On the Value of Information in Political Economy

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Off to new horizons.

— Timo, March 2025.

Shine bright when the lights down.

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Preface

In this dissertation, I examine the role and value of information in Political Economy. Four common themes recur throughout the individual chapters:

(i) Information can be extremely valuable.

(ii) The dissemination of information is influenced by strategic incentives.

(iii) Agents respond heterogeneously to new sets of information.

(iv) Decision-making depends on agents' access to information and their belief-updating.

I explore these themes in four self-contained chapters.

Chapter 1 investigates the extent to which government advertising influences media content, and how media content influences citizens' political preferences. I provide extensive evidence of media capture in a Western democracy, focusing on Austria, where ministers have wide discretion in the allocation of media funding. I show that ministerial payments to newspapers and coverage of ministers are positively correlated; a relationship that is stronger when newspapers are more reliant on public funds and reverses when politicians face political scandals. Government advertising also crowds-in positive reporting. Relying on a shift-share instrumental variable approach for identification, I demonstrate that advertisements causally increase coverage even within newspaper-minister pairs; ruling out that ideological proximity between newspapers and politicians drives the results. I combine the analysis of how advertising influences media content with an approach that estimates the effect of media content on citizens' voting intentions. Exploiting high-frequency panel data on households' media exposure, I derive estimates for the 'advertisement cost of a vote'. The results show that media content has a significant, but polarizing, influence on citizens' voting intentions and that government advertising contributes to this effect.

This chapter improves our understanding of how media, as the most important platform for distributing information, can be (mis-)used to achieve strategic political

incentives. The findings demonstrate that information distributed via media channels can be very persuasive and can significantly influence citizens' perceptions and beliefs. This underlines the value of information *per se*, but also that it matters who has access to widely disseminate information.

Chapter 2, which is based on co-authored work with Alastair Langtry, Niklas Potrafke, and Marcel Schlepper, studies the role of gambling-style behavior in political decision-making. Specifically, we show that politicians 'gamble for re-election' in the context of a political leader selection. At the heart of MPs' decision-making problem is an inherent information asymmetry: they have to make decisions about which leader to support, while facing *ex ante* uncertainty about the true quality of the leader. Constituents, in contrast, have the full set of information when making their voting decisions. We model MPs' behavior in a theoretical framework of rational risk-taking. Our key prediction is that MPs potentially trade off a leader's expected quality against their riskiness. We test our predictions with exclusive access to unique data: leaked information on MPs' individual decisions in a *de facto* vote for the 2021 leadership election of Germany's centre-right parties. This allows us to overcome empirical challenges arising from secret ballots. Our main finding is that MPs are more likely to vote for a riskier candidate when faced with lower re-election chances. Gambling for re-election provides a new explanation for intra-party dissent and rationalizes why parties may choose low quality leaders when better ones are available.

This chapter shows how information asymmetries can influence economic and political decision-making. When risk-neutral agents do not bear all of the costs of a bad outcome, their incentives can sometimes drive them to act as if they are risk-seeking. Uncertainty about the 'true state of the world' can exacerbate incentives to 'gamble'. In the realm of political decision-making, this has important implications for the selection of political leaders, wider policy-making, and ultimately for voters' welfare.

Chapter 3, which is based on co-authored work with Klaus Gründler, Michael Lamla, and Niklas Potrafke, explores differences in information processing across agents. The consequences of economic policy are often difficult to understand for households and firms, but they have important macroeconomic implications. This chapter investigates whether and when economic experts can act as intermediaries to effectively explain economic policies to the general public. As a laboratory, we leverage the monetary policy shock induced by the sharp interest rate increases to tackle the 2022-23 inflation surge. We first design three large-scale surveys among influential economic experts in 21 European countries, Canada, and the United States to study how economic experts update their expectations and beliefs in response to monetary policy

shocks. Our findings demonstrate that experts react swiftly not only to changes in interest rates but also to the more nuanced tone of monetary policy announcements. Given the strong and immediate updating of experts, we design an information provision experiment to explore the extent to which experts can serve as intermediaries for households. We find that both experts' inflation expectations and their macroeconomic explanations ('narratives') impact macroeconomic expectations of households. We also show that experts help reduce forecast errors of other economic agents and that firms and households fail to update their macroeconomic expectations in response to shocks without expert explanations. Our results suggest that experts can lead households to effectively break through the 'veil of inattention' of monetary policy, help central banks to achieve anchored expectations and, more generally, can serve as important intermediaries for economic policies.

This chapter demonstrates that different agents respond heterogeneously to new sets of information ('information shocks'). Reasons for this include varying levels of attention and differing abilities to correctly process new sets of information. A lack of ability to correctly decode new information can therefore lead to systematic errors in agents' expectations and decision-making. This chapter contributes to our understanding of how to decrease such errors.

Chapter 4, which is based on co-authored work with Sebastian Hager and Marcel Schlepper, tells the story of a largely forgotten – but very important – Allied program at the end of the Second World War. After the defeat of Germany, the United Kingdom and the United States engaged in a large-scale program of 'intellectual reparations'. British and U.S. experts investigated firms and production plants across Germany to collect technological expertise that could benefit their private industries. We provide the first systematic and quantitative analysis of this unprecedented program of intellectual reparations. We present a newly assembled dataset on the universe of the investigations, which allows us to map the extent of the resulting knowledge transfers. We provide facts on the German firms, industries, and technologies that were investigated. Moreover, we link the investigated German firms to the investigating U.S. firms to directly observe knowledge transfers.

The findings in this chapter substantiate the value of information. They also speak to the economic returns of espionage and the drivers of technology diffusion. More generally, this chapter contributes to our understanding of how access to valuable information (e.g., intellectual property and technology) shapes economic development.

Chapter 1

Media Capture and Voters: Evidence from Austria

This chapter is based on single-authored work (see Wochner, 2025). ChatGPT (model GPT-4o) was used as a supporting tool to improve the linguistic quality of this chapter.

"Do you know of any quid pro quo [for government advertisements]?"
— *"I really hope there was something in return, which is coverage and an ad, because that's the price you pay."*

— Sebastian Kurz, Former Austrian Chancellor, *7 October 2021*

1.1 Introduction

Mass media play a crucial role in disseminating information to broad audiences. Corporations and the public sector strongly rely on mass media to reach target audiences and deliver public information. In a political context, independent media are essential for ensuring the accountability of politicians by providing voters with accurate information. While printed newspapers still hold a significant share of the media market, the rise of online and social media has led to a decline in readership and advertising revenue in the newspaper industry (see e.g., Angelucci and Cagé, 2019; Bhuller et al., 2024; Djourelova et al., 2024). This development has made newspapers increasingly dependent on large advertisers and creditors, including political institutions in many countries. Paradoxically, public funding schemes intended to support media diversity and bolster the resilience of the media market may undermine media reliability. Politicians might try to exercise their market power to capture editorial content. Such practices can have far-reaching consequences: they could distort public opinion and influence electoral outcomes – ultimately threatening a key pillar of democracy.

Prior research has established a link between government advertisements and corruption coverage in newspapers in Argentina, Hungary, and China (Di Tella and Franceschelli, 2011; Szeidl and Szucs, 2021; Zhuang, 2022), and has demonstrated that reliance on advertisers and creditors matters for the independence of media markets (Petrova, 2011; Durante et al., 2020). There is, however, a distinct lack of evidence of media capture in Western democracies, where media markets and the institutional features of media financing schemes often differ from those in previously studied countries. A growing body of literature also documents that media content influences voting behavior. Yet, this issue has been studied exclusively in isolation from the potential impact of public media capture; illustrated by Szeidl and Szucs (2021) call that future research should '*combine an approach [...] that estimates the impact of government advertising on content with an approach that estimates the impact of content on votes*'.

