	activités politiques	political activities
2000	clubs, cercles de réflexion	clubs, reflection
3000	défense de droits fondamentaux, activités civiques	defending rights, civil activism
4000	justice	justice
5000	information communication	information communication
6000	culture, pratiques d'activités artistiques, culturelles	art
7000	clubs de loisirs, relations	leisure
9000	action socio-culturelle	social-cultural action, youth or old age
10000	préservation du patrimoine	patrimony preservation
11000	Sports, activités de plein air	sports
13000	chasse pêche	hunting
14000	amicales, groupements affinitaires, groupements d'entraide (hors défense de droits fondamentaux)	affinity/solidarity groups
15000	éducation formation	education and formation
16000	recherche	research
17000	santé	health
18000	services et établissements médico-sociaux	social-medical service
19000	interventions sociales	social-intervention
20000	associations caritatives, humanitaires, aide au développement, développement du bénévolat	charity
21000	services familiaux, services aux personnes âgées	domestic assistance
22000	conduite d'activités économiques	economic activities
23000	représentation, promotion et défense d'intérêts économiques	representation and economic interest
24000	Environnement, cadre de vie	environment
30000	aide à l'emploi, développement local, promotion de solidarités économiques, vie locale	helps to job, local development and economic solidarity
32000	logement	housing
34000	Tourisme	tourism
36000	sécurité, protection civile	security
38000	armée (dont préparation militaire, médailles)	army
40000	activités religieuses, spirituelles ou philosophiques	religion
50000	domaines divers, domaines de nomenclature SITADELE à reclasser	others

Table 3.35: Global Key Word Dictionary: Full List By Frenquency

Afrique	sénégalais	vietnamien	qu'international	Sahara	Lomé	Tambacounda	Bénin;	Antilles
international	centrafricain	Afrique.	amazon	Maroc;	Tamil	dogon	Togolais	pointe-noir
mond	cameroun	Pays	haït	Madagascar.	européen	Dogon	Unesco	marocain;
marocain	congo	Brésil	Rwanda	arménien	Bolivie	international;	Haïtiens	kivu
Maroc	malien	indigent	Colombie	rwand	Martinique	Koudougou	Belgique	MONDE.
entraïd	haïtien	bénois	roumanie	Ouagadougou	Benin	Nadu	caribéen	Bali
divers	burkina	maritim	Ukrainiens	péruvien	bilatéral	sud-sud	nord/sud	indonésien
africain	congol	Kinshasa	Kenya	Congo-Brazzaville	gabon	moyen-orient	dominicain	Ouganda
Europe	tiers-mond	togolais	Internationale	Tibet	Pologne	bolivien	d'emmas	Balkans
étrang	univers	ile	islam	berber	Russie	soudan	nianing	franco-marocain
peupl	mondial	philippin	pays;	Antananarivo	Européens	marocain.	Europe.	Kolda
Sénégal	Vietnam	camerounais	indigen	afric	populations.	Subsaharienne	Anjouan	Occident
Sud	congolais	marrakech	franco	Mongolie	d'afriqu	bamako	Moldavie	Bulgarie
Madagascar	senegal	Africains	oriental	Orient	Guatemala	côte-ivoir	kinshas	molдав
Congo	palestinien	cambodgien	Thaïlande	Sénégalais	Faso.	gabonais	rajasthan	espagne
Cameroun	Mauritanie	emmaus	Africaine	d'afriqu	Mbour	libyen	burkinabe	congo.
Burkina	burkina-faso	bénin	étranger;	himalayen	Japon	japon	angolais	etranger.
République	Algérie	tunisien	Mexique	ukraine	Mali.	Grèce	international	boundou
ivoir	guinéen	guadeloup	Sénégal.	khmer	guinée-bissau	lao	juif	hémispher
alphabétis	éloign	Européenne	libanais	mongol	colomb	Somalie	Arabe	Cambodgiens
afrique	racism	Ile	afrique.	equateur	haitien	faso;	multicultural	Saint-Martin
Mali	refug	monde;	Burundi	tropical	franco-africain	bobo-dioulasso	Bosnie	Haïti.
étranger	burkinab	Sri	Angola	Djibouti	alger	Madagascar;	internationales;	touarègu
echang	musulman	Ghana	internationale.	brésilien	colombien	Congo;	ghanéen	Unis
faim	ivoirien	occidental	Indonésie	nouvelle-calédon	sahélien	katmandou	Congo.	romain
ong	comorien	Centrale	euros	Bangladesh	algérie	réunionnais	senegal.	Chiapas
continent	togo	asiat	mauritanien	Maurice	sahraou	Dominicaine	créol	Beyrouth
Togo	nord-sud	expédi	asie	Soudan	tchernobyl	mexicain	Saint-Louis	M'Bour
européen	océan	liban	Chine	palestin	latino-américain	dom-tom	tamil	Irak
ouest	monde.	argentin	népalais	saharien	peuples.	Togo;	algerien	Antalaha
interculturel	Tunisie	Argentine	sud-marocain	international.	emmaüs-international	centrafr	guadeloupéen	maroc.
malgach	brazzavill	anglais	chinois	telethon	laotien	judaïsme	martin	Albanie
Bénin	syrien	Laos	sri	mauritan	Nigéria	Camerounais	emmaüs-fr	antanarivo
Haïti	Pérou	afriqu	multiculturel	tchadien	Nord/Sud	Népalais	bresil	Togo.
francophon	subsaharien	Maroc.	benin	oultre-m	Caraiïbes	maghreb	Ladakh	emmaï
sénégal	RDC	Dakar	Indien	touareg	États	franco-malgach	égyptien	africain.
Népal	Centrafrique	unicef	etranger	laos	Chili	beninois	chilien	Serbie
global	étrang	burkin	sub-saharien	méditerranéen	vivre-ensembl	perou	Guyane	bilingu
Asie	expédit	l'étrang	Israël	alphabetis	Yaoundé	amérindien	Etats-Unis	ivoire
ukrainien	Liban	Andes	caraiï	américain	tribal	amazigh	Nicaragua	Penh
jumelag	Centrafricaine	cambodg	himalai	nigérien	madagascar.	espagnol	djiboutien	Salvador
indien	Maghreb	Ethiopie	afghanistan	Martin	burund	Cameroun.	tamatav	mauricien
Cambodge	Espagne	sénégazel	Haiti	éthiopien	internationale	d'emmaï	soudanais	maghrébin
madagascar	tibétain	vietnam	Lille	l'afriqu	tibétain	métissag	africains.	sénégal.
ethniq	Syrie	Bretagne	Bamako	pakistan	tchad	cultures.	Géorgie	Kosovo
Amériq	Tchad	Afrique;	Arménie	Donala	marocains.	cultures;	euro	
Ukraine	Gabon	algérien	étranger.	kurd	méditerranée	bafou	thaïland	
emmaï	Réunion	Unies	Cuba	Sénégal;	royaum	Europe;	senegalais	

Categorization Criteria and Example of Global and Local Charity We categorize charities as global if they have at least one global key word in the description.

This categorization of global charity should be considered as a lower bound: while the charities who have the key words are explicitly global, charities that do not contain a key word are not necessarily all focused on local issues.

- "Generalized" charities with very few words on their specific activities are categorized as local
- So are some charities who are global in nature but do not contain a key word because they focus on specific issues or places that are too rare to show up on the list, or because of spelling or grammar mistakes in the description.

For example, the charities with the following description are categorized as global:

- *"humanitarian action: distribution of school supplies in Morocco, Africa"*
- *"aid for the development of disadvantaged populations in Laos"*
- *"aid for the social, cultural and educational development of togo's children "*
- *"providing one-off and/or permanent material, physical and moral assistance to disadvantaged children living in Togo in the form of international solidarity initiatives: educational support, cultural, artistic and sporting activities. This includes support for children who are destitute, orphans and vulnerable to HIV, placed in foster care or from single-parent families, and who have sometimes dropped out of the school system; development of educational initiatives to promote citizenship, development and international solidarity among the general public at local, departmental and regional level (young people, adults, schoolchildren, students, etc.); support, advice and support for children who have lost their parents. Intercultural exchange, discovery of Togo and dissemination of Togolese culture (graphic arts, plastic arts, design, traditions, literature, music, gastronomy, tourism, etc.)."*

While the followings are not:

- *"feed, heal and sterilize the stray cats of saint-médard"*
- *"promote, organize and manage leisure and any form of reception, primarily aimed at young children, children and adolescents pedagogical concerning children's free time"*
- *"charity and general interest"*
- *"carry out a humanitarian foot race"*

Table 3.36: Global Key Word Dictionary: Frequency of top 30 words by frequency

Word	Frequency
Afrique	3869
international	3731
mond	3701
marocain	3355
Maroc	2910
entraïd	2448
divers	2187
africain	1986
Europe	1807
étrang	1605
peupl	1540
Sénégal	1513
Sud	1153
Madagascar	1114
Congo	844
Cameroun	757
Burkina	747
République	672
ivoir	616
alphabétis	613
afrique	596
Mali	581
étranger	551
echang	535
faim	529
ong	514
continent	494
Togo	489
européen	485
ouest	474

4

Fact-Checking and Misinformation: Evidence from the Market Leader

This chapter is based on a paper co-authored with Julia Cagé (Sciences Po), Emeric Henry (Sciences Po) and Nathan Gallo (Sciences Po).

Abstract

*What are the dynamic effects of fact-checking on the behavior of those who circulate misinformation and on the spread of false news? In this ongoing work, we address this question by building a unique partnership with the “Agence France Presse” (AFP), the largest fact-checking organization in the world. For 18 months, we collected the stories proposed by fact-checkers during the daily editorial meetings, some of which are ultimately fact-checked while others, despite being *ex ante* “similar”, are left aside. Using a Difference-in-Differences approach, we show that Facebook posts related to stories that are fact-checked receive 26 – 30% fewer shares compared to stories that were considered but not ultimately fact-checked. Moreover, we document that journalists, due to a time constraint, do not rate all posts associated to a story, despite them being mostly identical in content. We leverage this for a second with-story identification strategy on the post level, where we find important spillovers of ratings on unrated posts which are deleted pre-emptively by users. We draw on our unique data on the production of fact checks to formulate policy recommendations to improve the efficiency of fact-checking.*

JEL Classifications: D8, D83, D91, L82, L86

Keywords: fact-checking, misinformation, Facebook, fake news.

1 Introduction

Is fact-checking efficient at reducing the spread of misinformation? How does it affect the behavior of users or politicians who circulate false information? While the fact-checking

industry has been rising in recent years due to global concerns about fake news (Allcott and Gentzkow, 2017; Allcott et al., 2019), the impact of fact-checking is still under intense scrutiny. The literature provides strong evidence that, while fact-checking is unable to correct beliefs or voting intentions, it is effective in reducing circulation (Pennycook et al., 2020a,b; Henry et al., 2022). However, most of the papers in the literature use controlled lab in the field experiment, that cannot document dynamic effects on the behavior of participants.

In this ongoing paper, to address these questions, we rely on a unique partnership with the “*Agence France Presse*” (AFP), the third largest news agency in the world and the world’s largest fact-checking organization. A journalist was hired for 18 months to attend the daily editorial meetings of “*AFP Factuel*”, the AFP’s unit working on fact-checking the news in French language. She collected information on all the stories that were discussed during the daily meetings, those that were approved and fact-checked and those that were left aside. She also recorded the reasons for rejections (lack of resources, lack of virality, etc.) based on regular meetings with the *AFP Factuel*’s chief editors.

The *AFP* is a member of the “Third-party fact-checking program” set up by Facebook.¹ This gives journalists direct access to the Facebook tool where they can rate posts directly once a fact-check is produced. It also gives access to the so-called “Facebook claim”, which contains a list of suspicious posts automatically detected by Facebook using algorithms. Importantly, the agreement with Facebook does not provide incentives to systematically rate all posts that relate to the same fact-checked misinformation. For each of the stories, fact-checked or not, the journalist we hired also collected information on the associated posts rated and non-rated by the “*AFP Factuel*”’s journalists.

This unique data collection effort allows us to build an original identification strategy to identify the causal effect of fact-checking on the circulation of misinformation. We use two approaches, one at the story level (controlling for story and time fixed effects) and the other one at the post level (controlling for story-time and post fixed effects). These two distinct approaches address different questions. At the story level, we use a Difference-in-Differences (DiD) approach, comparing stories that were fact-checked to “similar” stories that were initially considered but left aside, in particular due to a lack of resources. Our preferred outcome variable is the (logarithm of the) sum of posts related to the stories on Facebook. The key identifying assumption is that the two types of stories would have had similar trajectories in terms of circulation absent the fact-checking intervention. To ensure the validity of this identifying assumption, we impose two restrictions on the data exploiting

¹There are 123 accredited organisations worldwide, for example Reuters and *The Washington Post* in the US, or *Le Monde* and *Libération* in France.

the details of the editorial process. First, we exclude the stories that were not fact-checked because of lack of virality.² Second, we exclude stories that were fact-checked even though the journalist proposing the story was already working on another fact-check at the time, since for those the cutoff to accept the story is higher.³ Importantly, consistent with our identification assumption, we show that both the fact-checked and the non fact-checked stories were facing similar popularity trend before being first considered by the AFP.

The second identification strategy uses only fact-checked stories and, for each story, compares the posts that were rated to those that were not. As explained above, the agreement with Facebook does not provide incentives to systematically rate all posts. Rating additional posts linked to the story is up to the willingness of the journalist who fact-checked the story. Working together with the *AFP Factuel* team allowed us to understand that journalists rate as many posts as they can but often not all of them due to a lack of time. Our identification assumption here – based on extensive discussions with AFP staff – is that the last post that is rated is “similar” to the first one that is not, i.e. that the fact-checker decides to stop rating posts at some point for random reasons. Consistently with this assumption we show that the posts that are and are not rated were following parallel trends in terms of number of shares on Facebook before the AFP Factuel team first considered the story.

Our preliminary results show that fact-checking reduces the circulation of the posts related to fact-checked stories. The story-level identification strategy shows that this reduction is significant – both from a statistical and from an economical point of view. On the story level, a fact-check reduces the circulation of associated posts by 26 – 30% relative to the control group. We find that this is driven by fact checks that are published fast for misinformation that was discovered quickly, but is only half as efficient if the process takes long. On the post level, we show that rating posts has important spillover effects on unrated posts, with accounts deleting their posts before they are rated to avoid downranking penalties by Facebook. Hence, our result reflect the combination of an enforcement effect by Facebook reducing circulation and a behavioral response by users.

Together with the descriptive evidence that we gather, we find several policy-relevant margins to improve the efficiency of fact-checking. First, we argue that, since speed matters, fact-checkers should be equipped with better tools to find misinformation, since speeding up the writing process itself may come with a trade-off with respect to the fact check quality.

²There are 5 main reasons for not fact-checking a story: (i) a lack of resources, (ii) a lack of virality of the story, (iii) the fact that the story is probably true, (iv) the fact that the fact-check would be infeasible, and (v) the fact that the story has already been fact-checked.

³Journalists indeed usually only work on one fact-check at a given moment of time. Note however that we show that our main results are robust even absent these two restrictions.

Second, we document that – in lack of properly working tools provided – the currently dominant way to find misinformation relies heavily on screening sub-communities on Facebook, leading to imperfect and path-dependent monitoring efforts. Third, we document that, as discussed, due to misaligned incentives, posts that share identical misinformation are not flagged. Improving on these three points appear to be low-hanging fruits, which would require little additional resources and significantly disburden the fact-checkers.

Literature review Our work relates to the growing literature on the impact of fact-checking using evidence from randomized survey experiments. The first strand of papers looks at the efficiency of fact-checking in correcting false beliefs. [Barrera et al. \(2017\)](#), in the context of presidential elections in France, expose users to false statements from the far-right candidate on the issue of immigration, while some are also randomly shown fact-checks of the statements. The paper shows that fact-checking works to correct purely factual beliefs, but does not change more subjective beliefs. In particular, the voting intentions for the far-right candidate increases by the same amount with and without fact-checking. Similar results are obtained by [Swire et al. \(2017\)](#) and [Nyhan et al. \(2020\)](#) in the context of Trump’s presidential campaign of 2016.⁴

Though inefficient in changing beliefs, there is a broad consensus, emerging from similar types of randomized survey experiments, that fact-checking can still play a role by decreasing the circulation of fact-checked content ([Henry et al., 2022](#); [Mena, 2020](#); [Pennycook et al., 2020a](#)). [Pennycook et al. \(2020a\)](#) for instance carried out an online experiment where the participants were shown true and false statements. They find that adding the “false” label to a statement significantly reduces participants’ self-reported intention to share the statement on social media.⁵

There is however a lack of evidence from the field on the impact of fact-checking, in particular dynamic effects, that our paper tries to fill. There is one exception: [Mattozzi et al. \(2022\)](#), in a closely related paper, study how fact-checking affects behavior of politicians. They partner with an Italian fact-checking company and randomly fact-check middle-range politicians. They find that after being fact-checked, politicians are less likely to make incorrect statements. They also show it makes them less likely to make statements that are verifiable.

⁴There is also a large related literature studying more generally the impact of information on political beliefs and behavior ([Alesina et al., 2018](#); [Kuziemko et al., 2015](#); [Grigorieff et al., 2016](#); [Cagé, 2020](#)).

⁵See also the literature on the factors influencing the decision to share ([Altay et al., 2020](#); [Guess et al., 2019](#)). [Chopra et al. \(2022\)](#), in an online experiment, study the demand for fact-checking. Fact-checking increases demand for a newsletter when it features stories from an ideologically non-aligned source.

The literature has also studied patterns of circulation of false news on social media. [Allcott and Gentzkow \(2017b\)](#) show that fake stories were intensely shared on Facebook during the 2016 U.S. presidential election campaign. [Vosoughi et al. \(2018\)](#) show that false news spread faster than real news.

The rest of the article is organized as follows. In [Section 2](#) below we describe the institutional setting and present the unique partnership we have set up with the AFP Factuel team. In [Section 3](#), we present our empirical strategy, both at the story and at the post level, and [Section 4](#) presents our main results. Finally, [Section 5](#) concludes.

2 Institutional setting and Data collection

2.1 Institutional Setting

Over the past decade, we have witnessed a steady growth of the fact-checking industry in a context of rising concern for the spread of misinformation. Fact-checking organizations range from small NGOs to mainstream media. More recently, the main social media companies have set up partnerships to verify the content circulating on their platforms. Meta (Facebook), in particular, partners with the International Fact-Checking Network (IFCN) to accredit fact-checking organisations as part of its “Third-party fact-checking program.” There are about 120 accredited organisations worldwide, for example Reuters and *The Associated Press* in the US, or *France 24* in France⁶.

We entered in a partnership (described in more details below) with one of the participants in the “Third-party fact-checking program”, AFP Factuel. AFP Factuel is the largest fact-checking organization in the world. It was created by the AFP in 2017 and gathers around 130 fact-checkers worldwide, with about 8-14 fact-checkers during our sample period (see [Appendix Figure 4.15](#)). Note that the AFP is a private nonprofit media organization, without shareholders and independent from the State. According to its bylaws, the agency is a “*autonomous organization with a civil status, operating under commercial rules.*” Its main goal is to seek “the elements of a complete and objective information.” Hence the agency – including the AFP Factuel team – is not politically involved and can be considered as non biased politically (as should be any global news agency). AFP Factuel published 8,614 fact-checks per year (in 2021), out of which 792 in French.⁷ We concentrate on the French team,

⁶As of July 26th, 2023. For an up-to-date list, consult <https://www.facebook.com/formedia/mjp/programs/third-party-fact-checking/partner-map>.

⁷As a global agency, the AFP indeed works in several languages, including German, English, Arabic, Spanish and Portuguese.

which fluctuates over the study period between 8-15 active members (see Appendix Figure 4.15). It is useful to describe the process of production of a fact-check at AFP Factuel, which should be representative of the procedure in most organizations.

The daily morning meetings Journalists, in a daily morning meeting,⁸ propose news stories to be verified and the AFP Factuel team decides whether to approve the different proposals, with the editor in chief being pivotal in the decision. If approved, the journalist who proposed the story typically writes the fact-check.

Finding the right fact-checkable story to propose in the morning meeting is a crucial and scarce input into the production of fact-checks. Importantly, journalists only suggest stories for a fact-check that they believe *ex ante* to be eligible for a fact check, which is important for our identification strategy detailed below. Journalists rely on a variety of sources to choose the fake news they propose to work on, and each journalist adopts her own search method, which partly dependent on her topic of specialization.⁹ The strategies usually involve certain keyword searches and monitoring of known offenders across different platforms.