In this paper, I examine the *joint* interplay between government advertising, newspaper coverage, and citizens' voting intentions. Using data from Austria, I provide the first extensive and causal evidence on how government advertisements in a Western

democracy influence media coverage. I then study the downstream effects on voters, providing causal evidence on the effectiveness of public media capture on citizens' voting intentions. To the best of my knowledge, this is the first study to use a single empirical framework to examine (i) how public media capture influences editorial content and (ii) how media content influences citizens' beliefs.

Austria is uniquely suited for my research design due to its distinctive media market characteristics. Although the country has a consistently high ranking on democracy indices, its media market possesses institutional features that make it particularly vulnerable to potential manipulation. Austria's media financing scheme relies heavily on direct funding, such as advertisements or grants. Media funding is allocated at ministry level, with no central agency coordinating payments. Crucially, the allocation of funding is not strictly tied to equity or need, but involves considerable discretion on the part of individual ministers. The European Commission has raised concerns about this practice, highlighting that the lack of regulation surrounding state advertising contracts in Austria may facilitate political influence over the media (European Commission, 2020). Additionally, Austria has one of the highest concentrations of media ownership in Europe (Grünangerl et al., 2021) and faced numerous high-profile scandals over recent years.[1] As a result, there were six different governments in power between 2012 and 2021, which may have increased incentives among politicians to manipulate media coverage.

Following a policy reform in 2012, payments from official sources to media outlets have to be reported, which enables detailed tracking of payment flows from ministries to the press. I collect two datasets providing comprehensive information on these payment flows. The first dataset comprises administrative data on payment flows at the quarterly level from 2012 to 2021. The second dataset provides more granular data, capturing payment flows on a daily basis between 2020 and 2021. I combine the data on payment flows with the universe of over six million newspaper articles from five leading Austrian newspapers and high-frequency household panel data.

The data reveals a striking concentration of public sector spending on the print segment. Until 2016, more than 90 percent of the public sector's total media budget was allocated to the print segment. Despite this share having decreased to around 70 percent by the end of my sample period in 2021, expenditure on the print segment still represents the predominant share of the ministries' overall advertising expenditure. In contrast, the private sector allocates less than half of its total advertising budget to

[1]Some of these scandals were directly linked to media financing practices. Since 2012, two Chancellors (Sebastian Kurz and Werner Faymann) have been investigated by parliamentary committees for misconduct related to media financing.

print media (45 percent in 2013 and 28 percent in 2021). As a result, notwithstanding the declining market reach of print media, the share of public funds contributing to print outlets' annual revenues has steadily increased, with a notable spike following the onset of the COVID-19 pandemic.

Payments from the Austrian ministries for advertisements are economically important for newspapers. In some cases, they represent up to one third of newspapers' annual revenues and can even exceed their profits. A multitude of newspapers would be unlikely to survive without public financial support through public advertisements (Kaltenbrunner, 2021). The scale of public advertising is considerable: Austrian ministries spend more than ten times the amount per capita on newspaper advertisements compared to the government in Germany (Wetz, 2021).

I document five main results. The first main result is that politicians exercise discretion when allocating media funds. Payment flows from ministries to newspapers differ substantially depending on the ministers' political affiliation.

The second main result is that politicians successfully capture the media content of newspapers. In my empirical baseline specification, I regress the coverage that an individual minister receives in a particular newspaper on the payment flows from that ministry to the same newspaper, controlling for a full set of fixed effects. I document that advertisement payments are positively correlated with the newspaper coverage of ministers.[2] This pattern applies even within ministry- or minister-newspaper pairs; thus ruling out the notion that ideological proximity between ministers and newspapers drives the results. A one standard deviation increase in advertising (75,000 Euros) by a ministry is associated with seven more articles about the corresponding minister in a quarter (ten percent of a standard deviation). The same pattern applies at the daily level: If a ministry publishes an ad in a newspaper on a given day, the likelihood of an article about that minister appearing in the same newspaper on that day increases on average by more than five percentage points. This suggests that the favor exchange operates at a high frequency. In addition, timing specifications show that only contemporaneous advertisement payments influence coverage, with no evidence of dynamic effects. The increase in coverage in response to advertisements is more pronounced when newspapers are more reliant on public advertisements. In line with the highly suggestive evidence for two-way favors, I provide evidence that advertisements by ministries induce 'excessive coverage' – coverage unique to newspapers that received advertisements. Furthermore, the results are not driven by underlying events, such as

[2]In contrast to previous studies that narrowly focused on corruption coverage, I consider media coverage more broadly and demonstrate that the previously found negative relationship between advertising and coverage on corruption is a special case.

shocks to individual politicians or newspapers (e.g., demand shocks), but persist with ministry-quarter and newspaper-quarter fixed effects. The results are also robust to using alternative coverage measures, controlling for potential time-varying confounders, and more restrictive specifications.

The inclusion of ministry-newspaper (or minister-newspaper) interaction fixed effects helps alleviate concerns regarding the potential simultaneity between advertisement payments and media coverage. To fully address endogeneity concerns, I employ a shift-share instrumental variable design for causal identification. Leveraging exogenous temporal variation in ministers' access to discretionary funds and cross-sectional variation in the allocation of those funds, I predict advertisement flows in a first stage. Using the predicted advertisements in the second stage, I provide causal evidence that advertisements by ministries lead to an increase in coverage of the respective ministers.

I next employ natural language processing techniques to trace more nuanced effects on soft factors of newspaper reporting. The third main result is that advertisements by ministries also shape the tone and thematic focus of reporting. Using a large-scale online learning Latent Dirichlet Allocation (LDA) model, I demonstrate that increased coverage of individual ministers extends to a broader focus on their ministries' specific policy areas. More importantly, I find that advertising leads to a noticeable shift in the tone of reporting: Articles about a minister become more positive as advertising volumes increase. This shows that advertisements by ministries crowd in *positive* coverage about the corresponding minister. Moreover, I provide evidence that politicians engage in 'negative campaigning'. I find *positive* spillover effects on coverage about ministers of the opposing party. The increased focus on opposition ministers is marked by a deterioration in the sentiment of their media coverage. Higher advertisement volumes therefore crowd in *negative* reporting about ministers from the opposing party.

The fourth main result is that the effects are most likely driven by an advertiser effect, and are at odds with other alternative mechanisms (such as demand-driven bias, complementarities in content, or other market-based explanations). I establish this result based on two empirical findings. First, the effect of advertisements on coverage reverses when politicians face a political scandal. In 'good times', more advertisements lead to more coverage, whereas in 'bad times', more advertisements lead to less coverage. Second, I exploit the 2012 reform, which imposed heavy restrictions on the permitted content of advertisements by ministries. Prior to the reform, advertisements by ministries could effectively be used for direct campaigning. Direct incentives for two-way favors thus arise only after the reform. This is reflected in the data: I pro-

vide suggestive evidence that advertisement increases coverage only after the reform. The *asymmetric* response to advertisement, and the incentive-consistent behavior after the reform supports the notion that market-based explanations cannot account for the results.

Media bias is an equilibrium concept, which makes it challenging to establish causal estimates on its impact using observational data. To address this, I combine the data on media content with high-frequency household-level panel data on media consumption and political attitudes. This makes it possible to assess whether government advertising — through influencing media content — shapes citizens' voting intentions. My empirical strategy allows to hold individual demand for media content constant while leveraging exogenous variation in the supply component, driven by fluctuations in government advertising. By using payment flows from ministries to newspapers as an instrumental variable for media content, I derive causal estimates of the effectiveness of media capture.

The fifth main result is that media content has a sizable effect on individuals' voting intentions and that the government's media financing contributes to this effect. The results show that a one standard deviation increase in weekly exposure to coverage of a party (91 articles) increases the likelihood of voting for that party by 0.55 points on a one to ten scale (15 percent of a standard deviation). A back of the envelope calculation makes it possible to estimate an 'advertisement price of vote'. A party would need to spend 1.35 million Euros to achieve that treatment effect. However, this average effect conceals an important non-linearity. Increased media coverage of a party strengthens voting intentions among individuals with high baseline support for that party, but it *decreases* the likelihood of voting for the party among those with low baseline support. Media content can therefore create a polarizing effect: It consolidates support among core supporters, while triggering a backlash effect among voters who are politically opposed.

Contribution to the literature. The main contribution of this paper is to provide comprehensive evidence on both the incidence *and* consequences of public media capture in a Western democracy. As such, I mainly contribute to two strands of the literature.

The first strand of literature relates to the political economy of the media, with a focus on how media financing (by governments) influences media content. The seminal work by Di Tella and Franceschelli (2011) shows that corruption scandal coverage in Argentinian newspapers was negatively correlated with government advertising pay-

ments. Building on this first evidence, it has also been shown that the Hungarian government gives advertising favors to politically connected newspapers, and that these newspapers reciprocate by favoring the government in their corruption coverage. Using a structural model and exploiting ownership changes, both shared ideology and favor exchange were found to be key drivers for this pattern (Szeidl and Szucs, 2021).[3] Local Chinese newspapers have also been shown to underreport on corruption investigations of well-connected officials. Underreporting is less severe among newspapers that rely more on private advertising revenue, which reduces their dependence on local governments (Zhuang, 2022). A similar pattern of media dependence on advertising revenue was also observed for historical U.S. newspapers (Petrova, 2011).

There is a distinct lack of evidence of media capture in Western democracies. This paper addresses this gap by examining favor exchanges between media outlets and politicians in Austria. Previous studies have focused exclusively on payments from the general government to newspapers and on corruption-related coverage. I am the first to examine the relationship between payments and coverage at the individual minister level, and, using the full range of newspaper articles, consider coverage more broadly — going beyond corruption-specific coverage. I demonstrate that the previously identified negative relationship between advertisements and corruption coverage is a special case that occurs in 'bad times', such as when politicians face political scandals. I find asymmetric responses to advertisements depending on the state of the world; this provides evidence for direct advertiser effects and rules out alternative market-based explanations.

The second strand of literature that I contribute to examines how media content influences voters' attitudes and behavior. Growing evidence shows that media bias and political candidates' exposure to the media can impact electoral outcomes (Druckman and Parkin, 2005; DellaVigna and Kaplan, 2007; Gerber et al., 2009; Enikolopov et al., 2011; Martin and Yurukoglu, 2017; Mastrorocco and Minale, 2018; Prat, 2018; Durante et al., 2019; Caprini, 2023).[4] These studies, however, study the impact of media bias in isolation from media capture. Besley and Prat (2006) provide a *theoretical*

[3]Similar patterns of (one-way) favor exchanges have been observed for commercial advertisers. For example, advertisement expenditure by Italian companies was positively correlated with their newspaper coverage (Gambaro and Puglisi, 2015). Similarly, car manufacturers' advertising in U.S. newspapers has reduced coverage on car safety recalls (Beattie et al., 2021) and led to a more skeptical tone in climate change coverage (Beattie, 2020) – a pattern of favor exchange also found in financial media (Reuter and Zitzewitz, 2006). Media outlets also bias their news coverage in favor of the banks they borrow from (Durante et al., 2020), providing further evidence that dependency on creditors can influence reporting.

[4]A smaller part of this literature also examines the effects of advertisements (Larreguy et al., 2018; Spenkuch and Toniatti, 2018; Sides et al., 2022; Esteban-Casanelles, 2024; Hewitt et al., 2024). These studies focus specifically on electoral advertisements, which are designed to persuade voters. In

framework for how governments can influence political outcomes through endogenous media capture. I am the first to combine these two approaches in a joint empirical design. I examine the effectiveness of media capture on votes by deriving an estimate for the 'advertisement price of a vote'. This approach complements previous studies on the impact of campaign spending on votes (e.g., Bekkouche et al., 2022) and contributes to the broader understanding of how media bias can provoke shifts in public opinion.

1.2 Institutional Background

This section provides an overview of the Austrian media landscape and its institutional features. It also offers background information on Austria's political landscape, the country's media financing scheme, and the use of advertisements by ministries.

1.2.1 Political Landscape

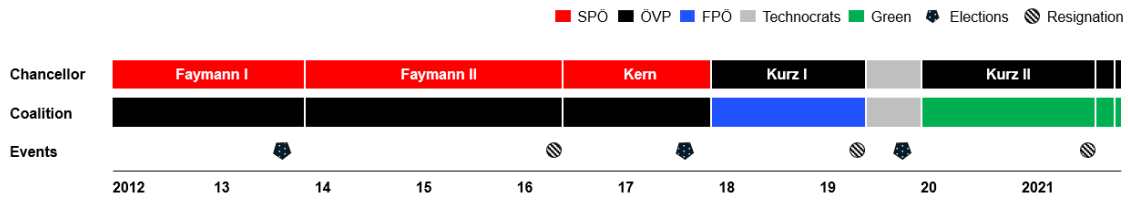
Austria's political landscape is volatile. Owing to a range of political scandals, a total of eight coalition governments were in power over the sample period from 2012 to 2021.[5] Four parties were part of a government coalition. Two traditionally strong parties have governed Austria for most of the post-war years in a grand coalition. One is the center-right Austrian People's Party (ÖVP), which has a conservative policy platform. The other is the center-left Social Democratic Party (SPÖ), which accords the government a key role in resolving social issues and improving living standards. The ÖVP and SPÖ governed Austria between 2012 and 2017 under the Chancellors of Faymann and Kern (both SPÖ). Two smaller parties have also been part of governing coalitions. The populist Freedom Party of Austria (FPÖ) operates on a euroskeptic, anti-immigrant, and anti-Muslim platform. The FPÖ came into power in 2017 when the ÖVP, with Chancellor Sebastian Kurz, won the national elections and selected the FPÖ as their coalition partner. Following a political scandal of FPÖ's leadership in May 2019, the parliament dissolved to clear the way for new elections. A technocratic government was in power for this interim period. Sebastian Kurz returned as Chancellor at the start of 2020, partnering with the Green Party. The Greens are an environmentalist party, joining the government for the first time. In October 2021, Kurz was investigated for alleged criminal behavior and resigned. The coalition of the ÖVP and the Green

contrast, I demonstrate that general political advertisements – those not tied to campaigning – can also be used to capture media content.

[5]For my analysis, I do not count the interim government led by former Vice-Chancellor Löger that lasted only a couple of days. I also disregard the technocratic government without party affiliation.

Party continued and was at first led being led by Schallenberg and then by Nehammer. Figure 1.1 summarizes the events and governments between 2012 and 2022.

Figure 1.1: FEDERAL GOVERNMENT COALITIONS SINCE 2012



Notes: The technocratic government was led by Brigitte Bierlein. From October to December 2021, Alexander Schallenberg was Chancellor. His successor Karl Nehammer was elected in December 2021. Both are members of the ÖVP.

1.2.2 Media Landscape

In recent years, media freedom in Austria has steadily declined. The country ranked 31st on the 2022 Press Freedom Index, a dramatic drop from its fifth place position in 2011. Several factors can account for this development.