For our analysis, we set aside fact checks on statements by politicians, as these cannot be rated on Facebook in accordance with the Third-party fact-checking programme. Political statements are made by politicians either on social media (most often Twitter) or on traditional media (TV or radio) that the journalists monitor (e.g. the morning shows interviews)¹⁰.

According to the data we collected, 40% of the stories considered and discussed during morning meetings originate from Twitter, despite the fact that the fact checks are produced for Facebook. 27% from other Facebook sources (see Table 4.1). In both cases, the journalists usually visit account and pages that are known to spread misinformation and screen new posts for new misinformation. Appendix Figure 4.16 further shows that in recent periods, WhatsApp and other social media have become more important sources of misinformation. In addition, about 14% of the proposed stories are stories that circulate in French but for which a fact checks has already been done by other AFP desks in other languages.

Only 15% of proposed stories come from the Facebook Claim, an algorithm provided by

⁸The meeting takes place every morning on weekdays, but not during the week-ends. The meeting is attended by all the journalists from the French fact-checking team, the news editors in charge of the fact-checking office in France, the editor of the Africa desk covering disinformation in the francophone area and the journalist covering disinformation in other European French-speaking countries (Belgium and Switzerland)

⁹In the empirical analysis below, we take into account journalists' characteristics by controlling for story fixed effects.

¹⁰Online Appendix Table 4.7 provides descriptive statistics for stories that concern political statements.

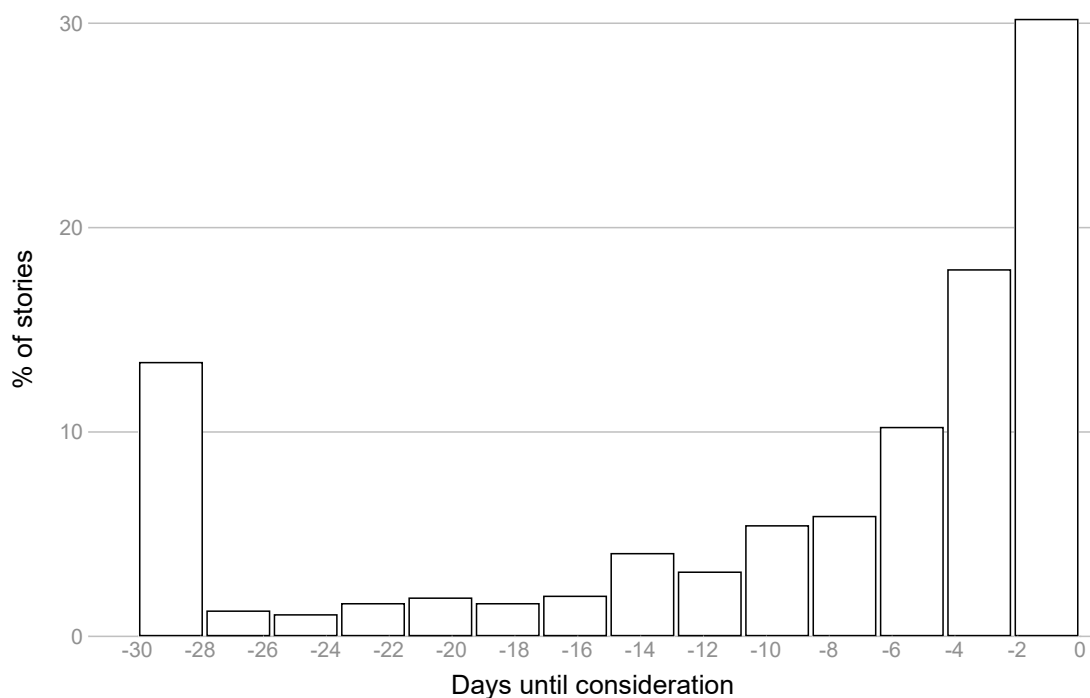
Table 4.1: Stories discussed during the morning meetings: Descriptive statistics

	Mean	SD	Min	Max	N
Decision on Fact-checking					
Fact-checked	0.61	0.49	0.00	1.00	878
Origin of story					
Origin: Twitter	0.40	0.49	0.00	1.00	687
Origin: Facebook claim	0.15	0.36	0.00	1.00	687
Origin: Facebook other	0.27	0.44	0.00	1.00	687
Origin: Whatsapp	0.06	0.25	0.00	1.00	687
Origin: Translation	0.14	0.34	0.00	1.00	687
Origin: Media	0.01	0.11	0.00	1.00	687
Origin: TikTok	0.02	0.13	0.00	1.00	687
Origin: Other social media	0.02	0.13	0.00	1.00	687
Topic of story					
Topic: Climate	0.04	0.20	0.00	1.00	878
Topic: Covid	0.18	0.39	0.00	1.00	878
Topic: Vacines	0.14	0.35	0.00	1.00	878
Topic: Elections	0.05	0.22	0.00	1.00	878
Topic: Ukraine	0.18	0.39	0.00	1.00	878
Topic: Inflation	0.06	0.23	0.00	1.00	878
Topic: Other francophone regions	0.03	0.17	0.00	1.00	878
Topic: Other health	0.03	0.16	0.00	1.00	878
Topic: Other	0.00	0.00	0.00	0.00	878

Notes: The table reports descriptive statistics on the stories discussed during the AFP Factual team’s morning meetings. An observation in the story. All the stories are included. The origin of stories was not collected for the first months of the study period. More details are provided in the text.

Facebook to the accredited organizations that selects suspicious news. This suggests that fact-checkers are insufficiently equipped with tools by Facebook to detect misinformation at large, instead relying on heuristics and network knowledge to fact-check misinformation from a small network of known offenders. Note that we started collecting the origin of stories only some months into the project, which is why it is missing for 22% of the sample.

As a result of these imperfect screening heuristics, it takes journalist some time to unearth potential misinformation varies substantially. Figure 4.1 shows that 50% of stories that are discussed have been posted more than for days before consideration by *AFP Factuel*, and 15% of stories have been circulating on Facebook more than 30 days before they are discussed.



Notes: The figure plots the distribution of the time interval (in days) between the first post of a story and its consideration by AFP in the morning meeting. Each bin is of size one day, and we winsorize the time interval at -30 days.

Figure 4.1: Distribution of the time interval (in days) between the first post of a story and its consideration by AFP in the morning meeting

For each fact checked story, we also retrieve all topic classification the AFP has attached to it from the published fact check. We also collect information on its exact date of publication, its length and potential updates that have been published on the fact check. Furthermore, for all stories that are not fact check, the AFP journalists provide us with the topic a potential fact check would have corresponded to choosing from the set of topic categories observed for fact checked stories (climate, Covid, elections, Ukraine, Inflation and others). As can be seen from table 4.1, topics of stories are centred around rather few issues and follow closely the overall news circle. For example, the war in Ukraine has become an important issue since February 24th 2022. Similarly (potential) fake news stories on the 2022 Presidential and Legislative Election appeared mostly shortly before and after the election dates. The evolution of topics over time is reported in Appendix Figure 4.17.

Among the news that are proposed by journalists, 60% are approved on average during the daily meetings. For the approved stories, the process of writing the fact-check then starts, involving discussion with experts and a careful study of different sources. The writing phase takes in a third of the cases less than a day and up to a few days. Figure 4.2 presents

the distribution of the time interval between the discussion of a story during the morning meetings and the date of publication of the fact-check. All fact-checks are published on the AFP Factuel’s website and available for free.

Rating the posts Once a fact-check is published, for non-political statements, the journalist who wrote it rates Facebook posts related to the story.¹¹ As part of the “Third-party fact-checking program”, journalists can do so directly on Facebook, choosing one of the ratings set up by Facebook: in order of severity “Missing Context”, “Partly False”, “Altered Photo/Video” and “False”. Regardless of the rating, the user who posted receives a notification and has the possibility to un-share the content. If rated “False”, an overlay is put on the post to blur its content which clearly indicates that it was checked by an independent fact-checker (Online Appendix Figure 4.8). The user then has the choice to view the fact check or to ignore and see the post. If the user would like to see the content nevertheless, she is prompted with an additional warning flag (Figure 4.12 in the Online Appendix). The post is also strongly demoted and repeated offenders may suffer penalties.¹²

If the content is rated “Partly False” (Figure 4.9 in the Online Appendix) or “Missing Context” (Figure 4.11 in the Online Appendix), the post remains visible, but with a banner at the bottom of the page. For the “Partly False” rating there is also a demotion, but much lighter than for the “False” rating.

In all cases, sharing flagged content requires the user to confirm additionally that she wants to share content flagged by fact-checks (Figure 4.13 in the Online Appendix)

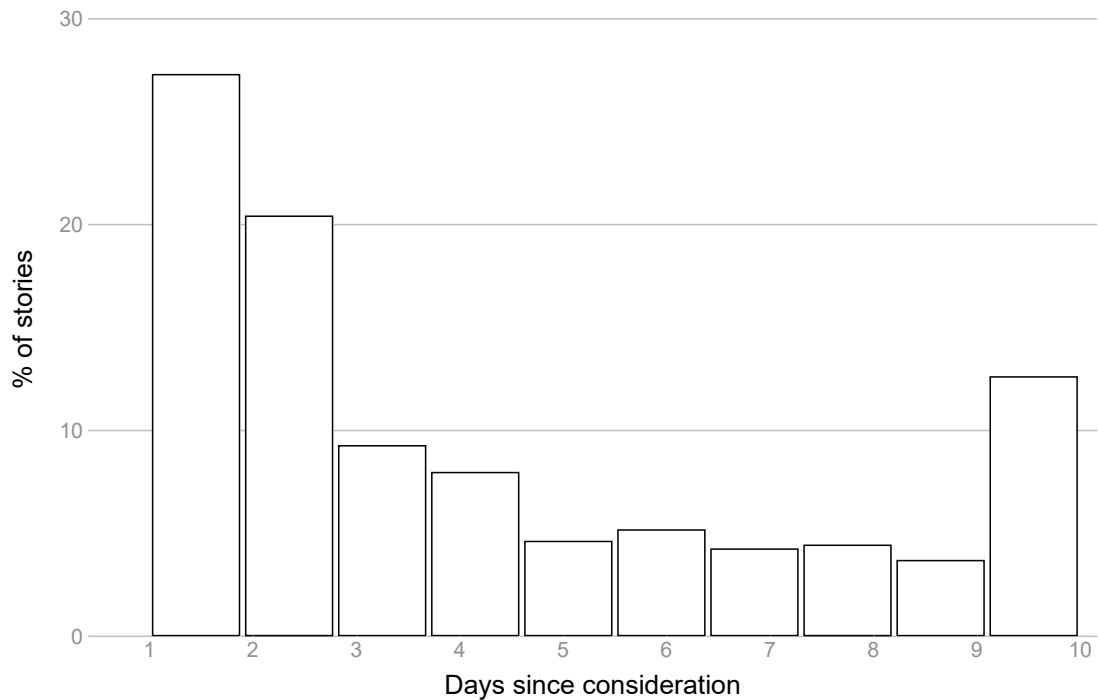
As part of the “Third-party fact-checking program”, AFP Factuel is paid a flat rate by Facebook for the the production of a fact check, not for rating the posts.¹³ If posts corresponding to the same fact check are rated, this will not involve additional payments. In fact, journalists also have the possibility to rate false information on other platforms and websites that might not be shared on Facebook (yet). It is therefore up to the journalist to decide how much effort to exert to rate additional posts on Facebook relaying a fake news story. To do so, the journalist searches on Facebook for posts that circulate the fact-checked news.¹⁴ Facebook also has an algorithm that detects posts related to the fact check and rates them automatically, but it makes up less than 2% of ratings observed in our data and seems limited to images that share similar fake news. As a result, not all posts that relate to the same

¹¹According to Facebook, posts related to political statements cannot be rated.

¹²For both of these dimensions, the exact modalities are not clearly communicated by Meta.

¹³The amount is not disclosed by Meta.

¹⁴If the source of the story is Facebook, then it is easy for the journalist to find associated posts; if it is not, she usually relies on links or keywords.



Notes: The figure plots the distribution of the time interval between the discussion of a story to be fact-checked and the publication of the fact-check. Each bin is of size one day, and we winsorize the time interval at 10 days.

Figure 4.2: Distribution of the time interval (in days) between the discussion of a story to be fact-checked and the publication of the fact-check

fact-checked story are flagged. For an illustration, compare Online Appendix Figures 4.9 and 4.10, which both were posted in the same group on different days with almost identical content, but only one was finally flagged. As a result, only 39% of posts that are associated with a fact-checked story are rated (see Table 4.2). We return to this point below.

2.2 Partnership with AFP and data collection

We entered in the partnership with AFP Factuel in October 2021. From December 1st, 2021, we recruited a journalist who attended the daily morning meeting during which his role would be to collect the information on all the news stories discussed for potential fact-check, the content of the discussion, the (anonymized) identity of the journalist, and the decision that was finally taken on whether to fact-check the story. Following the meeting, for each story that was proposed (accepted or not), the journalist proposing it sent us links and posts that the journalist proposing the story identified as promoting the claim. Independently, we collect from journalists the origin of the claim (i.e where it was found by the journalist: Twitter, Facebook queue, etc.).

Our researcher/journalist also had three additional data collection tasks. First, for the rejected stories, she needed to collect information on the reasons for rejection, a classification that we designed for the project in collaboration with the AFP team. The categories are (i) lack of resources, (ii) lack of virality, (iii) probably true, (iv) unfeasible, and (v) already done (see Table 4.2). She does so on a weekly basis with the editor in chief, who has the final say in the decision to accept or reject a story and has the most oversight over the team’s activities. Note that it is a policy at the AFP not to publish fact-checks on news stories that turn out to be true.¹⁵ We see that 44% of the considered stories are not fact-checked because they are considered as not fact-checkable by the AFP Factual team. For example, regarding the declaration of the European MP Karima Delli who claimed in November 2022 that Iran sentenced 15,000 protesters to death, the AFP Factual team considered that there were not enough official sources to investigate whether this claim was false. 17% of rejected stories are not fact-checked because of a lack of virality, and 25% because they are probably true. Finally, 18% are not chosen because of lack of resources. An example was the claim that excess mortality in France and in other countries would be linked to vaccination. This was considered too time consuming to establish formally, in particular because the claim referred to several countries.

Second, the journalist/researcher had to launch the process of collection of public Facebook posts using Crowdtangle, a tool provided by Meta that collects time-series data on a range of public posts.¹⁶ For each story, the search was based on the exact keywords used in the claim. After June 2023, for all posts, this exact same search was done in 3 phases: phase 1, immediately after the meeting and phase 2, a week after the meeting. In addition, for the fact-checked posts, the data collection was also performed at the time of publication of the fact-check.

Third, she had to collect the rated posts. After the meeting, a journalist who has access to the Meta rating tool exports all the rating data. This includes the category of the rating, the rating date, the url of the rated content and the url of the fact-check.

Additional data collection We collected detailed information on the accounts publishing posts related to the stories in our data. As shown in Table 4.3, there is a large variety in the type of accounts involved. 70% are groups rather than individual pages. They have 76K followers on average, going up to 65M. What is striking is the strong skew in the data: While the median account is active on only one story, there are several “super spreader accounts”

¹⁵This approach is not shared by all fact-checking organisations.

¹⁶Crowdtangle only collects data for public posts from public pages, groups and verified profiles above a certain threshold. It does not collect information on private accounts.

Table 4.2: Story circulation and fact-checking production

	Median	Mean	SD	Min	Max	N
Story posts at consideration						
Δ considered - posting (days)	4.41	16.50	35.37	-0.10	152.79	878
# Posts	8.00	27.96	91.96	1.00	1,801.00	878
# Contents	7.00	9.50	7.71	1.00	60.00	879
# Shares	89.14	1868.74	9519.93	0.00	189,001.00	879
# Likes	75.00	2204.60	8806.61	0.00	104,309.93	879
# Comments	32.17	561.32	2130.06	0.00	40,036.67	879
Trend in shares	1.33	242.35	3158.94	-2.0e+04	71,862.28	879
Unchecked stories						
Reason: infeasible	0.00	0.44	0.50	0.00	1.00	340
Reason: probably true	0.00	0.25	0.43	0.00	1.00	340
Reason: lack resources	0.00	0.18	0.38	0.00	1.00	340
Reason: lack virality	0.00	0.17	0.38	0.00	1.00	340
Reason: already done	0.00	0.10	0.30	0.00	1.00	340
Checked stories						
Δ checked - considered (days)	2.19	4.02	4.66	0.15	17.14	538
Length FC (1000 words)	8.33	9.20	4.04	2.79	25.53	538
Share rated posts	0.39	0.43	0.36	0.00	1.00	538
Share blurred posts	0.13	0.45	0.47	0.00	1.00	538
Share posts w. infoflag	0.00	0.30	0.44	0.00	1.00	538

Notes: The table reports descriptive statistics on the reasons on the circulation of all posts related to stories, reasons for not checking the unchecked stories and details on the checked stories. More details are provided in the text.

Table 4.3: Accounts publishing posts: Descriptive statistics

	Median	Mean	SD	Min	Max	N
Account types						
Account is Facebook Group	1.00	0.70	0.46	0	1	9769
Account is Facebook Page	0.00	0.30	0.46	0	1	9769
Account country if specified						
Registered in France	0.00	0.44	0.50	0	1	2696
Registered in Africa	0.00	0.24	0.43	0	1	2696
Registered in Belgium	0.00	0.04	0.19	0	1	2696
Registered in US	0.00	0.02	0.15	0	1	2696
Registered in Canada	0.00	0.04	0.20	0	1	2696
Account activity						
Maximum number of subscribers	13005.00	76031.11	7.2e+05	0	65,296,165	9769
Number on Stories in sample	1.00	2.28	4.97	1	167	9769
Number on Post in sample	1.00	2.81	9.59	1	430	9769

Notes: The table reports descriptive statistics on the accounts who publish at least one post in our data. An observation is an account. More details are provided in the text and in Appendix table 4.11.

that participate in more than one of the stories fact checked by AFP, often with several posts. One group is in fact involved in 167 stories, and the maximum number of posts across all stories reaches 430. We show in Appendix Table 4.11 the names and characteristics of the groups that are most active. Most of them are groups opposing Emmanuel Macron, and in particular groups opposed to mandatory vaccination and to sanitary measures against Covid.

Time period covered in the paper As highlighted above, the partnership with AFP was decided in October 2021, and our researcher/journalist began to work on a daily basis with the AFP Factual team beginning on December 1st, 2021. However, and for obvious reasons, it took us time to perfectly understand the way the AFP Factual team worked, and the different data collection tasks that needed to be performed to estimate the causal effect of the fact-checks. Hence, the first months during which our researcher/journalist worked at the AFP can be considered as a “trial period”.

For this reason, some variables (e.g. the origin of the story) are available for a lower number of observations than others (e.g. the topic of the story) (see e.g. Table 4.1). The final time period for the study reaches from January 2022 until June 2023 and covers 878 proposed stories.

3 Identification and empirical strategy

The objective of our identification strategy is to identify “counter-factuals” for the content that is fact-checked by the fact-checking organization, in order to causally estimate the impact of the fact-check on the spread of the story, in a difference-in-differences framework. To do so, we use two different empirical strategies. The first, at the story level, compares stories that are fact-checked to those that are considered but left aside. The second compares rated posts with unrated posts within stories that are checked.

Story-level empirical strategy Our first empirical strategy is at the level of the stories. Thanks to our unique partnership with AFP Factual, we identify a group of stories that were considered by the AFP for fact-checking but were ultimately set aside, and that we use as our “control group”.

We estimate the following model:

$$\bar{Y}_{st} = \alpha + \beta Fact_s \times \mathbb{1}_{t > t_{sc}} + \delta_s + \gamma_t + \varepsilon_{st} \quad (4.1)$$

where s indexes the stories and t the time. The dependent variable, \bar{Y}_{st} , is the sum of shares of the posts p related to story s (that we denote $p \in I(s)$) at time t (i.e. $\bar{Y}_{st} = \sum_{p \in I(s)} Y_{pst}$). The higher \bar{Y}_{st} , the wider the spread of the story. We control for story (δ_s) and time fixed effects (γ_t).¹⁷

Our main explanatory variable, $Fact_s \times \mathbb{1}_{t > t_{sc}}$, is the interaction between an indicator variable equal to one if the story has been fact-checked and to zero otherwise ($Fact_s$), and an indicator variable equal to one after the story has been first considered by the AFP factual team ($\mathbb{1}_{t > t_{sc}}$).¹⁸ The coefficient β measures the causal impact of the fact-check on the spread of the fact-checked stories compared to what would have happened absent the fact check. If fact-checking is efficient at reducing the spread of misinformation, then β should be negative. We cluster the standard errors at the level of the stories.