There are no regulatory safeguards to prevent political interference in the appointment and dismissal of editors-in-chief. Therefore, appointments and dismissals of editors-in-chief are often driven by the commercial interests of media organizations (Seethaler and Beaufort, 2021). Editorial independence is further undermined by the fact that owners of media outlets frequently serve as editors-in-chief or managing directors. This structure enables political actors and wealthy private individuals to exert considerable influence over editorial content. More than 70 percent of Austrian journalists report experiencing advertising pressure, and more than 80 percent experience economic pressure on editorial content (Hanitzsch et al., 2019). These institutional features also impact the perceptions of Austrian citizens: In a representative survey by Gallup and Mediahaus Wien, more than one third of participants believed that positive reporting can be bought in most private media outlets.

The high media ownership concentration in Austria is also reflected in the print newspaper market. Table 1.1 lists the ten largest daily print newspapers by circulation. The top-selling newspapers are three tabloid newspapers, followed by midrange newspapers, newspapers with a regional focus, and quality newspapers.

Table 1.1: AUSTRIAN PRINT NEWSPAPER MARKET

	Type	Print circulation	Media reach (in %)
KRONE GESAMT	Tabloid	750.850	27,2
Heute Gesamt	Free Tabloid	566.815	12,2
ÖSTERREICH & oe24	Free Tabloid	562.123	8,8
Kleine Zeitung	Midrange	274.488	10,4
KURIER GESAMT	Midrange	135.577	7,0
OÖNachrichten	Regional	122.267	4,7
Tiroler Tageszeitung	Regional	87.234	3,7
Salzburger Nachrichten	Regional	72.567	2,9
DER STANDARD	Quality	68.271	6,6
Die Presse	Quality	65.632	4,2

Notes: The table shows the ten largest daily print newspapers in Austria by circulation. Newspapers highlighted in bold are included in my sample. Midrange newspapers are nationally distributed newspapers with a regional focus. Reach is according to the Austrian Media Analysis. The circulation figures refer to the annual averages listed as print circulation in the Austrian Circulation Control. Both figures are for 2019.

1.2.3 Media Financing and Advertisements

Since 2012, public legal entities are required to report their quarterly expenditure for advertising and information placements to KommAustria – an independent regulatory and supervisory authority ('Media Transparency Act'). Payments are differentiated between 'Media cooperations' (§2a and §2b), i.e. paid advertisements, and 'Media promotions' (§4), i.e. financial support for artistic or non-commercial media that is not matched by a direct printed or broadcast counterpart for the (public) sponsor. Despite the declining reach of daily printed newspapers, the share of public expenditure on newspaper (versus e.g., TV or radio outlets) is remarkably high. In 2013, federal ministries spent over 90 percent of their budgets on the print market and this number remained well above comparatively levels of the private market (above 70 percent in 2022). As part of the payments to printed newspapers, payments for advertisements represent the majority (88 percent in 2021) of expenditure and are far more important than media promotions.

The 'Media Transparency Act' in 2012 also placed restrictions on the design and content of advertisements. It prohibited the depiction of ministers and image advertising; requiring advertisements to be clearly labeled as such and to look different from editorial content. Most importantly, following the reform, government advertisements have to satisfy citizens' need for information, present factual information on the legal situation or instructions for action, and are not allowed to pursue marketing purposes. Unlike advertisements by political parties, advertisements by ministries – which are

financed by taxpayers' money – are explicitly banned for election campaigning. Public advertisements are therefore designed to be a powerful tool to communicate information to citizens and inform them about the work of the government. An example were the information campaigns by the Austrian Ministry of Health regarding public safety measures during the COVID-19 pandemic.

Ministries in Austria have communications and media budgets to run such advertisement campaigns. Each ministry alone can decide how to use its communications and media budget. Importantly, ministers have great discretion over how to use these funds, i.e. which media outlets to target. A report by the independent 'Medienhaus Wien' concludes that: '*The scattering of budgets shows that neither between ministries nor between coalition partners has a comprehensible common allocation formula been agreed upon or accepted across the board as a basis for accounting*' (Kaltenbrunner, 2020). This exemplifies that the granting of funding was not strictly tied to equity or need concerns, but involved high levels of discretion on the part of individual ministers.

General patterns of the data support the notion of discretionary use of funds. Advertising patterns to newspapers suddenly change, or advertisements to one newspaper even fully stop when there is a change in ministers. Advertisements also decline when ministerial resignations are completed, foreseeable or at least already personally planned (Kaltenbrunner, 2022).

The ministry of transport provides a well-suited example. During the Faymann I, Faymann II and Kern governments, the ministry was led by the SPÖ, during Kurz I it was led by the FPÖ and during Kurz II it was led by the Greens. The allocation of the advertising budget to newspapers strongly suggests that the ministry did not apply objective criteria that spanned legislative periods, but rather allocated funds according to party-political interests. For instance, SPÖ ministers allocated nearly 20 percent of their total media budget to the 'Kronen Zeitung', while the allocation percentages for FPÖ ministers ($\sim$ nine percent) and Green ministers ($\sim$ six percent) were significantly lower. Even more striking is that FPÖ ministers allocated virtually zero percent of their budget to the newspapers 'Der Standard' and 'Kurier', while both newspapers received a non-negligible part of the overall budget from SPÖ and Greens ministers. Section 1.4.2.1 provides evidence that differences in allocations can be traced back to political alignment between parties and newspapers.

1.3 Data

1.3.1 Media Content

Data on newspaper coverage comes from Genios, which is a commercial provider of electronic press, corporate and business information. Specifically, I use the entire corpus of all published articles in full text between 2012 and 2021 for five leading Austrian newspapers. The sample includes the largest nationally distributed tabloid newspaper, the 'Kronen Zeitung', as well as 'Der Standard' and 'Die Presse', which are the two largest nationally distributed, quality newspapers. I also cover the newspapers 'Kurier' and 'Kleine Zeitung' which are midrange newspapers (nationally distributed newspapers with a regional focus). The sample covers a substantial part of the print circulation and media reach and represents the whole range of the Austrian newspaper market (see Table 1.1). In total, the corpus amounts to more than 6.4 million articles. In addition to the text of the articles, I also observe meta-information such as the publication date, page, and category.

I use the media corpus to construct some of the main variables in my analysis. First, I filter the whole corpus to extract relevant articles that refer to individual ministers using Apache Lucene string search queries. For the string queries, I rely on tested and validated search keys from the Austrian National Election Study (AUTNES). Using a narrow window around nationwide elections, AUTNES collects data on voters, political parties, and candidates. It also collects data on media coverage during the election campaign. When available, I adopt the search string queries used by AUTNES or construct own queries that closely follow their logic.

The adaption procedure of those search strings included '*adding and removing of search terms, adapting the word distances, and varying different sub-blocks and operators. After each adaption round, the search strings were tested for recall and precision against a set of 100 articles [...] until no significant change in performance (i.e., recall and precision) was achieved*' (AUTNES, 2019). The search strings to identify whether an article refers to an individual minister deliver recall and precision rates of well above 90 percent. They are therefore well-suited to be applied to my corpus. For example, articles on defense minister Tanner are selected if either her full name 'Klaudia Tanner' is stated, or her family name 'Tanner' is used in combination with some form of her title e.g., 'minister' or some form of her party name e.g., 'ÖVP' in the previous or successive five words. All search strings are presented in Appendix 1.C.4.

After employing the search strings and thoroughly cleaning the text corpus, I observe a total of 133,195 articles that mention at least one minister.[6] This data can be used to measure the coverage of an individual minister in a particular newspaper on a daily basis. To match the quarterly advertisement data, I collapse the data to the quarterly level by individual newspapers and ministers.

Validation of the coverage variable. I extensively validate that the filtered articles capture relevant political articles in a data-driven way. I first perform part-of-speech (PoS) tagging on the 133,195 articles to keep only tokenized nouns. I then split the corpus so that articles about different ministers from the same ministry are grouped together. This makes it possible to assess whether these sub-group corpora feature articles on relevant political topics. For example, I check whether the articles about ministers from the Ministry of Education comprise topical features such as schools, teachers, or universities. Figure 1.A.3 displays word clouds of the 50 most common words within each sub-group corpora. The figures demonstrate that the selected articles capture relevant political coverage and are clearly linked to the ministerial topics. This provides strong evidence that the identified articles capture meaningful reporting about ministers.

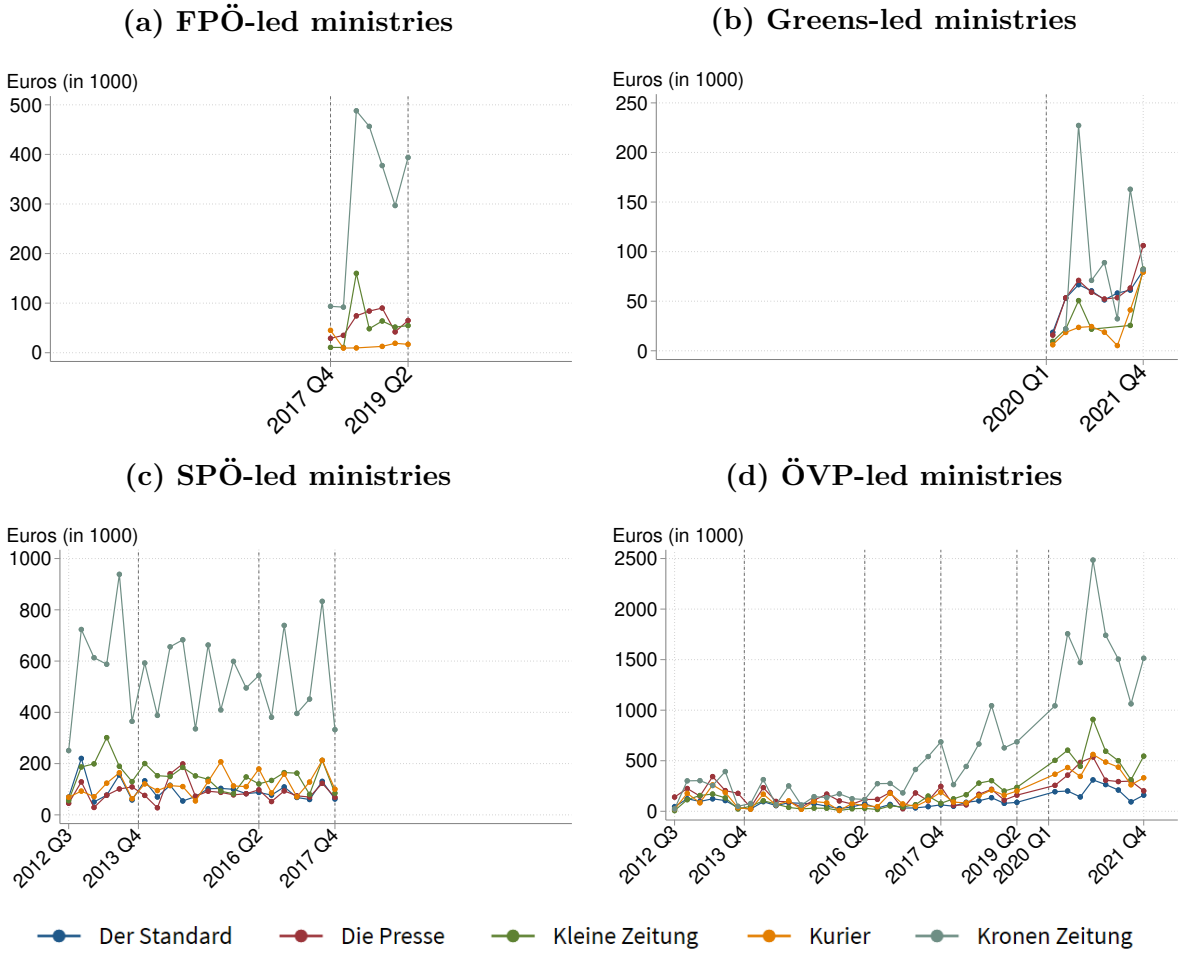
1.3.2 Advertisements

Quarterly advertisement payments. I obtain data on advertisement payments from the website *medien-transparenz.at*, which collects and stores the administrative data released by KommAustria. The data includes all payments to media outlets (print, TV, and radio) from public entities such as ministries and state-owned enterprises on a quarterly level from Q3 2012 until Q4 2021.[7] I focus on payments from ministries, including the Chancellor's office, to newspapers. As my sample includes large commercial newspapers, the payment flows only constitute paid advertisements and do not cover 'media promotions'. Figure 1.2 shows the payments made by the ministries, split by the party affiliation of the ministers.

[6] Importantly, advertisements themselves are not included in the corpus. I also remove letters to the editors and TV announcements, calculate similarities of articles using locally sensitive hashes to remove duplicates with a jaccard similarity of above 0.1 in the same newspaper on the same day, remove articles that are in the local news sections, and remove articles with less than 20 words.

[7] Payments have to reported if they exceed a threshold of 5,000 Euros during a quarter. As a consequence, the reported aggregated amount underestimates the actual expenditure. Payments to large newspapers, however, exceed the threshold by an order of magnitude.

Figure 1.2: PAYMENTS FROM MINISTRIES TO NEWSPAPERS



Notes: The figure shows payments from ministries to newspapers included in my sample led by the four parties FPÖ, Greens, SPÖ, ÖVP on a quarterly level from 2012 to 2021.

Daily advertisements. A disadvantage of the quarterly administrative data is that it does not allow for a more fine-grained analysis. I therefore supplement it with advertising expenditure by the federal ministries on a *daily* level between January 2012 to November 2021. I obtain this data from the private company 'Media FOCUS Research Ges.m.b.H.', which is a leading market research company on price and promotion mechanics in Europe. FOCUS records published advertisements in media outlets on a daily level. The company also calculates the list price of each advertisement according to its size, page, and weekday. The main benefit of this supplementary data is that I observe the exact date of advertisements by ministries as well as the associated list prices.

I use this data for two purposes. First, it allows to observe advertisements by political parties. Such advertisements – in contrast to advertisements by federal ministries – are allowed to be used for campaigning. Party advertisements could therefore be a

confounder in the empirical model when they are strategically used to supplement or complement a ministry advertisement. Second, I use daily data on advertisements for the Kurz II government (2020 to 2021) to match advertising data with the household-level data that starts in 2020. In total, the data in this time span contains 1,721 relevant advertisements (on average, 2.45 ads per day) between January 2020 to November 2021.[8]

1.3.3 Household Panel

To assess the downstream effects of public media capture, I employ data by the Austrian Corona Panel Project (ACPP). The ACPP is a household survey that was launched in late March 2020 (Kittel et al., 2020). Building on a panel of 1,500 socio-demographically representative households, the main purpose of the survey is to track the Austrian population's opinions and behavior during the COVID-19 pandemic. From the end of March 2020 to the beginning of July 2020, the survey was conducted weekly, afterwards the interval gradually increased, and since August 2020, the data has been collected on a monthly basis. The high frequency of the survey and the broad coverage of variables makes the ACPP data uniquely suited for my purposes. I use survey waves one to 27, covering the time span from March 2020 to December 2021.

Crucially, respondents are asked about their media consumption in the last week. This allows to create an 'exposure' variable that reasonably proxies to which newspaper articles households have been exposed. The survey also records voting intentions on the intensive margin (on a one to ten scale) on a similar high frequency, allowing to test for immediate effects of media content on voting intentions.