The key identification assumption is that the treated and control stories would have followed similar trends absent fact-checking. Note that, when journalists propose to check a story

¹⁷Note that, given we control for δ_s , we cannot introduce in equation (4.1) other time-invariant story level characteristics such as the source of the story (Twitter, Facebook, WhatsApp, etc.) or its topic.

¹⁸ t_{sc} is the time at which the story has been first considered by the AFP factual team. We use the time of consideration as our benchmark time to define the pre/post treatment period – rather, for example, than the time of the fact-check – given this time is accurately defined both for the stories that are fact-checked and those that are not.

in the morning meeting, she believes and argues the case that it should give rise to a fact check, which supports the idea that all proposed stories are ex ante comparable. To increase the likelihood that this assumption is met, we impose restrictions on both the control and treatment groups. First we remove from the sample stories that were not fact checked because of lack of virality, since the selection was precisely done according to the future trends. Second, we remove stories that were fact-checked, even though the journalist proposing the story was already working on a different fact-check. The intuition is that, for such stories, the bar is higher to have them accepted and the difference with the control group is more likely to be sizeable.

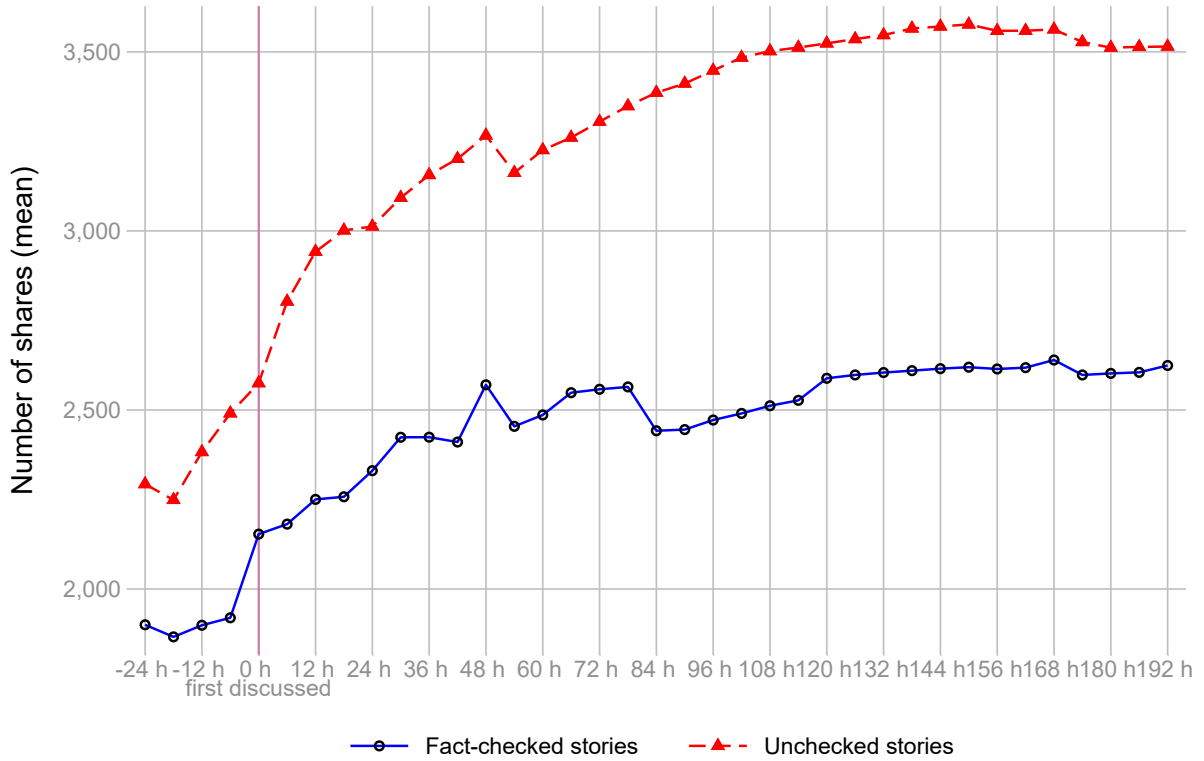
Figure 4.3 plots the raw trends in the average number of shares of posts associated with checked vs. unchecked stories. If anything, unchecked stories are somewhat more viral than checked stories, although they are overall close and parallel to each other and only diverge after consideration, when the checked stories are flagged subsequently. Moreover, checked and unchecked stories follow very similar trends in terms of comments, likes and the total number of active posts, as reported in the parallel trend Figures in the Appendix (see Figures 4.21, 4.20 and 4.22). This is confirmed by the balance Table in the Appendix (4.9) that shows that checked stories are very similar to unchecked stories at the time of consideration, both in terms of shares, likes and comments as well as the origins and topics of fact-checks, with differences either not being statistically or economically significant.

We also perform an event-study analysis, to provide evidence consistent with the parallel trend assumption. Using six hour intervals to measure circulation, we estimate the following model:

$$\bar{Y}_{st} = \sum_{d=-24}^{192} \beta_d \text{Fact}_s \times \mathbb{1}_{t-t_{sc}=d} + \delta_s + \gamma_t + \varepsilon_{st} \quad (4.2)$$

where, as before, t_{sc} is the time at which the story is first considered by the AFP Factual team. We are interested in the β_t coefficients; the parallel trends assumption requires that the coefficients β_{-24} to β_{-6} to be close to zero and not statistically significant. We omit β_0 as the reference category, as we expect the treatment to start only after $d = 0$, e.g. after the fact-checkers discussed the story.

Post-level empirical strategy Our second empirical strategy uses quasi-experimental variations at the level of the posts. As described above, once a story is fact-checked, the journalist in charge of fact-checking this story rates several posts associated to the story. We



Notes: Each line plots the average sum of all shares of posts associated with a story for checked vs unchecked stories relative to the date of consideration of a story by the fact-checkers.

Figure 4.3: Raw trends in sum of story shares

use the fact that, due to a time constraint, and to the fact the agreement with Facebook does not provide incentives to rate all posts, journalists tend to rate some but not all the posts associated to the story. We study the evolution of the popularity of the rated posts (treated posts) compared to the non-rated ones (control posts). Compared to the story-level specification, only the stories that are fact-checked by the AFP Factual team are included in the sample. Appendix Figures 4.18 shows the distribution of stories over the share of rated posts associated with a story. We exclude all stories in which all or no post was rated. This leaves us with a sample of 349 stories for which some but not all posts are rated (14,754 posts in total). Appendix Figures 4.19 shows that stories are evenly distributed over the share of rated posts.

We proceed to estimate the following difference-in-difference model:

$$Y_{p(s)t} = \zeta Rate_{p(s)} \times \mathbb{1}_{t > t_{sr}} + \mu_{p(s)} + \lambda_{s,t} + \varepsilon_{pst} \quad (4.3)$$

where, as before, p index the posts, s the stories and t the time. The dependent variable of interest, $Y_{p(s)t}$, is a variable measuring engagement with a post p on story s at time t . (A post can only be related to a single story here.) The higher this number, the higher the spread of the post on Facebook.

Our explanatory variable of interest, $Rate_{p(s)} \times \mathbb{1}_{t > t_{sr}}$, is the interaction between an indicator variable equal to 1 if the post p on story s has been rated and to zero otherwise ($Rate_{p(s)}$) and t_{sr} is the time the first rating is done on story r . Contrary to the story level identification strategy, time is taken relative to the date of the first rating within a story rather than the time of consideration. Since we expect the first rating to have an effect as well, we omit the last pre-rating timestep in the the event study estimate. We control for post fixed effects $\mu_{p(s)}$, as well as for a story-time fixed effect $\lambda_{s,t}$. Standard errors are clustered at the story level. The corresponding event study specification is thus:

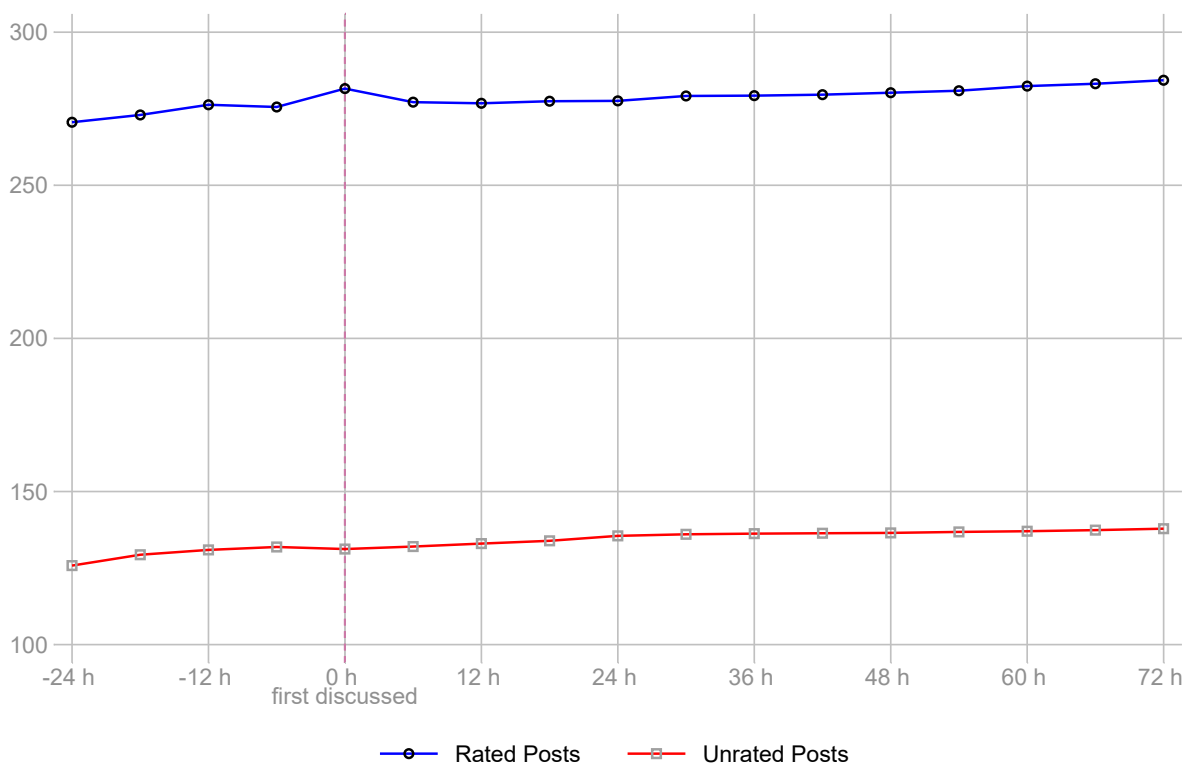
$$Y_{p(s)t} = \sum_{d=-24}^{192} \zeta_d Rate_{p(s)} \times \mathbb{1}_{t-t_{pr(s)}=d} + \mu_{p(s)} + \lambda_{s,t} + \varepsilon_{pst} \quad (4.4)$$

where now $t_{pr(s)}$ is the time of the first rating of a post within a checked story. We are interested in the ζ_d coefficients; again the parallel trends assumption can be assessed through the pre-treatment coefficients ζ_{d-24} to ζ_{d-6} . As discussed, we control for the story-time fixed effect $\lambda_{s,t}$, which takes out the overall trend of a checked story, in particular due to spillovers. We omit ζ_{d-1} since a rating at $d = 0$ within a story will have an immediate impact (in contrast to the consideration date on the story level).

If rating a post is efficient at reducing its spread on Facebook, we expect the coefficient ζ to be negative and statistically significant. Our identification assumption here is that the last post that is rated is "similar" to the first one that is not, i.e. that the fact-checker decides to stop rating posts at some point for random reasons. We believe that this assumption is satisfied for several reasons. First, note that the fact-checkers are only paid for the first post they rate (so they do not have any incentive to maximize the number of posts they rate, nor there is an economic trade-off between rating more posts or considering a novel story). Second, this assumption is consistent with several discussions we had with the members of the AFP Factual team regarding their decisions of whether to rate additional posts. Finally, at the time of the first rating within a story, rated and unrated posts are surprisingly well balanced, which underlines the anecdotal evidence that fact-checkers do not engage in much selection between different posts due to time constraints. Appendix Table 4.10 further shows that rated posts are younger than unrated posts, perhaps because this makes them easier to

find, but they are not significantly shared, liked or commented more often.

We show that the posts that are and are not rated were following parallel trends in terms of number of shares on Facebook before the AFP Factuel team first considered the story. Figure 4.4 confirms that, while there are some (insignificant) level differences between flagged and unflagged posts, their overall trend before the rating is flat.



Notes: Each line plots the average shares of flagged vs unflagged posts associated with checked stories relative to the date of the first rating of a post within a checked story.

Figure 4.4: Raw trends in shares of flagged vs unflagged posts within checked stories

4 Results

The two identification approaches have strengths and weakness. The post-level approach allows us to control for story level trends, capturing potential external factors giving visibility to the story. On the other hand, it rests on the assumption that the rated and unrated posts have similar trends absent treatment, which we can test indirectly, but without being able to document why certain posts are rated and not others. On the contrary, the story level analysis does not allow us to control for story level trends, but allows to control for some factors that can explain why certain stories were fact checked.

These two approaches also capture different behavioral responses. While the post level analysis really examines the impact of rating posts on Facebook, the story level approach captures the more general effect of the publication of the fact check. It can therefore capture direct effects of the fact check even on the unrated posts. It can also capture substitution effects, such as groups reposting the content after having their original posts. We now first turn to the story-level analysis.

4.1 Story-level analysis

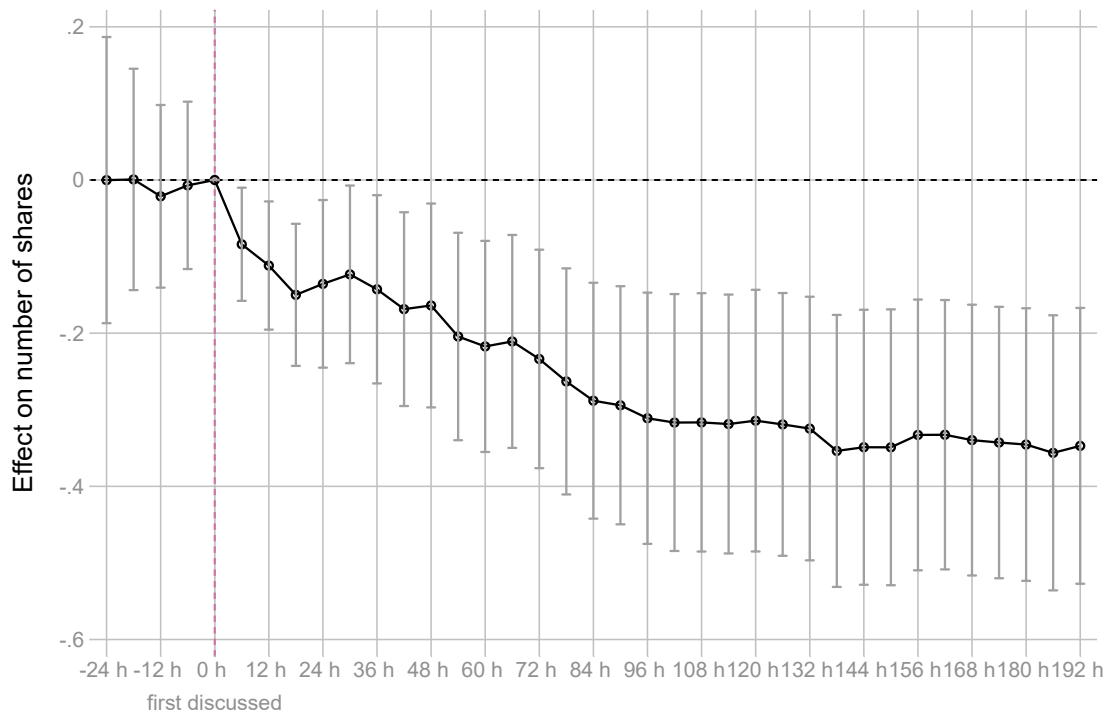
Event study We first report the event study estimates that allow us to re-visualise the parallel trend assumption before the date of consideration checked vs unchecked stories. We estimate equation (4.4) and report the result in Figure 4.5 and Appendix Table 4.12. It is evidence that there is no sign of any pre-trends, and the coefficients β_{-24} to β_{-6} are close to zero and not statistically significant). Furthermore, it appears that, compared to the non-fact-checked stories, the popularity of the fact-checked stories decreases following the fact-check. Interestingly, given we define the pre/post period with respect to the time of the first consideration by the AFP Factual team, we see that the drop happens “on impact” and becomes more pronounced after a day or two.

Regarding the magnitude of the effect, we see that, compared to the non-fact-check stories, the publication of the fact-check leads to a drop of around 30% in the total number of posts associated to a story.

Difference-in-differences estimates Table 4.4 reports the results of the estimation of equation (4.1). Compared to the event study, the difference-in-differences estimation allows us to better understand the overall magnitude of the effect of the fact-check on the popularity of the stories. In Column (1), we report this effect: we see that, compared to stories considered but not fact-checked by AFP Factual, the number of shares of the posts associated to the story dropped by 26% following the discussion in the morning meetings. This result is robust to controlling for day-of-the-week fixed effects (Column (2)).

Robustness This finding is robust to a variety of robustness checks. Most importantly, they are robust to restricting the control group to stories that were not checked because a lack of resources or infeasibility of the fact check, i.e. stories that otherwise would have most certainly been worthy of a fact check. This is reported in Appendix Figure 4.23 and the corresponding Difference-in-differences estimates reported in Appendix Table 4.17

Furthermore, Appendix Figures 4.25 and 4.24 with the associated tables 4.15 and 4.16 show



Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). Standard errors are clustered on the story level and 95% confidence intervals are reported. The dependent variable is the logarithm of the total number of shares of the posts. An observation is a story-time and we control for story and time fixed effects. The estimates are presented in Appendix Table 4.12. See the text for more details.

Figure 4.5: Story-level analysis: Event-study estimation

Table 4.4: Difference-in-differences estimates, story level

	Number of shares (logs)	
	(1)	(2)
Post * Fact-check	-0.26*** (0.08)	-0.26*** (0.08)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	23,842	23,842
Mean DepVar	5.16	5.16
Sd DepVar	2.47	2.47

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing checked stories with unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. Standard errors are clustered at the story level.

that the result is very similar when looking at comments and likes respectively instead of shares. We also see a significant effect at the extensive margin, i.e. on the number of posts active on stories (Appendix Figure 4.26 and Appendix Table 4.14). These findings, together with the parallel trends in these alternative measure of engagement that were discussed above, increase our confidence in interpreting the estimates as causal.

Heterogeneity by speed The main finding masks an important dimensions of heterogeneity with respect to the speed at which a claim is fact-checked. There are two dimensions, which matter in economically different ways. Table 4.5 shows that the effect is twice as large for fact checks that were published within two days after they were first discussed in the morning meeting. On the production side of fact checks, this concerns the way fact-checkers write fact checks, with an important trade-off of speed against quality. However, some fact checks require more elaborate research and investigations, e.g. when fact-checkers need to consult with experts to identify why certain parts of a claim are wrong, for example regarding technical topics such as Covid vaccines. At heart, these trade-off touch upon journalistic standards and can come with important costs in terms of the quality and credibility of fact-checks if their production were to be sped up further.

Table 4.5: Difference-in-differences estimates, depending on fact-checking speed

	Number of shares (log)	
	(1)	(2)
Post * Fact-check	-0.17*	-0.17*
	(0.09)	(0.09)
Post * Fact-check * Fast	-0.20**	-0.19**
	(0.10)	(0.10)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	23,842	23,842
Mean DepVar	5.16	5.16
Sd DepVar	2.47	2.47

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing checked stories with unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. The "Fast" dummy indicates stories that were checked within two days after consideration. Standard errors are clustered at the story level.

The second dimension concerns the speed at which journalists find a potential misinformation. As discussed above, journalists rely much on individual heuristics that are both time-consuming and imperfect in identifying checkworthy misinformation quickly. Importantly, the tools and algorithms provided by Facebook do not seem to support them very efficiently, as it is one of the least important sources of misinformation. Yet, as table 4.6 shows, the time window between the appearance of misinformation and its consideration by fact-checkers just as important to maximise the efficiency of fact-checking. Fact-checking is twice as impactful for stories that were considered within four days of the first posting on Facebook. This input into fact-checking – finding misinformation quickly – is best achieved by leveraging the technical and inside knowledge of big platforms like Facebook. Improvements in the speed of unearthing misinformation are also less likely to be affected by a speed-quality trade-off.

4.2 Post-level analysis and behavioral responses

We next present the results from the post-level analysis. This approach directly measures the impact on interactions users have with the posts. The story-level identification strategy captures in addition the more global effect the fact-check can have on circulation of the story and strategies posting groups might put in place in reaction to the rating. To do the analysis at the post-level, we restrict to stories that are fact-checked and to stories where less than 100% of posts that we identify as related are rated. Figure 4.27 shows that for this

Table 4.6: Difference-in-differences estimates, depending on how long it takes to find the story

	Number of shares (log)	
	(1)	(2)
Post * Fact-check	-0.16*	-0.16*
	(0.09)	(0.09)
Post * Fact-check * Quick consideration	-0.20**	-0.21**
	(0.10)	(0.10)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	23,842	23,842
Mean DepVar	5.16	5.16
Sd DepVar	2.47	2.47

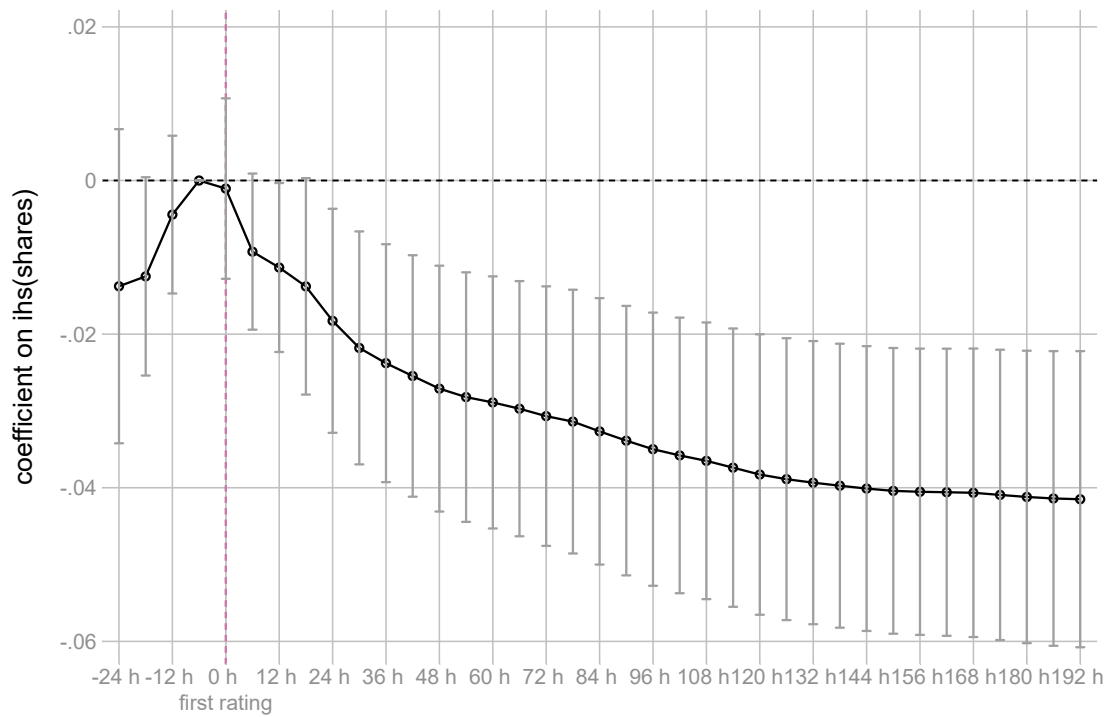
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing checked stories with unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. The "Quick consideration" dummy indicates stories that were considered within four days of the first posting in a story. Standard errors are clustered at the story level.

subsample of treated stories, the estimated treatment effect on the story level is somewhat less pronounced than in the overall sample, which appears natural given that by definition not all posts of a story are rated.

The first result from the post-level event study presented in 4.6 is that on average, the presence of a rating decreases circulation significantly on the post level by about 4%. This is lower than on the story level. As discussed, one factor for this is the fact that the story level estimate for the sub-sample of stories with within-story variation in post rating is somewhat lower, although this cannot fully explain the difference in magnitude.

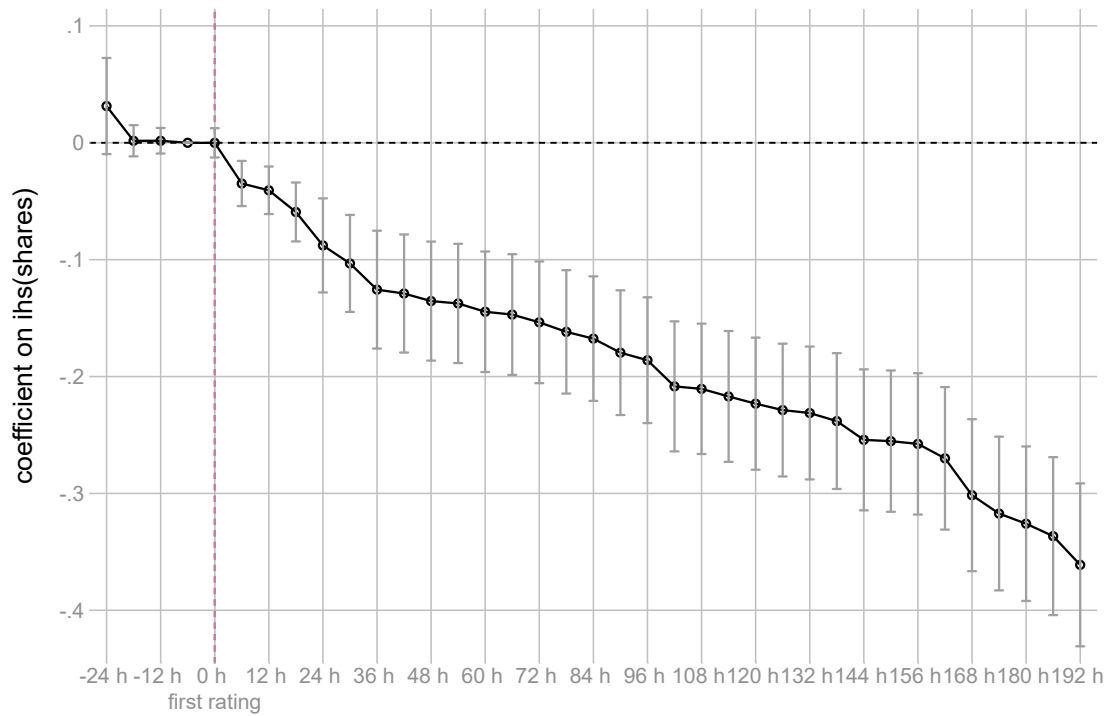
The average estimate hides an important behavioural response to the first rating by unrated posts. Appendix Figure 4.28 shows that posts that are not rated are actually *more* often deleted than other posts that are ultimately rated within a story. This makes sense from a user perspective: As discussed, many groups engage frequently in spreading misinformation and risk punishments for repeated offences, in particular in the form of downranking ("shadow-banning") by Facebook. Hence, as soon as a rating strikes other similar posts, users have an incentive to delete their post before it is rated as well. In short, there is an endogenous spillover of the rating on other accounts that posted similar content.

Reassuringly, when we excluded deleted posts from the control group as we do in figure 4.7, the magnitudes matches exactly the estimates from the story-level identification strategy.



Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). Standard errors are clustered on the story level and 95% confidence intervals are reported. The dependent variable is the logarithm of the total number of shares of the posts. An observation is a post-time and we control for post and story-time fixed effects. See the text for more details.

Figure 4.6: Post-level analysis: Event-study estimation



Notes: The figure reports the results of the event-study estimation of equation (4.4). Standard errors are clustered on the story level and 95% confidence intervals are reported. The dependent variable is the logarithm of the total number of shares of a posts. An observation is a post-time and we control for post and story-time fixed effects. We omit the last time period before the first rating of a post within a story. In contrast to figure 4.6, posts that are deleted in the control group are omitted. See the text for more details.

Figure 4.7: Post-level analysis: Event-study estimation, excluding deleted posts from the control group

5 Conclusion

In this ongoing work, we provide the first non-lab causal estimation of the effect of fact-checking on the spread of misinformation, thanks to a unique partnership with the largest fact-checking organization in the world. We show that Facebook posts related to stories that are fact-checked circulate less compared to Facebook posts related to stories that were considered but ultimately not fact-checked. Our preliminary findings hold importance implications for policy makers, platforms and fact-checkers:

First, we provide an independent causal estimate on the efficiency of fact-checking in practice. Our estimates of about 30% lower circulation are in contrast with Facebook’s occasionally communicated point estimate of a reduction of about 80%. Importantly, our estimate is transparent on the careful construction of a counterfactual, the measure used, alternative identification strategies and the robustness checks employed.

Second, we point out low-hanging fruits to improve the efficiency of fact-checking. We demonstrate that speed matters in two ways: The time it takes to find (potential) misinformation, and the time it takes to write a fact check. We argue that while the latter might involve a quality trade-off for fact-checkers, the former only requires better technical support for fact checkers by Facebook, in particular in unearthing relevant misinformation faster.

Third, we gather novel and detailed descriptive evidence on the production side of fact checks in the field. We show that fact-checking closely follows the news cycle. As a result of inadequate monitoring tools provided, we further document that fact-checkers use imperfect individual heuristics to find misinformation. Fact-checkers instead monitor only specific accounts, which locks them in a path-dependent fact-checking struggle, in which always the same “super spreaders” are checked. This risks to miss out on other (more susceptible) accounts off the radar. We will continue to explore this hypothesis in further expansions to this work.

Fourth, we describe potentially imperfect incentive structures in how fact checks are used by platforms. Since journalists are paid for writing fact checks and are not (and perhaps should not) be paid for flagging posts, posts that share the same misinformation go unflagged. As a result, we were able to match many posts that share identical misinformation with relatively cheap, simple and scalable techniques. At the same time, we only detect very marginal efforts by Facebook – equipped with much more resources and better technology – to automatize the flagging of identical misinformation. This poses a question of a possible misalignment of Facebook’s incentives to tackle misinformation efficiently.

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A Appendices

A.1 Additional Figures

Daniel
7 May at 19:36 · 🌐

Comment mentir avec les statistiques ? C'est très simple, au lieu de prendre, une échelle de temps de 10000 ans, qui voyait les températures au Groenland (et ailleurs) bien plus chaude qu'aujourd'hui de2,5° svp, vous attendez le petit âge glaciaire, et vous prenez comme nouvelle origine, il y a 140 ans en criant au réchauffement climatique. Montrez ces 4 minutes à tout le monde !

False information. Checked by independent fact-checkers.

[See why](#)

🙄 1 36 shares

👍 Like 💬 Comment ➦ Share

Write a public comment...

Related articles

Agence France Presse **Fact-check**
Cette ancienne vidéo sur des températures d'une carotte de glace du Groenland ne prouve pas que le réchauffement climatique est naturel
Le réchauffement climatique et son origine anthropique font aujourd'hui l'objet d'un ...

Figure 4.8: Post rated False



Figure 4.9: Post rated Partially False



Figure 4.10: Unflagged post in rated story (see 4.9)