1.4 Empirical Strategy and Main Results

1.4.1 Conceptual Framework

My empirical strategy is divided into four parts. First, I provide descriptive evidence that political alignment between parties and newspapers predicts advertising payment flows. In the second part, I present extensive evidence of a positive association be-

[8]The data by Focus overestimates the actual expenditures because the list prices do not take discounts into account. On average, the reported expenditures by Focus are 1.34 times larger than the expenditures reported by KommAustria during Q1 2020 until Q3 2021.

tween ministerial advertisement flows and newspapers' coverage of individual ministers. These patterns emerge both in the quarterly long-run data and in the daily data. The findings are robust across a range of specifications, alternative measures of coverage, controlling for potential confounders, and several timing misspecification tests. Relying on a shift-share instrumental variable approach for identification, I demonstrate that the baseline estimates reflect causal results. I also provide evidence for a direct advertiser mechanism, ruling out alternative market-based explanations.

Third, I examine soft factors of reporting and trace channels of media bias. I demonstrate that advertising also influences the *intensive* margin of media content, specifically its topic focus and sentiment.

Finally, I combine the data on media content with panel data on citizens' media consumption, voting intentions, and political attitudes. Exploiting advertisements by ministries as an instrumental variable for media coverage, I examine how media content shapes voters' perceptions and can contribute towards changes in beliefs. Hence, those results illustrate the effectiveness of media capture.

1.4.2 Two-way Favors between Advertising and Coverage

1.4.2.1 Political Alignment and Payment Flows

Discretion in advertisement allocations. Anecdotal evidence shows that ministers have wide discretion over how to use their media budgets (see Section 1.2.3). The use of an economic-based formula also seems extremely unlikely when considering the government's expenditure per reader. Based on the payment flows and print circulations in 2021, all ministries combined paid 5.1 (3.6) euros to reach one reader of the newspaper 'Presse' ('Kronen Zeitung'), while they paid only 2.1 euros for one reader of the newspaper 'Der Standard'. Finally, there are clear electoral cycles visible in the amounts of ministerial spending: The value of advertisements by ministries is significantly higher in quarters with national elections (see Figure 1.A.1). Therefore, the evidence overwhelmingly suggests that ministries could use their budgets for advertisements to strategically target individual newspapers.

Advertising favors. Figure 1.A.2 plots the share of the total media budget that ministers of different political parties allocated to individual newspapers over the time span 2012 to 2021. Parties' spending allocations differ substantially across newspapers. I construct a measure of political alignment between parties and newspapers to

examine whether alignment can account for the different allocations.[9] I regress the share of the total budget that newspapers received from the different parties on the ideological alignment measure, conditional on newspaper fixed effects (to account for different newspapers characteristics such as reach). This regression tests whether ideological alignment between a newspaper and the political parties predicts payment flows. The regression yields a highly statistically significant coefficient of -0.041 ($t = 1.92$), showing that a greater ideological distance between parties and newspapers lowers received payments for advertisements. This provides (suggestive) evidence for advertising favors.

1.4.2.2 Coverage Favors

To assess whether advertising spending by ministries relates to newspapers' coverage decisions, I estimate the following baseline specification:

$$Coverage_{mnq_t} = \beta * Advertising_{mnq_t} + \mu_{q_t} + \phi_n + \phi_n \times \zeta_m + \epsilon_{mnq_t} \tag{1.1}$$

where $Coverage_{mnq_t}$ is the number of articles about ministers from ministry m in newspaper n during quarter q (in year t); and $Advertising_{mnq_t}$ are the payments from ministry m to newspaper n in the same year-quarter q_t, measured in 1,000 euros. The unit of observation in my empirical analysis is thus at the ministry-newspaper–quarter level. Fixed effects on the newspaper (ϕ_n) and quarter-of-year (μ_{q_t}) level account for newspaper-specific factors and period-specific shocks. Using those fixed effects (only) exploits variation in ministerial spending across newspapers ('*cross-sectional variation*'). The newspaper-ministry interaction fixed effects, $\phi_n \times \zeta_m$, (or alternatively, newspaper-minister fe's) explicitly account for the ideological proximity between newspapers and ministries (ministers). Including those fixed effects exploits variation in advertisements over time *within newspaper-ministry (newspaper-minister) pairs*.[10]

[9] I use party ratings on a left-right scale form the V-Party project to assign ideology scores to the parties. For the newspapers, I first construct a partisan media use measure based on the ACPP data. I then assign the newspapers an ideology score based on the V-party scores weighted with readers' party preference shares. This yields an intuitive left-right ranking of newspapers: 'Der Standard' is the newspaper with the most left-leaning ideology, followed by the 'Kleine Zeitung'. The newspaper 'Kurier' is placed in the middle, while 'Die Presse' and 'Kronen Zeitung' are placed on the other side of the ideological spectrum. The absolute difference between the (standardized) party- and newspaper score measures ideological alignment: The lower the difference, the more closely aligned a newspaper is with a political party.

[10] I treat ministries that (partly) change responsibilities across legislative periods – e.g. the 'Ministry of Labor' and the 'Ministry of Labor and Social Protection' – as separate ministries. Most changes in minister are therefore within the same party and not across parties when using newspaper-ministry

The identifying variation on the ministry-newspaper-quarter level further allows to augment the baseline model with ministry × quarter-of-year (or newspaper × quarter-of-year) interaction fixed effects. This allows to account for ministry- and newspaper-specific shocks in individual quarters. In all estimations, I allow standard errors to be autocorrelated and heteroskedastic by using Newey–West standard errors with one lag.

Table 1.2: QUARTERLY REGRESSION RESULTS 2012–2021 — COVERAGE

Dependent variable: Coverage (# of articles)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Advertising	0.320***	0.091***	0.017	0.049*	0.077***	0.058**	0.068***
	(0.042)	(0.022)	(0.033)	(0.026)	(0.024)	(0.025)	(0.026)
Advertising × Dependency			0.057**				
			(0.023)				
Advertising × Scandal							-0.070*
							(0.039)
Fixed effects:							
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Newspaper	Yes	-	-	-	-	-	-
Ministry x News.		Yes	Yes	-	Yes	Yes	Yes
Minister x News.				Yes	-	-	-
Newspaper x Quarter					Yes	-	-
Ministry x Quarter						Yes	Yes
Observations	2,165	2,165	2,165	2,165	2,165	2,165	2,165
Ministries (#)	29	29	29	29	29	29	29
Ministers (#)	47	47	47	47	47	47	47

Notes: The table shows regression results on the relationship between advertisements by ministries and coverage of ministers on the quarterly level, empirically estimating equation (1.1) using data from 2012 to 2021. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Theoretical priors. From a theoretical perspective, the sign of the coefficient of interest, β, is *a priori* unclear. The coefficient could be positive when advertising is associated with an overall increase in coverage. It could be negative when advertising crowds out (critical) reporting. When newspapers' coverage of ministers is independent of advertisements by ministries, then β is expected to be zero.

Results. Table 1.2 shows the main baseline results. Column (1) shows that payments for advertisements by ministries are associated with *higher* coverage of the corresponding minister *within newspapers* – explicitly allowing for cross-sectional variation in min-

fixed effects. The specification using newspaper-ministry fixed effects is thus less demanding due to fewer fixed effects.

isterial spending across newspapers. Column (2) adds newspaper-ministry interaction fixed effects, demonstrating that this pattern holds *within newspaper-ministry pairs*. The coefficient size decreases considerably but stays statistically significant. Ideological proximity between newspaper and advertiser is therefore relevant for advertising and coverage decisions, but cannot fully explain the positive relation between advertisement and coverage. In numerical terms, the estimated coefficient means that a one standard-deviation increase in advertising by one ministry (75,000 Euros) is associated with seven more articles about the corresponding minister (nine percent of a standard deviation).[11]

Column (3) adds an interaction term between the advertisement variable and a time-varying dependency measure, defined as the quarterly percentage of the newspapers' total revenue derived from federal government payments. This measure thus captures the extent to which a newspaper relied on public funds in a given quarter. The results show that, even within newspaper-ministry pairs, the increase in coverage is significantly more pronounced in quarters when the newspapers' dependency on public funds was higher – providing evidence that reliance on advertisers plays a key role for media capture.