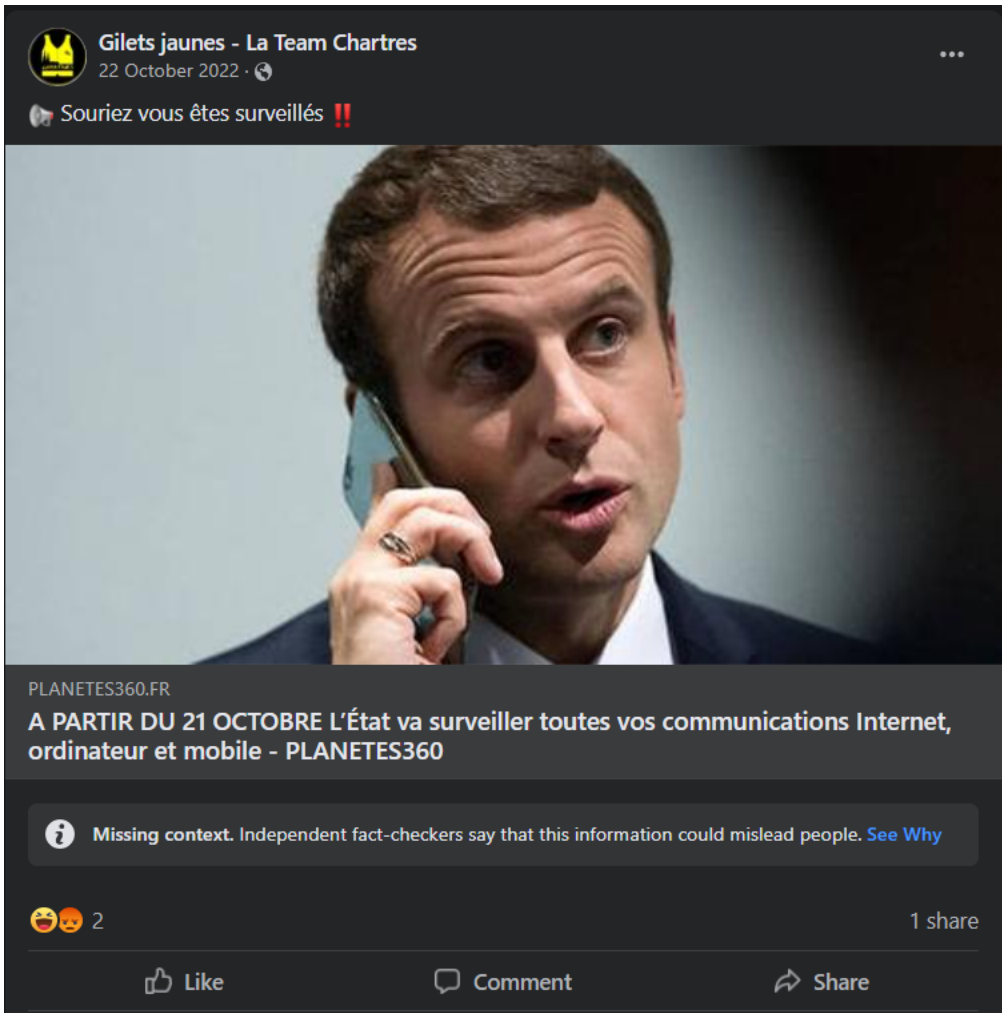


Figure 4.11: Post rated Missing Context

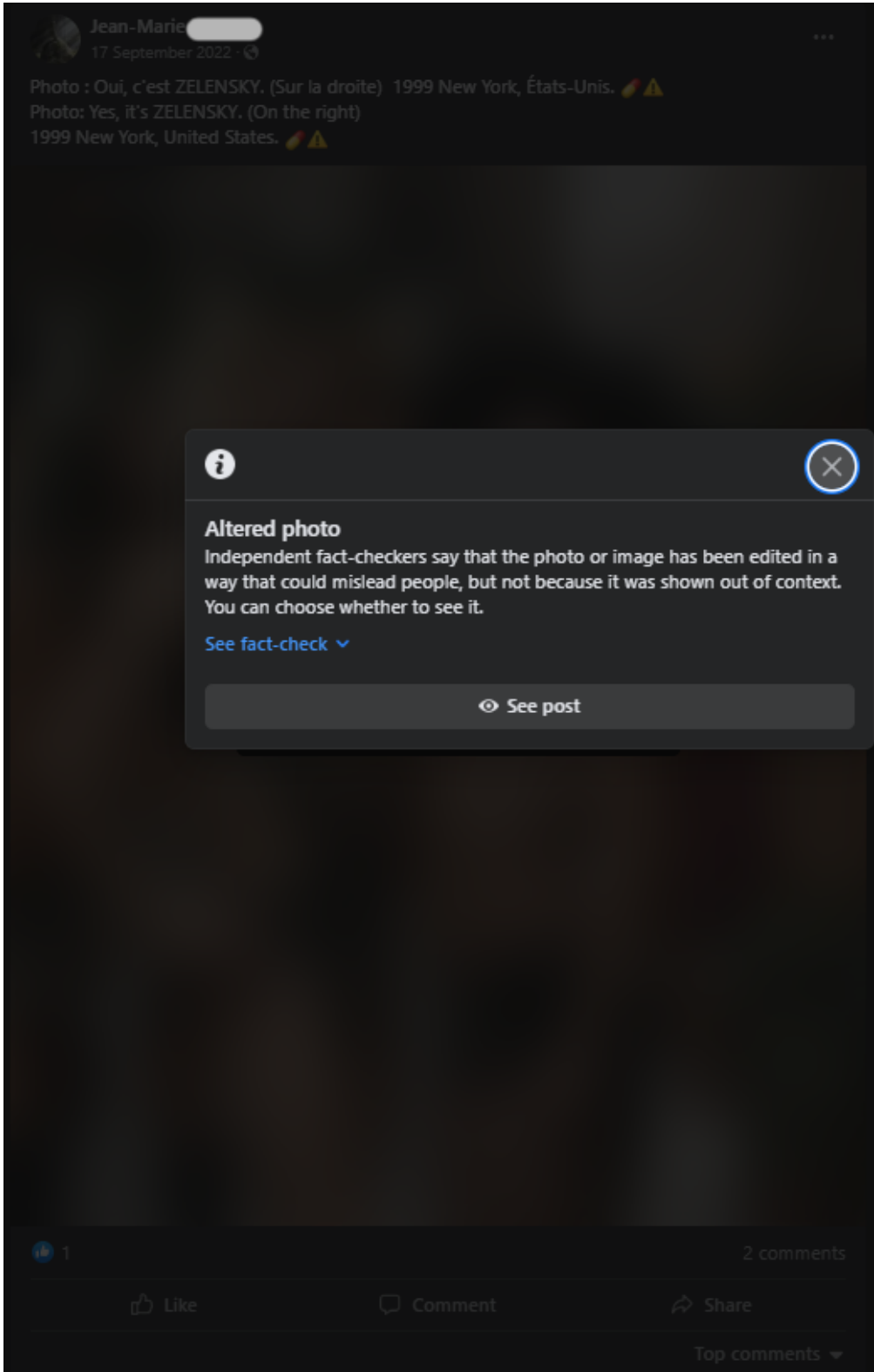


Figure 4.12: Illustration: Removing Blurring Barrier

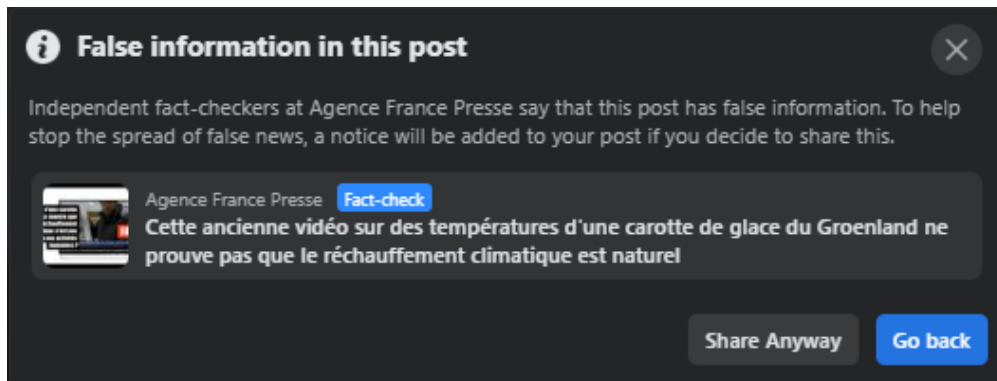


Figure 4.13: Illustration: Removing Sharing Barrier

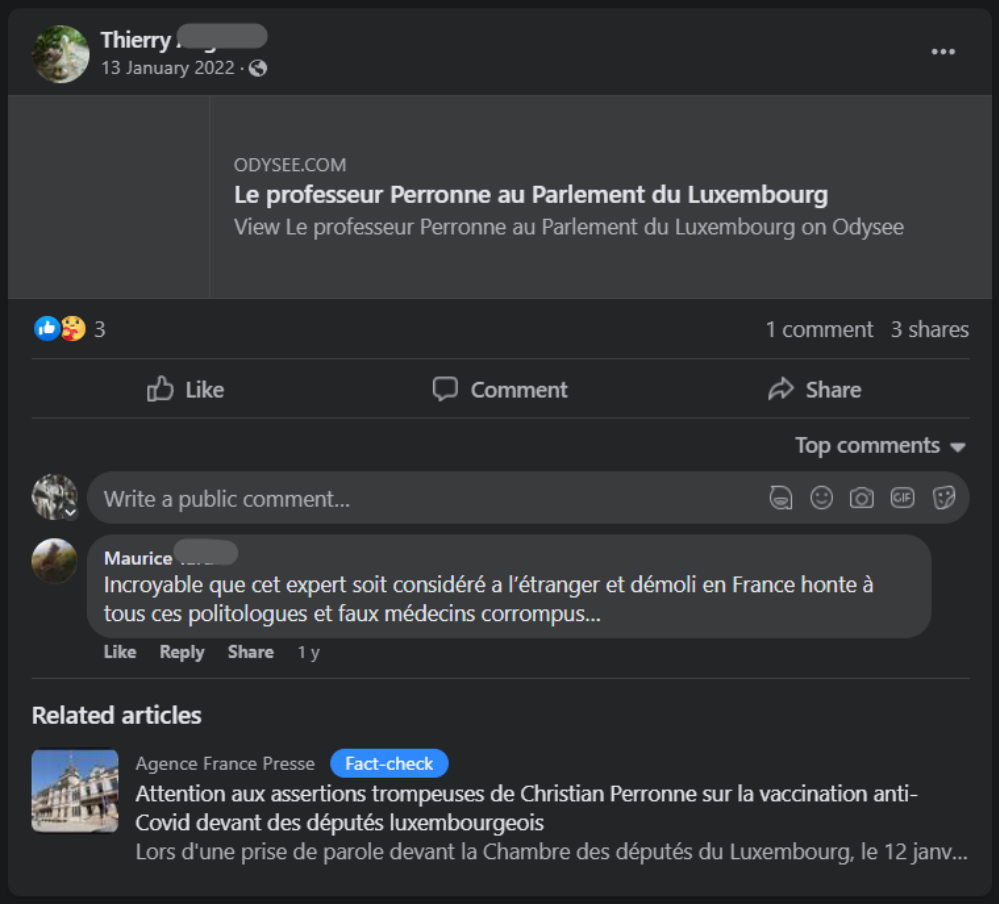
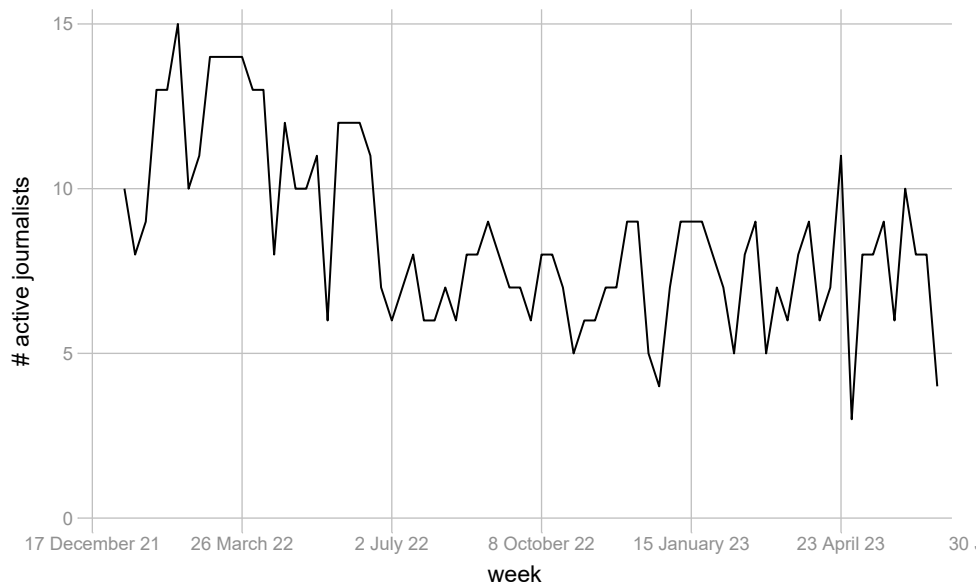


Figure 4.14: Illustration: Post with Fact-check link only



Dips: Winter holidays: 19.02.-07.03. , entre-tours: 10.-24.04.

Notes: The figure plots the active number of fact-checkers over the study period based on the fact-checks published. Note that the numbers represent a lower bound, as fact-checkers can work on other tasks than publishing fact-checks (e.g. answering WhatsApp requests or TikTok moderation requests) and sometimes work longer than one week. Large dips usually represent the holiday season around public holidays.

Figure 4.15: Active Fact-checkers over time

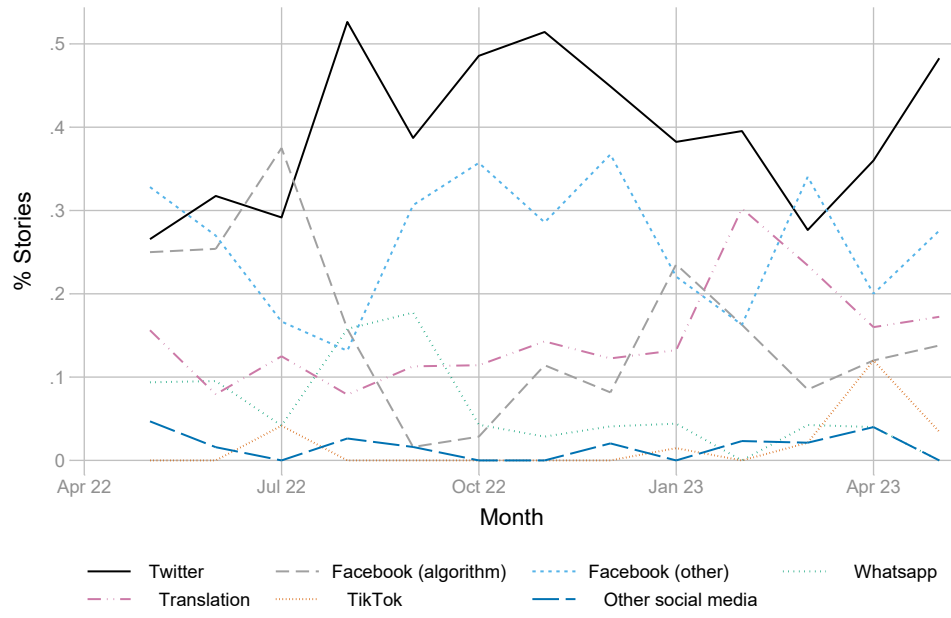


Figure 4.16: Origin of the stories: Evolution over time

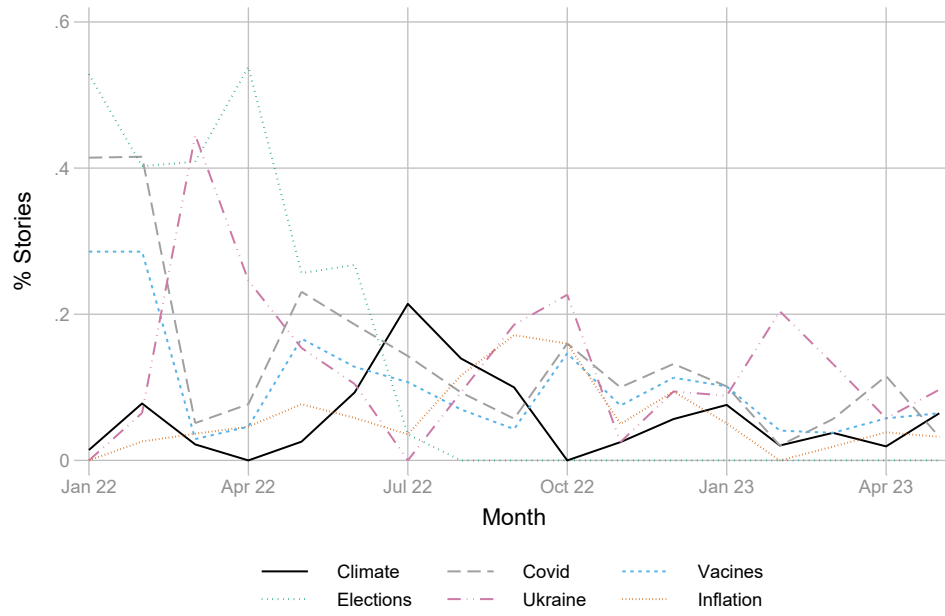
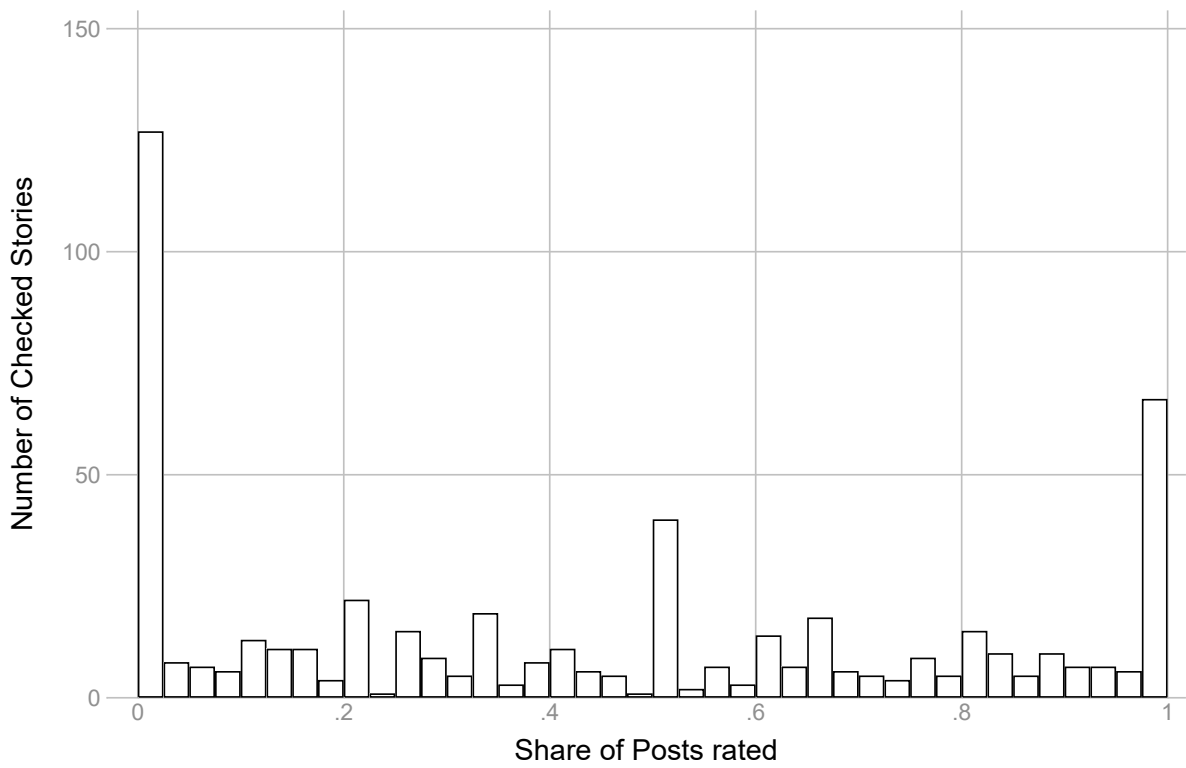
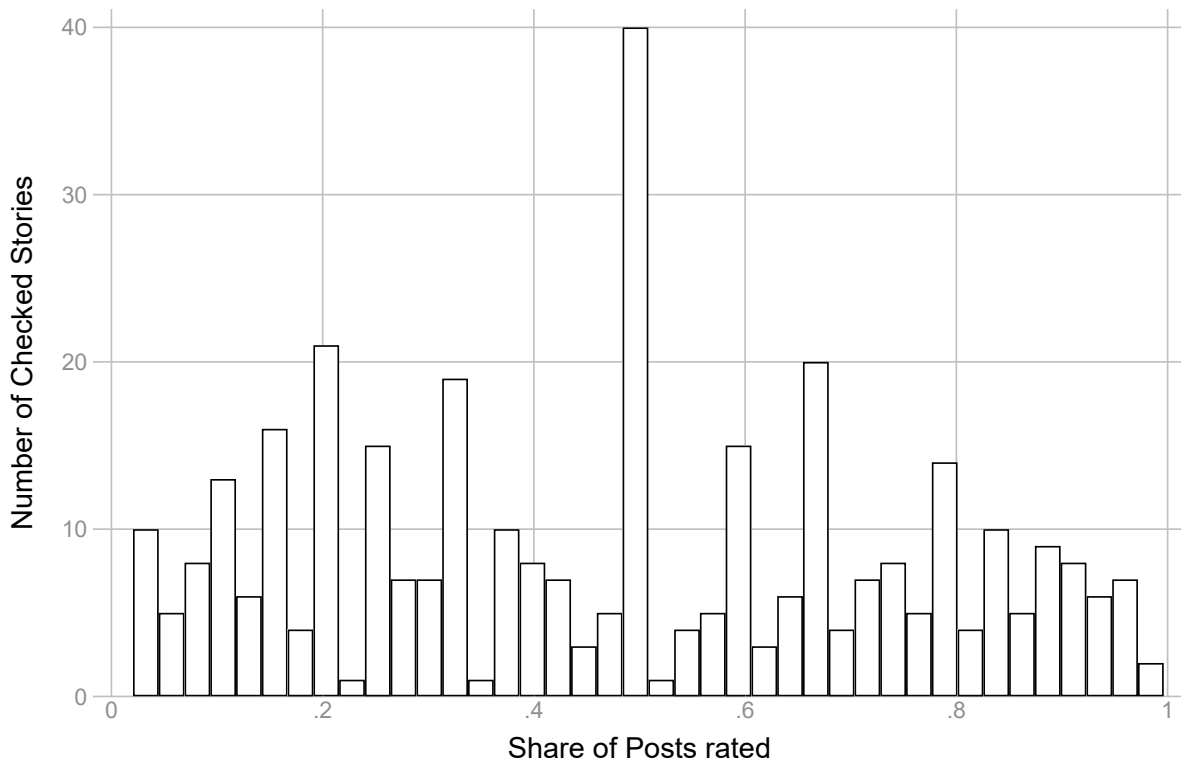


Figure 4.17: Topics over time



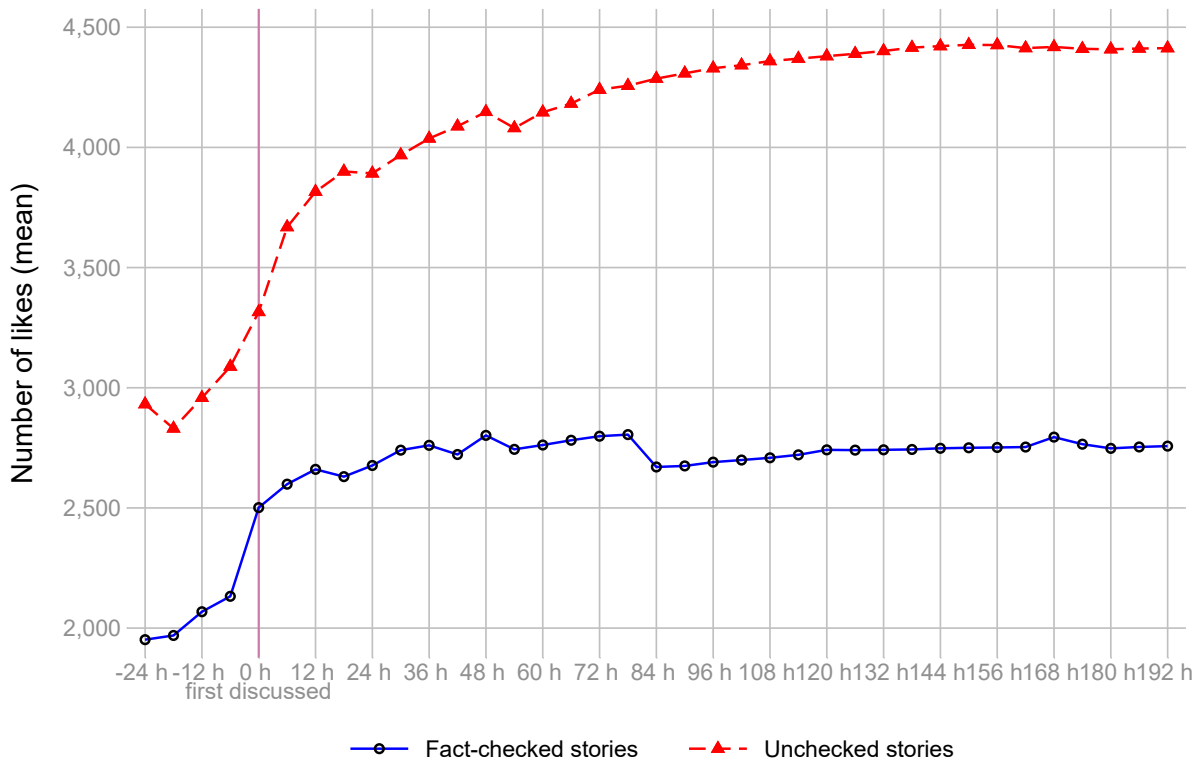
Notes: The figure plots the number of stories over the share of rated posts within a story ranging from [0;1]. The total number of stories is 538.

Figure 4.18: Share of rated posts within checked stories (N=538)



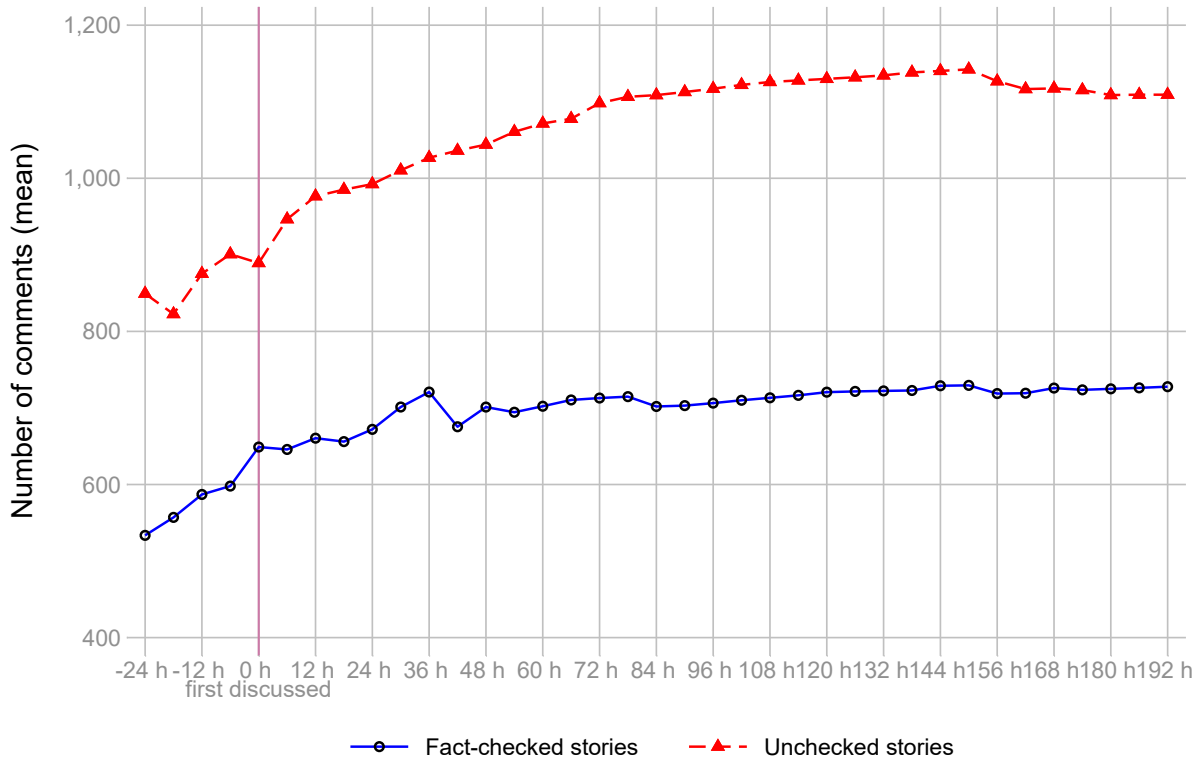
Notes: The figure plots the number of stories over the share of rated posts within a story ranging from (0;1), i.e. restricted to checked stories in which some but not all posts are rated. The total number of stories is 349, which corresponds to the sample used for the post-level identification strategy.

Figure 4.19: Share of rated posts within checked stories for stories with variation in post rating (N=349)



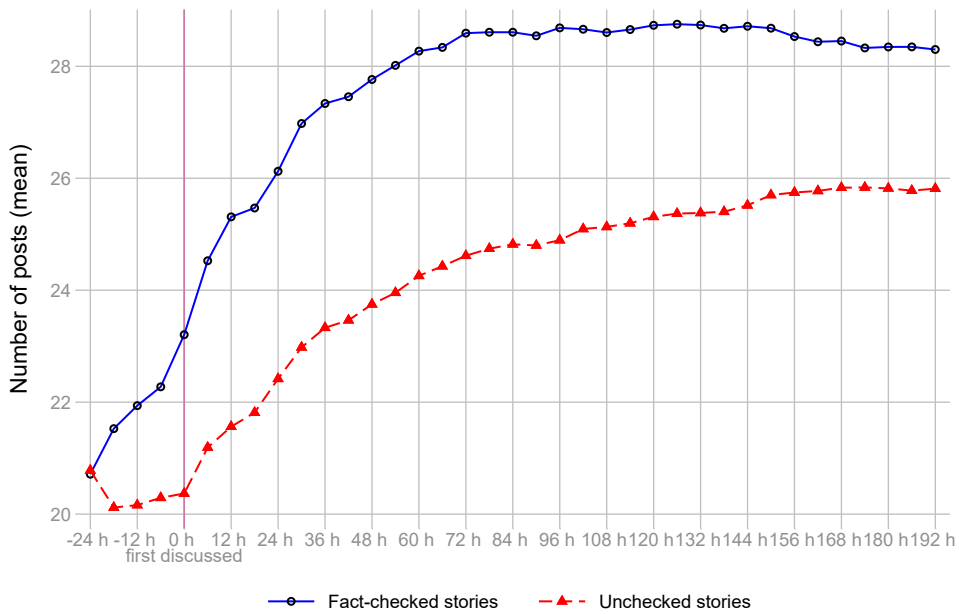
Notes: Each line plots the average sum of all likes of posts associated with a story for checked vs unchecked stories relative to the date of consideration of a story by the fact-checkers.

Figure 4.20: Raw trends in sum of story likes



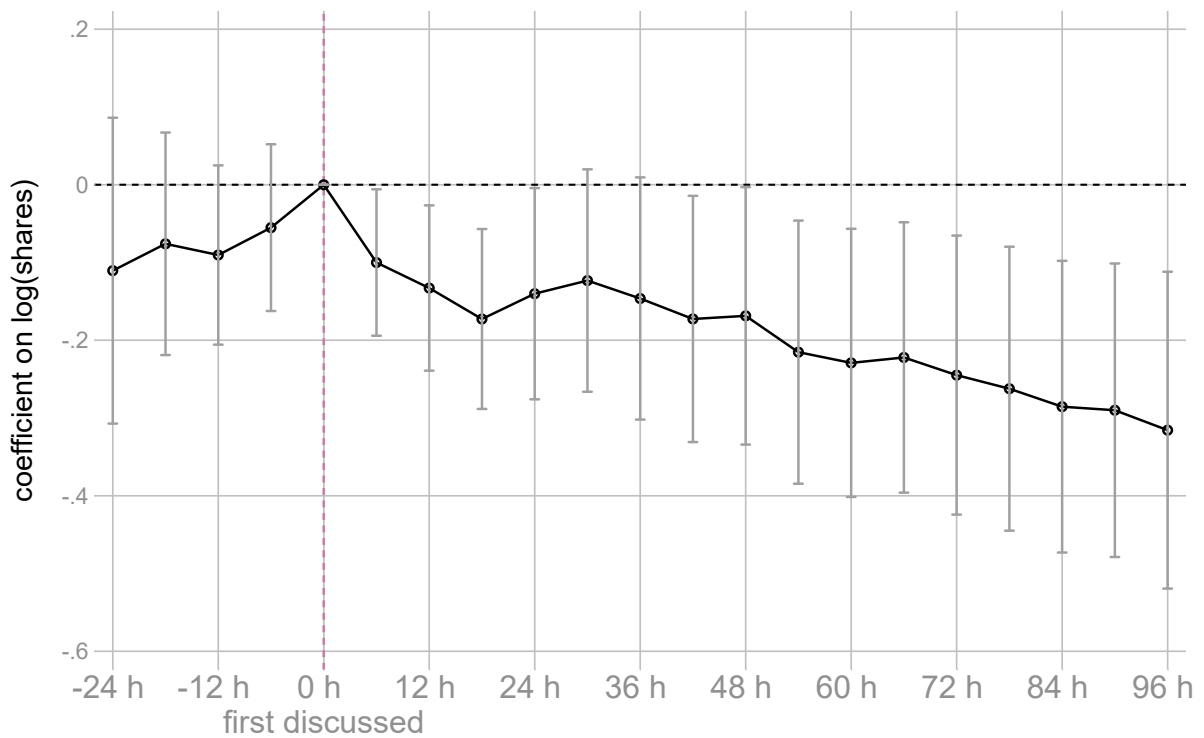
Notes: Each line plots the average sum of the number of comments of posts associated with a story for checked vs unchecked stories relative to the date of consideration of a story by the fact-checkers.

Figure 4.21: Raw trends in sum of story comments



Notes: Each line plots the average sum of the number of active posts associated with a story for checked vs unchecked stories relative to the date of consideration of a story by the fact-checkers.

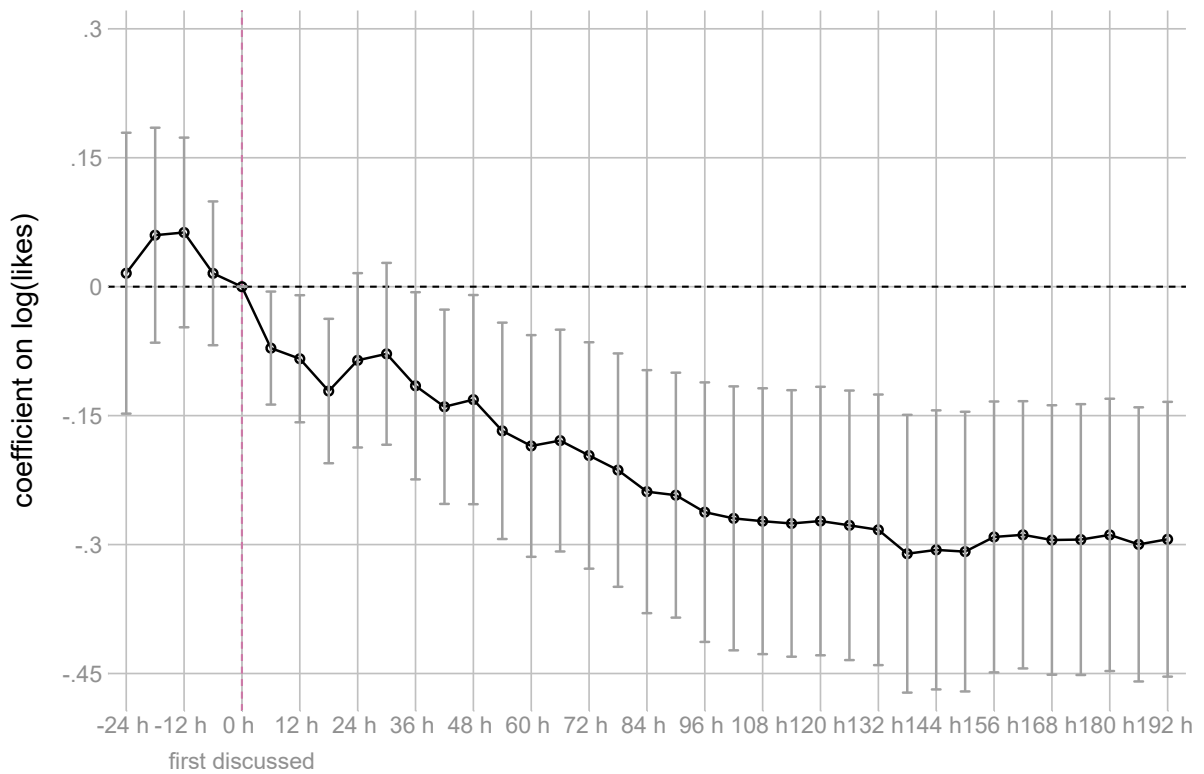
Figure 4.22: Raw trends in number of active posts



Control: Stories set aside because of lack of resources.

Notes: The figure reports the results of the event-study estimation of equation (4.4). The dependent variable is the logarithm of the total number of shares of posts associated with checked vs unchecked stories. The control group is restricted to only stories that were unchecked because they were infeasible or lacking resources (time, fact-checkers) made a check impossible. An observation is a story-time and we control for story and time fixed effects. See the text for more details.

Figure 4.23: Event study on the log of shares, restricted control group



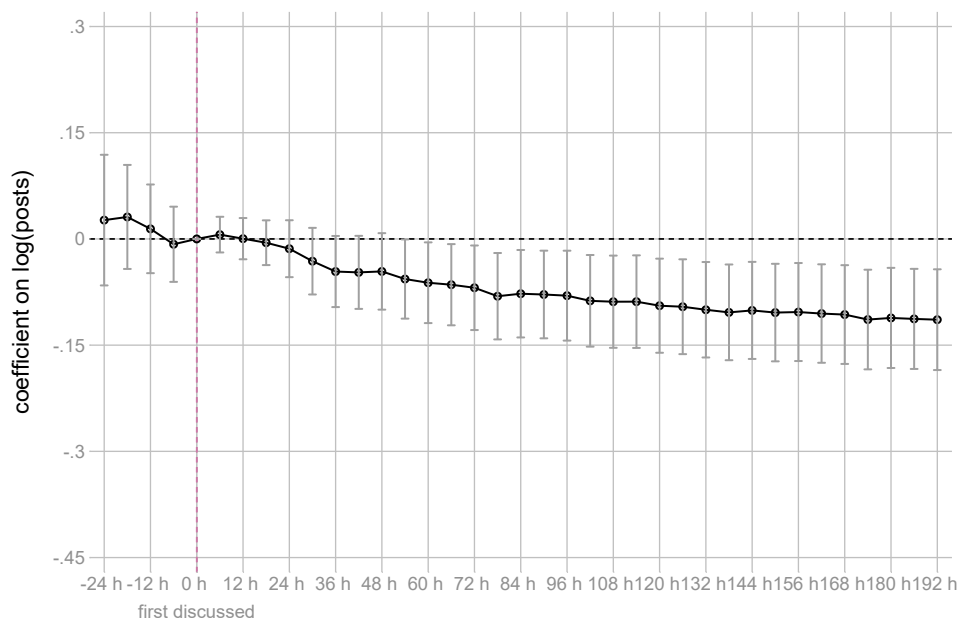
Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). The dependent variable is the logarithm of the total number of likes of posts associated with checked vs unchecked stories. An observation is a story-time and we control for story and time fixed effects. See the text for more details.

Figure 4.24: Event study on the log of likes



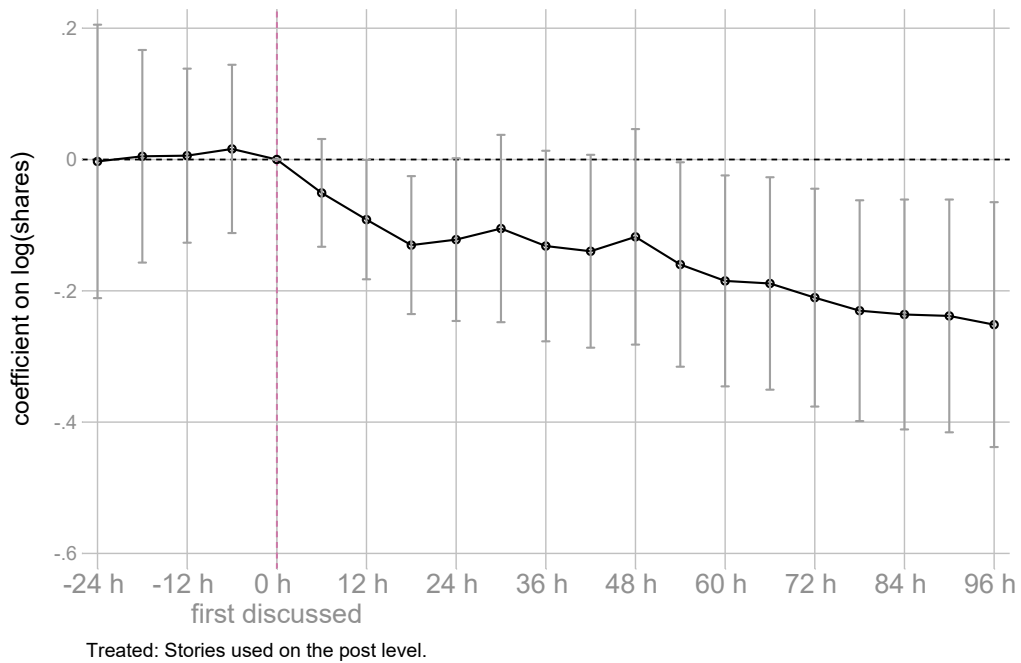
Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). The dependent variable is the logarithm of the total number of comments of posts associated with checked vs unchecked stories. An observation is a story-time and we control for story and time fixed effects. See the text for more details.

Figure 4.25: Event study on the log of comments



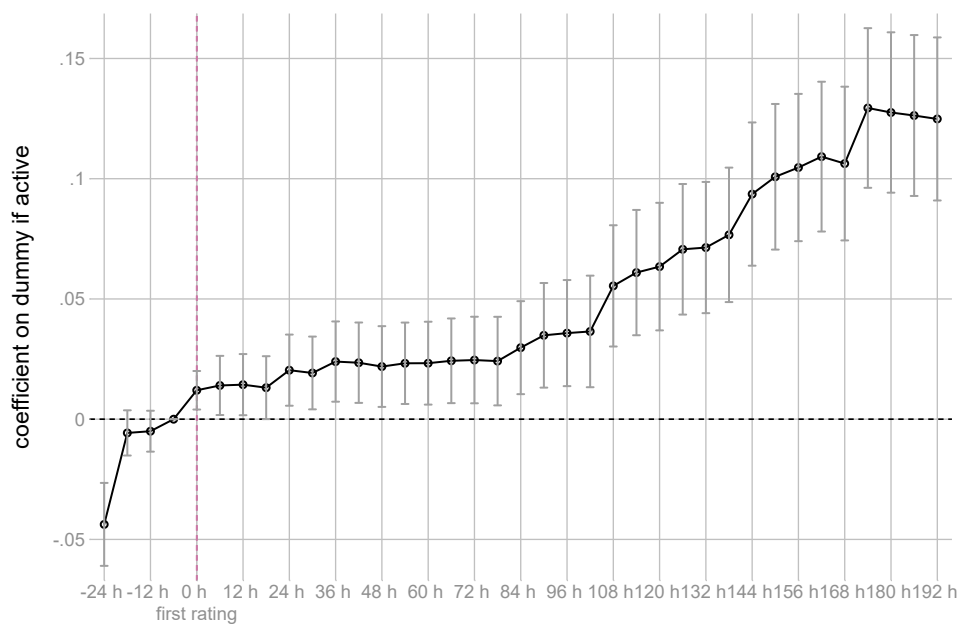
Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). The dependent variable is the logarithm of the total number of active posts associated with checked vs unchecked stories. An observation is a story-time and we control for story and time fixed effects. See the text for more details.

Figure 4.26: Event study on the log of active posts



Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). The dependent variable is the logarithm of the total number of shares associated with checked vs unchecked stories. An observation is a story-time and we control for story and time fixed effects. Stories that were not checked because they were judged not viral enough are excluded from the control group. The treated stories are restricted to stories with within-story variation in the share of posts that were rated, i.e. stories with no rating or stories in which all posts were rated are excluded. Standard errors are clustered at the story level. See the text for more details.

Figure 4.27: Event study on log of shares, restricting treated stories with post-level rating variation



Notes: The figure reports the results of the event-study estimation (estimation of equation (4.4)). Standard errors are clustered on the story level and 95% confidence intervals are reported. The dependent variable is the logarithm of the total number of shares of a posts. An observation is a post-time and we control for post and story-time fixed effects. We omit the last time period before the first rating of a post within a story. See the text for more details.

Figure 4.28: Post-level Event study on dummy if post is active

A.2 Additional Tables

Table 4.7: Stories discussed during the morning meetings: Descriptive statistics on political statements

	Mean	SD	Min	Max	N
Origin of story					
Origin: Twitter	0.22	0.42	0.00	1.00	114
Origin: Facebook other	0.00	0.00	0.00	0.00	114
Origin: Translation	0.00	0.00	0.00	0.00	114
Origin: Media	0.78	0.42	0.00	1.00	114
Origin: Unknown	0.51	0.50	0.00	1.00	229
Topic of story					
Topic: Climate	0.08	0.27	0.00	1.00	229
Topic: Covid	0.06	0.24	0.00	1.00	229
Topic: Vaccines	0.02	0.15	0.00	1.00	229
Topic: Elections	0.69	0.47	0.00	1.00	229
Topic: Ukraine	0.05	0.22	0.00	1.00	229
Topic: Inflation	0.07	0.26	0.00	1.00	229
Topic unclassified	0.18	0.39	0.00	1.00	229
Fact check information					
Story fact checked	0.41	0.49	0.00	1.00	229
Length FC (1000 words)	11.17	4.10	3.31	27.72	95
Rating: false	0.09	0.29	0.00	1.00	95
Rating: missing context	0.02	0.14	0.00	1.00	95
Rating: partly false	0.01	0.10	0.00	1.00	95

Notes: The table reports descriptive statistics on the political statements discussed during the AFP Factual team’s morning meetings. An observation in a story. Only the stories related to political statements are included. More details are provided in the text.

Table 4.8: Post-level descriptive statistics

	Mean	Median	SD	Min	Max	N
Posts ratings						
Rated	0.60	1.0	0.49	0	1	16874
Flag: False	0.72	1.0	0.45	0	1	10123
Flag: Missing Context	0.17	0.0	0.38	0	1	10123
Flag: Partly False	0.09	0.0	0.28	0	1	10123
Flag: Altered content	0.02	0.0	0.15	0	1	10123
At Consideration						
Days (posted - considered)	20.69	2.7	152.12	-423	2,731	16761
# Shares	100.87	1.0	2,251.51	0	176,547	9066
# Likes	121.06	1.0	1,630.69	0	103,236	9066
# Comments	28.53	0.0	293.41	0	16,370	9066
At Rating						
Days (posted - rated)	20.67	10.8	76.30	-326	2,208	10080
# Shares	124.50	1.0	2,815.69	0	176,547	4852
# Likes	92.34	1.0	1,202.27	0	58,824	4852
# Comments	28.42	0.0	249.08	0	8,383	4853

Notes: The table reports descriptive statistics on the reasons on posts in our sample. Note that posts can be posted after the story was considered by the AFD and after the first rating within a story. For some posts, we do not have the information on when they were posted.