Column (4) replicates the baseline analysis *within newspaper-minister pairs*. The coefficient gets slightly smaller, but stays significant. I obtain an almost identical coefficient estimate (0.044; se: 0.020), when using newspaper-ministry fixed effects and adding party affiliation fixed effects.

Column (5) adds newspaper-quarter interaction fixed effects and Column (6) adds ministry-quarter interaction fixed effects. The coefficients in both specifications are similar to the baseline estimate and remain highly significant. The inclusion of ministry-quarter interaction fixed effects allows to examine the differential effect of advertising on coverage when ministries/ministers experience shocks in individual quarters. Column (7) therefore adds an interaction term between the advertisement variable and a dummy on whether a minister experienced a political scandal in a given quarter.[12] The baseline effect of the scandal is absorbed by the fixed effects, but the coefficient of the interaction term turns out to be statistically significant and *negative*. This shows

[11]To visualize these results, Figure 1.A.4 presents value added plots between the fe-residualized advertising and fe-residualized article coverage, examining within newspaper variation (corresponding to column 1 in Table 1.2) and within newspaper-ministry variation (corresponding to column 2 in Table 1.2). The figures highlight the robust pattern that I obtain using both sets of variation, ruling out that the results are driven by outliers.

[12]I self-collect data on political scandals involving Austrian ministers between 2012 and 2021. Appendix Section 1.C.1 provides details on how I identify scandals. It also lists the 48 scandals identified and provides information on the actors involved.

that in 'good times', advertising increases coverage, and in 'bad times', advertising decreases coverage. This is in line with the strategic incentives faced by politicians. It also reconciles findings from the previous literature on a negative relationship between advertisements and negatively connoted corruption coverage.

1.4.2.3 High-Frequency of Coverage Favors

I next examine the frequency of coverage favors, exploiting the granular information on the exact date of advertisements in the FOCUS data. Using the sample from 2020 to 2021, I therefore estimate a variant of equation 1.1:

$$Coverage_{mnt} = \beta * Advertising_{mnt} + \mu_t + \phi_n \text{ x } \zeta_m + \epsilon_{tnq} \tag{1.2}$$

where $Coverage_{mnt}$ is the number of articles about a minister of ministry m in newspaper n and time-period t (week-of-year or day-of-year). $Advertising_{mnt}$ is the sum of the value of advertisements placed by ministry m in newspaper n during the time-period t in 1,000 euros and ϕ_n x ζ_m are newspaper-minister interaction fixed effects.

Table 1.3 shows the results. Columns (1) and (2) use data on the weekly level. Inferences drawn from the quarterly data are robust to higher-frequency: Weekly advertising by one ministry – measured both intensively (column 1) and extensively (column 2) – is associated with higher weekly coverage of that minister. Importantly, I also observe similar asymmetric effects of advertising on reporting depending on the state of the world. Advertisements increase coverage in good times and decrease coverage when a shock occurs. However, the limited number of scandals in this shorter sample period makes those estimates more imprecise.

To fully exploit the fine-grained temporal nature of the FOCUS data, columns (3) to (6) of Table 1.3 use daily data. Column (3) and (4) replicate the positive pattern between (extensive and intensive) advertising and coverage within newspaper-minister pairs, even with day (or weekday) fixed effects. If a ministry publishes an ad in a newspaper on a given day, it is on average more than six percentage points more likely to observe an article about the corresponding minister in that newspaper on the same day (column 5).

Excessive coverage. I also examine whether advertisements give rise to 'excessive' coverage. I create an indicator variable that takes the value of one if a given newspaper published an article about a minister and only one other newspaper also published an article about that minister on the same day. This measure therefore abstracts from

Table 1.3: WEEKLY AND DAILY REGRESSION RESULTS 2020–2021 — COVERAGE

	Weekly Level		*Daily Level*			
	(1) Cov.	(2) Cov.	(3) Cov.	(4) Cov.	(5) Has Article	(6) Exc. Cov.
Ad value	0.014**		0.010***			
	(0.006)		(0.002)			
Has Ad		0.698***		0.232***	0.059***	0.015*
		(0.238)		(0.045)	(0.013)	(0.008)
Fixed effects:						
Week	Yes	Yes				
Day			Yes	Yes	Yes	Yes
Minister x News.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,000	6,000	42,000	42,000	42,000	42,000

Notes: The table shows regression results on the relationship between advertisements by ministries and coverage of ministers on the weekly and daily level, empirically estimating equation (1.2) using data from 2020 to 2021. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity. As a variable of interest, I either include the value of the advertisements as measured by Focus or a dummy on the existence of an advertisement.

more general and influential (political) events, on which most newspapers report. I then re-estimate the (binary) specification using the excessive coverage indicator as the dependent variable. The parameter estimate of the advertising measure is positive and statistically significant (Column 6). If an ad from a ministry is published in a given newspaper, the likelihood that this newspaper publishes an 'excessive' article about that minister increases by 1.5 percentage points. Results for the intensive measurement of advertising (specification in column 3) yields similar inferences (p-value: 0.16).

Timing. If advertisements give rise to an increase in newspaper coverage, the exact timing between advertisement and coverage favor exchanges might be unclear. Using quarterly data arguably helps to eliminate potential timing misspecifications that could arise in more fine-grained data. As such, consistent results independent of the level of aggregation are reassuring. The concern that equations 1.1 and 1.2 potentially suffer from misspecification, however, remains. Qualitative evidence that contemporary advertising matters derives from §2 (5) of the Media Transparency Act: '*The relevant date for the obligation to disclose is in each case the time of performance of the service.*' The reported payment flows in one quarter therefore reflect the publishing of ads in the same quarter. Yet, this does not rule out that potential coverage favors arise in later time periods. I therefore thoroughly examine whether favor exchanges are dynamic.

Column (1) of Table 1.B.5 adds a lagged outcome variable to the empirical model to assess the degree of auto-correlation. The coefficient of the contemporary advertising variable remains positive and highly significant. I also assess the impact of potential auto-correlation in the explanatory variable. Tables 1.B.1, 1.B.2, and 1.B.3 include lagged values of the advertising variable on the daily, weekly, and monthly level. In all cases, the size and statistical significance of the contemporary advertising variable remains virtually unchanged when I include different lengths of aggregated lags (up to seven lags for the daily level, four lags for the weekly level, and three lags for the monthly level). With the exception of one-day lags – which are negatively correlated with the coverage variable – all daily lags do not turn out to be statistically significant. This supports the notion that 'excess' coverage is a direct response to current advertising. The same conclusion is also corroborated by controlling for aggregated lags on a weekly and monthly level where the coefficient of the contemporary advertising variable remains significant, but the coefficients on the lagged values (with one exception) do not turn out to be statistically significant.

I also test for dynamics in both the dependent and the independent variable in the quarterly data by estimating a system GMM model. Here, I allow for two lags in the advertising variable and control for the lagged dependent variable. The GMM results confirm that the contemporary advertising variable is positively and significantly correlated with the contemporary coverage, while longer lags of the advertising variable do not turn out to be statistically significant.[13]

The evidence strongly suggests that the two-way favors operate on a high frequency, with immediate effects and very little dynamic back-flow of favors. It is therefore unlikely that equations 1.1 and 1.2 are significantly misspecified. The results suggest that not only can ministries capture newspapers, but newspapers may also hold some power over politicians – even in well-established relationships – if they can credibly threaten to withhold positive coverage when payments decrease.