Table 4.9: Story-level balance check: Checked vs unchecked stories

	(1)		(2)		(3)		(4)	
	All		Not Checked		Checked		Difference t-test	
	mean	sd	mean	sd	mean	sd	b	p
# Shares	1868.74	9519.93	1918.68	8703.65	1840.55	10017.44	78.13	(0.90)
# Likes	2204.60	8806.61	2426.68	10870.69	2068.18	7221.30	358.50	(0.59)
# Comments	561.32	2130.06	627.79	2888.29	520.03	1465.33	107.76	(0.52)
Trend in shares	242.35	3158.94	231.60	2223.72	249.58	3632.31	-17.98	(0.93)
Origin: Twitter	0.40	0.49	0.48	0.50	0.34	0.47	0.14	(0.00)
Origin: Facebook claim	0.15	0.36	0.13	0.34	0.16	0.37	-0.03	(0.34)
Origin: Facebook other	0.27	0.44	0.26	0.44	0.28	0.45	-0.02	(0.51)
Origin: Whatsapp	0.06	0.25	0.09	0.28	0.05	0.21	0.04	(0.04)
Origin: Translation	0.14	0.34	0.05	0.21	0.20	0.40	-0.16	(0.00)
Origin: Media	0.01	0.11	0.01	0.10	0.01	0.11	-0.00	(0.82)
Origin: TikTok	0.02	0.13	0.01	0.10	0.02	0.15	-0.01	(0.21)
Topic: Oth. Soc. Med.	0.02	0.13	0.02	0.13	0.02	0.13	0.00	(0.99)
Topic: Climate	0.04	0.20	0.09	0.29	0.01	0.11	0.08	(0.00)
Topic: Covid	0.18	0.39	0.26	0.44	0.14	0.34	0.13	(0.00)
Topic: Vacines	0.14	0.35	0.22	0.42	0.09	0.29	0.13	(0.00)
Topic: Elections	0.05	0.22	0.04	0.18	0.06	0.24	-0.03	(0.05)
Topic: Ukraine	0.18	0.39	0.21	0.41	0.17	0.37	0.04	(0.13)
Topic: Inflation	0.06	0.23	0.09	0.29	0.03	0.18	0.06	(0.00)
Topic: Regional	0.03	0.17	0.08	0.27	0.00	0.00	0.08	(0.00)
Topic: Other health	0.03	0.16	0.06	0.24	0.00	0.04	0.06	(0.00)
Topic: Other	0.00	0.00	0.00	0.00	0.00	0.00	0.00	(.)
Observations	879		340		538		878	

Notes: The table reports the balance of measures of circulation for checked vs unchecked stories. Measures of circulation are taken at the time of consideration.

Table 4.10: Post-level balance check: Rated vs unrated posts of checked stories

	(1)		(2)		(3)		(4)	
	All		Not Rated		Rated		Difference t-test	
	mean	sd	mean	sd	mean	sd	b	p
Days (posted - considered)	12.72	109.66	18.18	153.96	9.91	77.42	8.27	(0.00)
# Shares	80.09	2167.13	49.04	550.25	95.63	2625.85	-46.59	(0.22)
# Likes	92.33	983.48	100.09	688.82	88.45	1101.78	11.63	(0.56)
# Comments	22.23	212.99	25.56	214.26	20.56	212.35	5.00	(0.32)
Observations	14652		4979		9673		14652	

Notes: The table reports the balance of measures of circulation for rated vs unrated posts in checked stories in which some but not all posts were rated. For some posts, we do not have the information on when they were posted.

Table 4.11: Accounts publishing posts: Descriptive statistics

	nr_stories	nr_posts	suscribers
1 - Pour la demission d’Emmanuel Macron	167	430	59168
2 - Reinfo Gard collectif extraordin...	117	234	15532
3 - Tempete en marche contre les dic...	107	170	6362
4 - Magazine NEXUS	90	4614	1184508
5 - Mopti24info	90	4614	1184508
6 - NON AU PASS VACCINAL	82	212	21174
7 - LES RESISTANTS CONTRE LE PASS SA...	79	213	15007
8 - Le groupe des non vaccines	74	130	12954
9 - odyssee.com	73	123	5960
10 - Stop a la dictature sanitaire	64	128	52801
11 - Stop a la mascarade ! On veut la...	60	149	11774
12 - La liberte commence par nos enfants	58	104	7353
13 - RESISTANCE!!! Nous Voulons Retro...	57	147	14099
14 - Le pouvoir du peuple pour le peu...	55	69	12959
15 - LES AMOUREUX D’UNE FRANCE LIBEREE	55	77	4556
16 - La Verite Cachee	53	61	7102
17 - Collectif ANTI-MASQUES !	53	68	4691
18 - Les Francais contre Macron	52	64	45281
19 - ANTI-COVID 19 France	52	82	11279
20 - S’informer autrement !	51	67	6080
21 - Les pestiferes (librement non-va...	50	160	20944
22 - Oliv oliv 2	49	81	6420
23 - Contre la vaccination, et la dic...	49	65	2346
24 - Stop a la dictature sanitaire (bis)	47	88	4800
25 - COMPTEUR REEL : Qui Sont Les Man...	47	79	17608
26 - Reaction 19	47	198	20938
27 - mouvement citoyen anti-pass vacc...	46	57	2727
28 - Groupe de soutien a Alexandra He...	45	116	8050
29 - Sentinelle Guadeloupe	44	72	5134

Notes: The table reports descriptive statistics on the accounts who publish at least one post in our data. An observation is an account. More details are provided in the text.

Table 4.12: Story-level analysis: Difference-in-differences estimations

	Number of shares (log)	
	(1)	(2)
-24h	-0.00 (0.10)	-0.00 (0.10)
-18h	0.00 (0.07)	0.00 (0.07)
-12h	-0.02 (0.06)	-0.02 (0.06)
-6h	-0.01 (0.06)	-0.01 (0.06)
6h	-0.08** (0.04)	-0.08** (0.04)
12h	-0.11*** (0.04)	-0.11*** (0.04)
18h	-0.15*** (0.05)	-0.15*** (0.05)
24h	-0.14** (0.06)	-0.14** (0.06)
30h	-0.13** (0.06)	-0.12** (0.06)
36h	-0.14** (0.06)	-0.14** (0.06)
42h	-0.17*** (0.06)	-0.17*** (0.06)
48h	-0.17** (0.07)	-0.16** (0.07)
54h	-0.21*** (0.07)	-0.20*** (0.07)
60h	-0.22*** (0.07)	-0.22*** (0.07)
66h	-0.21*** (0.07)	-0.21*** (0.07)
72h	-0.23*** (0.07)	-0.23*** (0.07)
78h	-0.26*** (0.08)	-0.26*** (0.08)
84h	-0.29*** (0.08)	-0.29*** (0.08)
90h	-0.29*** (0.08)	-0.29*** (0.08)
96h	-0.31*** (0.08)	-0.31*** (0.08)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week X		✓
Observations	23,842	23,842
Mean DepVar	5.2	5.2
Sd DepVar	349.5	2.5

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are estimated using an OLS (standard errors in parentheses are clustered at the story level). An observation is a story-time. The dependent variable is the logarithm of the total number of shares of the posts. All the specifications include story and time fixed

Table 4.13: Difference-in-differences estimates for stories with post-level rating variation

	Number of shares (log)	Number of media
	(1)	(2)
Post * Fact-check	-0.17** (0.08)	-0.17** (0.08)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	8,349	8,349
Mean DepVar	5.4	5.4
Sd DepVar	2.4	2.4

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing checked stories with unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. The treated stories are restricted to stories with within-story variation in the share of posts that were rated, i.e. stories with no rating or stories in which all posts were rated are excluded. Standard errors are clustered at the story level.

Table 4.14: Difference-in-differences estimates with log of shares as outcome

	log(posts)	log(posts)
	(1)	(2)
Post * Fact-check	-0.09** (0.04)	-0.09** (0.04)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	24,075	24,075
Mean DepVar	1.98	1.98
Sd DepVar	1.34	1.34

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the number of shares of all posts associated with checked stories vs unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. Standard errors are clustered at the story level.

Table 4.15: Difference-in-differences estimates with log of comments as outcome

	log(comments)	log(comments)
	(1)	(2)
Post * Fact-check	-0.30*** (0.08)	-0.30*** (0.08)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	22,749	22,749
Mean DepVar	4.46	4.46
Sd DepVar	2.31	2.31

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the number of comments of all posts associated with checked stories vs unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. Standard errors are clustered at the story level.

Table 4.16: Difference-in-differences estimates with log of likes as outcome

	log(likes)	log(likes)
	(1)	(2)
Post * Fact-check	-0.25*** (0.07)	-0.25*** (0.07)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	23,427	23,427
Mean DepVar	5.20	5.20
Sd DepVar	2.60	2.60

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the number of likes of all posts associated with checked stories vs unchecked stories before and after their first discussion in the morning meeting. Stories that were not checked because they were judged not viral enough are excluded from the control group. Standard errors are clustered at the story level.

Table 4.17: Difference-in-differences estimates with the log of shares as outcome, restricted control group

	<u>log(shares)</u>	<u>log(shares)</u>
	(1)	(2)
Post * Fact-check	-0.14*	-0.14*
	(0.08)	(0.08)
Story FEs	✓	✓
Time FEs	✓	✓
Day of the week		✓
Observations	11,722	11,722
Mean DepVar	5.1	5.1
Sd DepVar	2.5	2.5

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the number of shares of all posts associated with checked stories vs unchecked stories before and after their first discussion in the morning meeting. The control group only comprises stories that were not checked because they were judged infeasible or because of a lack of resources (fact-checkers, time, sources). Standard errors are clustered at the story level.

Table 4.18: Post-level event study. Treatment: Only posts flagged as "false"

	<u>log(shares)</u>	<u>log(shares)</u>
	(1)	(2)
Post * Fact-check	-0.01*	-0.03***
	(0.01)	(0.01)
Story*time FEs	✓	
Time FEs		✓
Post FEs	✓	✓
Observations	63,156	63,795
Mean DepVar	1.7	1.7
Sd DepVar	1.8	1.8

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the shares of flagged vs unflagged posts within a checked stories before and after the first post was rated within a story. Only posts that were flagged as "false" are included as treated posts, with other flags being dropped. Standard errors are clustered at the story level.

Table 4.19: Post-level event study. Treatment: Only posts flagged as "missing context"

	<u>log(shares)</u>	<u>log(shares)</u>
	(1)	(2)
Post * Fact-check	-0.03*** (0.01)	-0.05*** (0.01)
Story*time FEs	✓	
Time FEs		✓
Post FEs	✓	✓
Observations	34,524	35,309
Mean DepVar	1.8	1.8
Sd DepVar	1.8	1.8

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the Difference-in-difference estimates by comparing the log of the shares of flagged vs unflagged posts within a checked stories before and after the first post was rated within a story. Only posts that were flagged as "missing context" are included as treated posts, with other flags being dropped. Standard errors are clustered at the story level.

Conclusion

This thesis delves into critical intersections between politics and media, exploring the influence of media consumption, populist persuasion, media biases, and misinformation. In four distinct articles, my research sheds light on the complex dynamics characterizing these domains.

The first chapter, "The 'Valley of the Clueless'? Media Consumption and Populist Persuasion," highlights the significance of media consumption in the emergence of populist political parties. Utilizing a historical natural experiment in Germany, I demonstrate how differences in media consumption influence voting decisions and the persuasive power of populist narratives, particularly on social media.

The second chapter, "Hosting Media Bias: Evidence from the Universe of French" (co-authored with Julia Cagé, Nicolas Hervé, and Camille Urvoy), delves into media biases and political representation in French media. Drawing from a vast dataset on television and radio shows spanning 20 years, we examine the role of editorial choices and political preferences of hosts in shaping media coverage.

The third chapter, "The Far-Right Donation Gap" (co-authored with Julia Cagé and Yuchen Huang), examines the decline in charitable donations in Western countries, with a specific focus on the propensity of far-right voters to donate. We emphasize the importance of social norms in the drop of charitable giving and its implications for the charitable sector.

Finally, the fourth chapter, "Fact-Checking and Misinformation: Evidence from the Market Leader" (ongoing work with Julia Cagé, Nathan Gallo, and Emeric Henry), investigates the effects of fact-checking on the dissemination of misinformation. Collaborating with the "Agence France Presse" (AFP), we analyze the impact of fact-checking on the circulation of information and uncover unforeseen effects on misinformation spread due to audience reactions to fact-check ratings.

Through these four articles, my thesis provides a holistic perspective on the interactions

between media, politics, populist persuasion, media biases, and misinformation. My work contributes valuable insights into these pressing issues and enhances our understanding of contemporary media and political dynamics.

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L'Économie Politique de la Production et de la Consommation des Médias en France

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Jury

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Résumé de la thèse en français

LES modes de consommation et de production des médias changent à un rythme effréné. Du côté de la production, les journalistes, gardiens traditionnels de l'information de haute qualité, voient leur modèle commercial menacé par les médias sociaux, où tout consommateur peut également devenir producteur d'informations. Du côté de la consommation, les électeurs peuvent dissocier les médias qu'ils consomment : alors que les journaux et les diffuseurs (publics) regroupent la politique avec le divertissement, les algorithmes des médias sociaux personnalisent tout le contenu pour maximiser l'interaction des utilisateurs.

Les politiciens et les propriétaires d'entreprises s'adaptent rapidement à ces changements pour maximiser leurs profits et leurs parts de vote, respectivement. Pour les nouveaux mouvements populistes, les médias sociaux représentent une opportunité de persuader les électeurs au-delà des médias traditionnels dominés par les partis établis. De même, les propriétaires de médias jonglent entre la maximisation des profits et un journalisme impartial et équitable. Dans le même temps, les grandes plateformes de médias sociaux naviguent sur une ligne similaire entre la modération du contenu, la vérification des faits et la maximisation des profits grâce aux interactions des utilisateurs.

Ces changements tectoniques dans les fondements économiques de l'industrie des médias ont des conséquences importantes pour la prise de décision des électeurs et la formation de leurs préférences, que cette thèse se propose d'étudier en quatre chapitres.

Le premier chapitre se concentre sur la consommation des médias et le populisme. Il étudie un ancien exemple naturel en Allemagne pour comprendre comment les mouvements populistes de droite exploitent les schémas de consommation des médias pour influencer les électeurs. Pendant la division de l'Allemagne, la télévision de l'Allemagne de l'Ouest était une source clé d'informations non censurées en Allemagne de l'Est socialiste (1949-1990) - si importante que les endroits sans accès à cette télévision sont devenus connus sous le nom de "vallée des ignorants" (*Tal der Ahnungslosen*). Je montre que, jusqu'à aujourd'hui, ces endroits

consomment moins de télévision et se tournent plutôt vers les médias sociaux pour prendre des décisions de vote. Cependant, ces différences ne sont devenues politiquement pertinentes qu'avec l'entrée du parti populiste de droite *Alternative pour l'Allemagne* (AfD) en 2013 : en comparant des municipalités proches et similaires, les endroits sans exposition historique à la télévision de l'Allemagne de l'Ouest ont robustement un pourcentage de vote de 1,7 à 2 points de pourcentage plus élevé pour l'AfD aujourd'hui, ce qui correspond à 12% de sa moyenne. En utilisant les données des utilisateurs de Facebook, je montre que la stratégie d'entrée de l'AfD avec une campagne dominante sur les médias sociaux porte doublement ses fruits : d'une part, les gens s'engagent davantage avec les récits populistes de l'AfD sur les médias sociaux. D'autre part, l'accès différencié aux médias non censurés dans le passé a un impact sur l'alphabétisation médiatique actuelle, comme le suggèrent les données d'enquête récentes de la pandémie de Covid. Cela peut expliquer la plus grande force persuasive du récit populiste de l'AfD.

Le deuxième chapitre se concentre sur le pluralisme des médias et la tension entre les propriétaires de médias, l'indépendance journalistique et le journalisme impartial. Les démocraties ont besoin d'électeurs informés - des électeurs exposés à une diversité de points de vue. Les médias jouent un rôle actif dans le processus d'information des électeurs ; cependant, ils varient dans leur couverture des partis politiques. Dans ce document, nous explorons si les différences de couverture politique sont principalement dues aux choix éditoriaux des propriétaires (quelques-uns), ou aux préférences des journalistes diversifiés, à condition qu'ils aient une certaine autonomie. Pour ce faire, nous construisons un nouvel ensemble de données sur des millions d'émissions de télévision et de radio françaises sur 20 ans, avec des informations sur l'identité des hôtes, des invités et des opinions politiques des invités. Nous estimons un modèle à deux effets fixes identifié grâce aux nombreux hôtes travaillant sur plusieurs chaînes. Nous montrons que les hôtes se conforment largement aux décisions des chaînes, ce qui explique 85% des différences inter-chaînes dans la représentation politique. Complétant ces résultats, nous étudions comment les hôtes se sont adaptés à un changement majeur dans la ligne éditoriale lié à la propriété et constatons que les hôtes qui sont restés après le changement ont respecté les préférences du nouveau propriétaire. Cette partie exploite un ensemble de données exhaustif et riche sur la production des émissions de télévision et de radio françaises de 2002 à 2020. En utilisant les modèles d'invitation des hommes politiques et des autres invités ayant des opinions politiques marquées, elle met en lumière l'évolution des dynamiques de pouvoir entre les propriétaires de médias axés sur la maximisation des profits et les journalistes négociant leur indépendance éditoriale.

Le troisième chapitre étudie l'interaction entre les préférences politiques des électeurs d'extrême

droite et leur attitude envers les dons de bienfaisance. Il documente tout d'abord un déclin généralisé de la part des donateurs aux organisations caritatives dans les pays occidentaux au cours de la dernière décennie, et montre que cela peut s'expliquer en partie par une plus faible propension à faire des dons chez les électeurs d'extrême droite. En se concentrant sur la France, nous menons d'abord une enquête à grande échelle ($N = 12\,600$) et montrons que les électeurs d'extrême droite sont significativement moins susceptibles de déclarer un don de bienfaisance que le reste de la population, en tenant compte d'un ensemble riche de variables de contrôle. Deuxièmement, en utilisant des données fiscales administratives pour l'ensemble des municipalités françaises ($N \approx 33\,000$) combinées aux résultats électoraux, nous constatons que la relation négative entre la part des votes pour l'extrême droite et les dons de bienfaisance se maintient dans un large éventail de spécifications, à la fois au niveau de l'extensivité et de l'intensité, et en contrôlant pour les effets fixes des municipalités. Troisièmement, nous exploitons des données uniques de dons géo-localisés provenant de plusieurs organisations caritatives et constatons des tendances similaires. Toutes les preuves indiquent une diminution de la propension à faire des dons due à un changement dans les normes sociales qui menace l'acceptation générale du secteur caritatif.

Le quatrième chapitre étudie comment les grandes plateformes de médias sociaux abordent la désinformation en coopération avec des journalistes spécialisés dans la vérification des faits. Plus précisément, il étudie les effets dynamiques de la vérification des faits sur le comportement de ceux qui diffusent la désinformation et sur la propagation de fausses informations. Pour répondre à cette question, nous avons établi un partenariat unique avec l'Agence France Presse (AFP), la plus grande organisation de vérification des faits au monde. Nous avons collecté pendant un an, lors des réunions éditoriales quotidiennes, les histoires proposées par les journalistes, dont certaines sont finalement vérifiées, tandis que d'autres, bien que "similaires", sont laissées de côté en raison d'un manque de ressources. En utilisant une approche de différences en différences, nous montrons que les publications sur Facebook liées aux histoires vérifiées sont moins partagées par rapport aux histoires qui ont été considérées mais finalement non vérifiées. Cet effet disparaît lorsque la notation est un avertissement (partiellement faux, manque de contexte) plutôt qu'un faux flagrant. De plus, en utilisant des variations au niveau des publications au sein des histoires vérifiées, et en exploitant le fait que les journalistes, en raison d'une contrainte de temps, ne notent pas toutes les publications associées à une histoire, nous constatons que la circulation des publications recevant une notation ambiguë augmente après la publication de la vérification des faits, ainsi que d'autres interactions (commentaires et mentions "J'aime") avec la publication. Cela suggère un effet boomerang alimenté par les réactions à la notation.

Chapitre 1 : « La Vallée des Ignorants ? » Consommation des Médias et Persuasion Populiste

Dans ce chapitre, j'étudie le rôle de la consommation des médias dans la montée du populisme. Face à l'essor des partis populistes de droite dans de nombreuses démocraties occidentales dans les années 2010, il est devenu crucial de comprendre le rôle de la consommation des médias par les électeurs pour expliquer comment les nouveaux concurrents populistes rivalisent pour les voix avec les partis établis. Plusieurs études ont abordé l'impact à court et moyen terme de la consommation des médias sur le populisme. Cependant, peu d'entre elles peuvent exploiter une variation exogène durable des modes de consommation des médias pour expliquer comment les populistes persuadent les électeurs grâce à leur alimentation médiatique.

Dans cet article, j'utilise un ancien exemple naturel en Allemagne pour aborder cette question. Pendant la période de division (1945-1990), la plupart des Allemands de l'Est avaient accès à la télévision de l'Allemagne de l'Ouest via une diffusion terrestre. La faisabilité technique et l'absence de barrières linguistiques permettaient aux Allemands de l'Est de s'y brancher facilement, et ils le faisaient massivement pour le divertissement et l'information non censurée. Cependant, environ 15% de l'Allemagne de l'Est étaient coupés de la télévision de l'Allemagne de l'Ouest, devenant ainsi la "Vallée des ignorants" dans la culture populaire est-allemande (*Tal der Ahnungslosen*). Comme son nom l'indique, la disponibilité locale exacte de la télévision était déterminée par des caractéristiques géographiques telles que l'altitude et les forêts, et variait entre les municipalités voisines, rendant impossible la réception de la télévision autour d'une coupure de signal. De plus, il y avait des municipalités dans diverses régions sans accès à la télévision de l'Allemagne de l'Ouest, et dans l'ensemble, les zones avec et sans accès à la télévision de l'Allemagne de l'Ouest étaient très similaires les unes aux autres. Lorsque l'Allemagne s'est réunifiée en 1990 sous les institutions de l'Allemagne de l'Ouest, la plupart des Allemands de l'Est avaient donc été exposés aux médias d'un système politique auquel ils allaient alors devenir inopinément et immédiatement partie prenante. Vingt-trois ans plus tard, depuis 2013, le premier parti populiste de droite réussi en Allemagne, l'Alternative pour l'Allemagne (AfD), célèbre ses plus grands succès électoraux en Allemagne de l'Est en général, et encore plus dans les zones sans accès historique à la télévision de l'Allemagne de l'Ouest.

En premier lieu, je montre que les différences dans les schémas de consommation des médias persistent jusqu'à aujourd'hui et continuent de façonner le type de médias que les électeurs consomment pour informer leurs décisions de

vote. Les électeurs des zones sans accès à la télévision de l'Allemagne de l'Ouest avant 1990 sont toujours moins susceptibles de regarder la télévision publique pour informer leurs décisions de vote. Au lieu de cela, ces individus se tournent plus souvent vers les médias sociaux pour s'informer sur la politique. Des différences dans la consommation des informations télévisées existent uniquement pour les deux diffuseurs publics qui étaient disponibles pendant la période de division, mais il n'y a pas de différences pour les chaînes de télévision privées qui sont entrées sur le marché juste avant ou après la réunification, ce qui indique que ces tendances sont directement liées à l'accès historique à la télévision de l'Allemagne de l'Ouest. La propension accrue à utiliser les médias sociaux est toujours perceptible aujourd'hui, par exemple en ce qui concerne les sources consultées par les individus concernant la pandémie de Covid.

En second lieu, je montre que le manque d'exposition historique à la télévision de l'Allemagne de l'Ouest est associé à une augmentation de la part des votes pour l'AfD depuis sa fondation en 2013. En termes d'identification et de méthodes, je m'améliore par rapport à la littérature précédente en exploitant des données de signal au niveau des municipalités fines, en contrôlant pour les effets fixes des États et la distance par rapport à l'ancienne frontière avec l'Allemagne de l'Ouest, comparant ainsi uniquement des municipalités proches avec une exposition historique différente à la télévision de l'Allemagne de l'Ouest. Les résultats sont extrêmement robustes sur un large éventail de spécifications, en particulier en utilisant une approche robuste RDD autour d'une coupure de réception du signal et en utilisant deux enquêtes individuelles différentes. Ils sont également robustes en contrôlant pour les technologies Internet ou IT signal qui pourraient utiliser des antennes similaires à celles de la télévision de l'Allemagne de l'Ouest et affecter le vote (Zhuravskaya et al., 2020; Falck et al., 2014; Gavazza et al., 2019). En utilisant de nouvelles données de Cantoni et al. (2019), je m'améliore encore par rapport à la littérature existante en montrant que les municipalités (plutôt que les districts) étaient en grande partie bien équilibrées avant le traitement et que le contrôle des différences avant le traitement n'affecte pas l'estimation.

Un design d'événements montre que cet effet est spécifique à l'AfD et ne s'est pas traduit par des différences de vote pour d'autres partis populistes de droite à l'ère pré-Internet. L'effet est également beaucoup plus prononcé après le virage populiste de l'AfD en 2015, lorsque le parti est passé d'une plateforme fiscaliste et eurosceptique à une plateforme anti-immigration, anti-establishment et anti-médias avec une rhétorique fortement populiste.

Ensuite, je fournis des preuves que cette rhétorique populiste est plus persuasive en raison de la présence plus forte de l'AfD en ligne. Pendant la période d'étude, l'AfD est le parti dominant sur les médias sociaux allemands. Par exemple, il a généré plus de partages sur

Facebook lors des élections de 2017 que *tous les autres partis combinés* (Stier et al., 2018). En utilisant les données de Müller and Schwarz (2021), je montre que l'AfD est plus présent dans les zones sans exposition historique à la télévision de l'Allemagne de l'Ouest. De plus, cette présence est plus impactante dans ces zones après son virage populiste, mais seulement pour les actions qui induisent une propagation de ses récits (publications) et non pour de simples expressions d'approbation (j'aime).

Ensuite, j'utilise des données d'enquête sur l'accord avec des déclarations conspirationnistes pendant la pandémie de Covid pour montrer que les répondants des zones sans exposition historique à la télévision de l'Allemagne de l'Ouest sont plus susceptibles d'être d'accord avec le fait que "les médias manipulent l'information", mais pas avec d'autres théories du complot. Alors que l'AfD pousse généralement des récits conspirationnistes, son récit (spécifique à l'Allemagne) sur la "presse mensongère", un terme de propagande nazie, semble particulièrement efficace dans ces zones qui n'avaient pas accès à la télévision de l'Allemagne de l'Ouest non censurée comme source d'information sous le régime est-allemand social.

Enfin, je me tourne vers une riche enquête auprès des ménages pour démêler davantage le mécanisme et étudier d'autres différences potentielles dans les attitudes. Malgré une grande taille d'échantillon et diverses variables couvrant un horizon temporel étendu, je ne parviens pas à détecter des différences persistantes dans les attitudes d'extrême droite, ce qui est en accord avec la constatation de l'étude d'événements selon laquelle l'effet est spécifique à l'AfD et à Internet, et non à l'extrémisme de droite en général ¹. En revanche, je trouve des preuves que les électeurs sont moins informés sur la politique, ce qui appuie l'idée qu'ils sont moins critiques dans leur consommation des médias. J'utilise également ma stratégie d'identification améliorée pour revoir les conclusions antérieures de la littérature. Entre autres, je constate que, bien que Kern and Hainmueller (2009) avaient raison concernant les effets positifs du bien-être de l'accès à la télévision (voir aussi Chadi and Hoffmann, 2021), cela ne s'est pas traduit par un soutien accru au régime est-allemand social tel qu'il est mesuré par la satisfaction à l'égard de la vie et de la démocratie sous le régime dans une enquête de 1990 avant la réunification.

Les résultats soulignent

l'importance de la consommation des médias pour expliquer le soutien accru aux partis populistes de droite. Les médias sociaux ont permis à ces partis de rivaliser avec les partis établis en utilisant un récit populistes adapté à leur public cible et en ciblant spécifiquement

¹Il convient de noter que cela est en contradiction avec Hornuf et al. (2023), probablement en raison de l'attribution de traitement moins précise et de l'échantillon spécifique et des questions sélectives utilisées. Je reviendrai sur ce point ci-dessous.

les zones où les gens consomment principalement les médias sociaux pour s'informer sur la politique. Alors que la consommation des médias est le plus souvent considérée comme un problème de bulle informationnelle et de préférences politiques figées, les résultats montrent comment la consommation des médias peut être influencée par le passé et comment cela peut affecter les résultats des élections.

Chapitre 2 : "Le Bias s'invite : La Politique Éditoriale de la Radio et de la Télévision Française"

Ce chapitre explore les déterminants des choix éditoriaux dans les médias d'information, en se concentrant sur la représentation politique dans les programmes de télévision et de radio. Les démocraties dépendent de médias indépendants pour informer les électeurs et assurer un débat public diversifié, mais les médias varient dans leur couverture des partis politiques. Les différences dans la représentation politique peuvent être dues aux choix éditoriaux des propriétaires, ou aux préférences des journalistes, à condition qu'ils aient une certaine autonomie. Je tente de répondre à cette question en construisant un nouvel ensemble de données sur les émissions de télévision et de radio françaises sur une période de 20 ans, couvrant toutes les chaînes de télévision et de radio et incluant des informations sur l'identité des hôtes, des invités et des opinions politiques des invités.

Je montre que la représentation politique dans les programmes d'information dépend principalement des choix éditoriaux des propriétaires plutôt que des préférences des journalistes. Pour ce faire, j'exploite la variation dans les hôtes qui travaillent sur plusieurs chaînes de télévision ou de radio. En supposant que les préférences politiques des hôtes restent constantes au fil du temps, je peux identifier l'effet des chaînes de télévision ou de radio sur la représentation politique en observant comment la représentation politique change lorsque les hôtes passent d'une chaîne à l'autre. Les résultats montrent que les hôtes se conforment largement aux décisions des chaînes, ce qui explique 85% des différences inter-chaînes dans la représentation politique. Les hôtes qui travaillent sur différentes chaînes représentent les partis politiques de manière très similaire, et cela est vrai même lorsque les chaînes appartiennent à des propriétaires différents. En outre, je montre que les hôtes se sont adaptés à un changement majeur dans la ligne éditoriale lié à la propriété et ont respecté les préférences du nouveau propriétaire.

Enfin, je me tourne vers une analyse de la représentation des partis populistes dans les médias d'information français. Les partis populistes sont souvent accusés de bénéficier d'une couverture médiatique favorable et de recevoir une attention disproportionnée dans les médias.

J'étudie si cela est vrai pour le Rassemblement National (RN), le principal parti populiste de droite en France. Je constate que le RN reçoit effectivement une attention disproportionnée dans les médias par rapport à son soutien électoral. Cependant, cette attention accrue est principalement due aux choix éditoriaux des propriétaires de médias plutôt qu'à un biais des journalistes. Les hôtes qui travaillent sur différentes chaînes représentent le RN de manière similaire, et la représentation du RN est corrélée avec le soutien du propriétaire de la chaîne plutôt qu'avec les préférences des hôtes.

Ces résultats mettent en lumière les dynamiques de pouvoir entre les propriétaires de médias axés sur la maximisation des profits et les journalistes négociant leur indépendance éditoriale. Alors que les journalistes jouent un rôle crucial dans la production d'informations, ils sont souvent soumis aux contraintes des propriétaires et sont contraints de se conformer aux décisions éditoriales. Cela peut avoir des conséquences importantes pour la diversité et la qualité du débat public dans les démocraties.

Chapitre 3 : "Les Électeurs d'Extrême Droite Font-Ils Moins de Dons aux Associations ? Le Cas de la France"

Dans ce chapitre, j'étudie l'interaction entre les préférences politiques des électeurs d'extrême droite et leur attitude envers les dons de bienfaisance. Les démocraties ont besoin d'électeurs informés - des électeurs exposés à une diversité de points de vue. Les médias jouent un rôle actif dans le processus d'information des électeurs ; cependant, ils varient dans leur couverture des partis politiques. Dans ce document, nous explorons si les différences de couverture politique sont principalement dues aux choix éditoriaux des propriétaires (quelques-uns), ou aux préférences des journalistes diversifiés, à condition qu'ils aient une certaine autonomie. Pour ce faire, nous construisons un nouvel ensemble de données sur des millions d'émissions de télévision et de radio françaises sur 20 ans, avec des informations sur l'identité des hôtes, des invités et des opinions politiques des invités. Nous estimons un modèle à deux effets fixes identifié grâce aux nombreux hôtes travaillant sur plusieurs chaînes.

Nous montrons que les hôtes se conforment largement aux décisions des chaînes, ce qui explique 85% des différences inter-chaînes dans la représentation politique. Complétant ces résultats, nous étudions comment les hôtes se sont adaptés à un changement majeur dans la ligne éditoriale lié à la propriété et constatons que les hôtes qui sont restés après le changement ont respecté les préférences du nouveau propriétaire.

Ensuite, nous explorons la relation entre les préférences politiques des électeurs et leur

propension à faire des dons de bienfaisance. Nous utilisons d’abord une enquête à grande échelle ($N = 12\,600$) pour montrer que les électeurs d’extrême droite sont significativement moins susceptibles de déclarer un don de bienfaisance

que le reste de la population. Ensuite, nous utilisons des données administratives sur les dons de bienfaisance pour toutes les municipalités françaises ($N \approx 33\,000$) combinées aux résultats électoraux et montrons que la relation négative entre la part des votes pour l’extrême droite et les dons de bienfaisance persiste dans un large éventail de spécifications, en contrôlant pour les effets fixes des municipalités. Enfin, nous exploitons des données uniques de dons géolocalisés provenant de plusieurs organisations caritatives et constatons des tendances similaires.

Enfin, nous examinons si le lien entre les préférences politiques et les dons de bienfaisance est dû à des différences dans la composition socio-économique des électeurs d’extrême droite. En contrôlant pour un ensemble riche de variables de contrôle, nous montrons que la relation persiste. En outre, nous exploitons une manipulation dans la perception des avantages fiscaux des dons de bienfaisance et constatons que cela n’affecte pas les dons des électeurs d’extrême droite, ce qui suggère que le comportement est enraciné dans des préférences profondes plutôt que dans des facteurs financiers.

En somme, les résultats de cette étude mettent en lumière un écart dans le comportement de don entre les électeurs d’extrême droite et le reste de la population. Cette recherche suggère que les différences dans les normes sociales et les valeurs politiques jouent un rôle important dans la décision de faire un don de bienfaisance, et que cela peut être lié à une moindre propension des électeurs d’extrême droite à soutenir des causes caritatives.

Le fact-checking est-il efficace pour réduire la propagation de la désinformation ? Comment cela affecte-t-il le comportement des utilisateurs ou des politiciens qui diffusent de fausses informations ? Bien que l’industrie du fact-checking ait connu une croissance ces dernières années en raison des préoccupations mondiales concernant les fausses informations (Allcott and Gentzkow, 2017; Allcott et al., 2019), l’impact du fact-checking fait toujours l’objet d’un examen intense. La littérature fournit de solides preuves que, bien que le fact-checking ne parvienne pas à corriger les croyances ou les intentions de vote, il est efficace pour réduire la diffusion (Pennycook et al., 2020a,b; Henry et al., 2022). Cependant, la plupart des études dans la littérature utilisent des expériences contrôlées en laboratoire ou sur le terrain, qui ne permettent pas de documenter les effets dynamiques sur le comportement des participants.

Dans ce document en cours, pour répondre à ces questions, nous nous appuyons sur un partenariat unique avec l’Agence France Presse (AFP), la troisième plus grande agence de

presse au monde et la plus grande organisation de fact-checking au monde. Un journaliste a été engagé pendant 18 mois pour assister aux réunions éditoriales quotidiennes de “*AFP Factuel*”, l’unité de l’AFP chargée de vérifier les informations en français. Il a recueilli des informations sur toutes les histoires qui ont été discutées lors des réunions quotidiennes, celles qui ont été approuvées et vérifiées, ainsi que celles qui ont été laissées de côté. Il a également enregistré les raisons des rejets (manque de ressources, manque de viralité, etc.) lors de réunions régulières avec les rédacteurs en chef de *AFP Factuel*.

L’AFP fait partie du programme de fact-checking de tiers mis en place par Facebook.² Cela donne aux journalistes un accès direct à l’outil Facebook où ils peuvent noter les publications directement une fois qu’un fact-check est produit. Cela donne également accès au soi-disant “Facebook claim”, qui contient une liste de publications suspectes détectées automatiquement par Facebook à l’aide d’algorithmes. Importamment, l’accord avec Facebook ne donne pas d’incitations pour noter systématiquement toutes les publications liées à la même désinformation vérifiée. Pour chacune des histoires, qu’elles aient été fact-checkées ou non, le journaliste que nous avons engagé a également recueilli des informations sur les publications associées notées et non notées par les journalistes de “*AFP Factuel*”.

Cet effort unique de collecte de données nous permet de construire une stratégie d’identification originale pour identifier l’effet causal du fact-checking sur la circulation de la désinformation. Nous utilisons deux approches, l’une au niveau des histoires et l’autre au niveau des publications (en contrôlant les effets fixes des histoires). Ces deux approches distinctes abordent différentes questions. Au niveau des histoires, nous utilisons une approche de différences en différences (DiD), en comparant les histoires qui ont été fact-checkées aux histoires “similaires” qui ont été initialement envisagées mais laissées de côté, en particulier en raison d’un manque de ressources. Notre variable de résultat préférée est le logarithme de la somme des publications liées aux histoires sur Facebook. L’hypothèse clé d’identification est que les deux types d’histoires auraient eu des trajectoires similaires en termes de circulation en l’absence d’intervention de fact-checking. Pour garantir la validité de cette hypothèse d’identification, nous imposons deux restrictions sur les données en exploitant les détails du processus éditorial. Premièrement, nous excluons les histoires qui n’ont pas été fact-checkées en raison du manque de viralité.³ Deuxièmement, nous excluons les histoires qui ont été fact-checkées même si le journaliste proposant l’histoire travaillait déjà sur une autre véri-

²Il y a 123 organisations accréditées dans le monde entier, par exemple Reuters et *The Washington Post* aux États-Unis, ou *Le Monde* et *Libération* en France.

³Il y a 5 raisons principales pour ne pas vérifier une histoire : (i) un manque de ressources, (ii) un manque de viralité de l’histoire, (iii) le fait que l’histoire est probablement vraie, (iv) le fait que la vérification des faits serait irréalisable, et (v) le fait que l’histoire a déjà été vérifiée.

fication des faits à ce moment-là, car dans ce cas, le seuil pour accepter l'histoire est plus élevé.⁴ Importamment, en accord avec notre hypothèse d'identification, nous montrons que les histoires fact-checkées et non fact-checkées faisaient face à des tendances de popularité similaires avant d'être envisagées pour la première fois par l'AFP.

La deuxième stratégie d'identification utilise uniquement les histoires fact-checkées et, pour chaque histoire, compare les publications qui ont été notées à celles qui ne l'ont pas été. Comme expliqué ci-dessus, l'accord avec Facebook n'incite pas à noter systématiquement toutes les publications. La notation de publications supplémentaires liées à l'histoire dépend de la volonté du journaliste qui a vérifié les faits. En travaillant en collaboration avec l'équipe d'AFP Factual, nous avons pu comprendre que les journalistes notent autant de publications qu'ils le peuvent, mais souvent pas toutes en raison du manque de temps. Notre hypothèse d'identification ici – basée sur de nombreuses discussions avec le personnel de l'AFP – est que la dernière publication notée est "similaire" à la première qui ne l'est pas, c'est-à-dire que le vérificateur de faits décide

d'arrêter de noter les publications à un moment donné pour des raisons aléatoires. En accord avec cette hypothèse, nous montrons que les publications qui sont notées et celles qui ne le sont pas suivent des tendances parallèles en termes de nombre de partages sur Facebook avant que l'équipe d'AFP Factual ne considère l'histoire pour la première fois.

Nos résultats préliminaires montrent que le fact-checking réduit la circulation des publications liées aux histoires vérifiées. La stratégie d'identification au niveau des histoires montre que cette réduction est significative, tant d'un point de vue statistique qu'économique. Nos résultats reflètent la combinaison d'un effet de contrôle par Facebook réduisant la circulation et d'une réponse comportementale des utilisateurs. Au niveau des histoires, un fact-check réduit la circulation des publications associées de 26 à 30

En plus des preuves descriptives que nous recueillons, nous identifions plusieurs marges pertinentes sur le plan politique pour améliorer l'efficacité du fact-checking. Premièrement, nous soutenons que, étant donné que la rapidité est importante, les vérificateurs de faits devraient être équipés de meilleurs outils pour détecter la désinformation, car accélérer le processus d'écriture lui-même peut entraîner un compromis en termes de qualité de la vérification des faits. Deuxièmement, nous constatons que – en l'absence d'outils de travail adéquats – la méthode actuellement dominante pour détecter la désinformation repose fortement sur l'examen de sous-communautés sur Facebook, ce qui conduit à des efforts de surveillance

⁴En effet, les journalistes travaillent généralement sur une seule vérification des faits à un moment donné. Notez cependant que nous montrons que nos principaux résultats sont robustes même en l'absence de ces deux restrictions.

imparfaits et dépendants du chemin suivi. Troisièmement, nous constatons que, comme discuté, en raison des incitations mal alignées, les publications partageant des informations identiques ne sont pas signalées. Améliorer ces trois points semble être des mesures évidentes, qui nécessiteraient peu de ressources supplémentaires et déchargerait considérablement les vérificateurs de faits.

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