1.4.3 Robustness

Heterogeneity. An important question is whether the relationship between advertisement payments and coverage is a product of a specific legislative period, or driven by

[13]I perform a two-step sys-GMM model following best practice recommendations in Roodman (2009). I treat the lagged dependent variable as well as the current-, first-, and second-order lagged advertising variable as endogenous. I collapse the instruments, resulting in 129 instruments (with 145 groups), and employ the Windmeijer correction for two-step estimations. The GMM model specification passes conventional diagnostic tests (Arellano-Bond test for AR(1) p-value: 0.00; AR(2) p-value: 0.702; Hansen test p-value: 0.105).

individual newspapers. Hence, I examine potential heterogeneity among the time- and newspaper-dimension by plotting the residuals of the advertising and coverage variable after regressing them on time- and newspaper-ministry interaction fixed effects. Such value-added plots visualize the *partial* effect of advertising on coverage after removing the fixed effects. Panel (a) of Figure 1.A.5 provides evidence that the positive association between the two variables holds – with the exception of the Kurz I government – in all legislative periods. Panels (b) – (d) consider the relationship between advertising and coverage separated by parties and individual newspapers. The figure reveals interesting heterogeneity between and within newspapers. While the association is nearly flat (or even negative) for the newspaper 'Der Standard' (Panel f), there are stronger associations for the newspapers 'Kleine Zeitung' or 'Kronen Zeitung' (Panels b and d).

Party advertisements. I include advertisements by political parties as an additional control using the FOCUS data. Table 1.B.4 shows that the coefficients of the advertisements by ministries, and their statistical significance, virtually remain unchanged when controlling for party advertisements. In addition, the results show that the positive association between advertisement and coverage about ministers arises almost exclusively for advertisements by ministries and not for party advertisements. This indicates that for party advertisements – which can be used directly for campaigning – there are smaller incentives for favor exchanges.

Aggregating to the party level. The results so far were derived from regressions on the individual ministry-level. I obtain consistent results when aggregating the data to the *party* level (see Column 2 of Table 1.B.5).

Linear time trends. To rule out that the baseline results are driven by time trends, I interact the ministry-newspaper pairs with linear time trends (using a function over the number of quarters the government has been in office) during the different government periods. If anything, the coefficient size increases and remains highly significant (see Column 3 of Table 1.B.5).

Time-variant newspapers confounders. The results are robust to controlling for time-variant newspaper characteristics (e.g., their market reach), suggesting that the results are not driven by changes in the size of the newspapers' audiences (see Column 4 of Table 1.B.5).

Clustering of standard errors. The results are robust to clustering the standard errors at the ministry-newspaper level instead of using Newey-West standard errors (see Column 5 of Table 1.B.5).

Alternative coverage measures. I use an alternative measure of coverage for a minister by focusing on the *number* of mentions rather than the number of articles mentioning the minister. While the previous specification only counted an article once, even if it referred to the minister multiple times, this approach aggregates all mentions. This distinction captures the difference between an article that scarcely mentions a minister, and one that extensively focuses on him/her. The results are qualitatively unchanged and remain statistically significant (see Column 6 of Table 1.B.5).

Placement of articles. The analysis so far has focused on the *extensive* margin of coverage, i.e., whether newspapers report more on individual ministers regardless of article placement. To assess whether advertisements influence prominence, I examine their impact on article positioning or the incidence of front-page articles. The results show that, conditional on coverage, advertisements neither improve article placement nor increase the likelihood of front-page coverage (see Column 7 of Table 1.B.5).

1.4.4 The Tone and Thematic Focus of Reporting

Topic shares. The dependent variable so far was derived from the mention of a minister's name. I alternatively employ an unsupervised machine learning algorithm (LDA) to predict latent topics in the full corpus.[14] For each ministry, I construct the share of associated topics covered in each newspaper during a given time period. I then examine whether advertisements by ministries are also associated with an increase in the respective ministry topic share covered by the newspapers. Specifically, I replace the dependent variable, ministers' coverage, in Equation 1.1 and 1.2 with the topic share (associated with ministry m) covered in each newspaper n in time t.

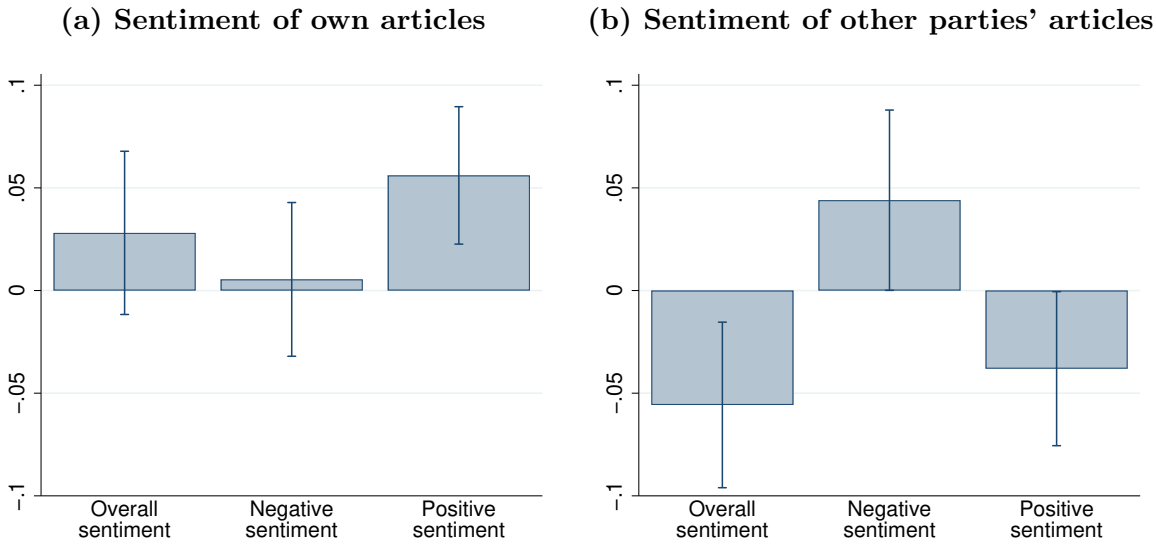
The results show that advertisements are also *positively* correlated with ministries' topic shares – both on the quarterly and even on the daily level (see Table 1.B.6). An advertisement by one ministry in one newspaper increases the covered topic share of that ministry in that newspaper by 0.44 percentage points *on the same day*. I also again observe (imprecisely estimated) differential responses to scandal shocks. As

[14] I describe in detail how I train the LDA model and assess its performance in Appendix 1.C.2.

advertisements themselves are not featured in the sample and cannot account for the increase in topic shares, the results provide evidence that advertising induces separate coverage.

Spillovers and sentiment. I next examine whether and how advertisements by ministries influence the *tone* of the media coverage, i.e. the articles' sentiment.[15] I find that advertisements influence articles' sentiment. Panel A of Figure 1.3 shows the estimated coefficients of the advertisements variable when using the average sentiment of articles about the minister as the dependent variable in Equation 1.1. The figure shows coefficients on three sentiment indicators: Overall sentiment, the share of negative words, and the share of positive words. The results show that advertising by a ministry crowds-in *positive* reporting about the corresponding minister.

Figure 1.3: ADVERTISEMENTS AND SENTIMENT



Notes: The figure shows the estimated coefficients on the advertisements variable when using articles' sentiment as the dependent variable, using the quarterly data from 2012 to 2021. In Panel A, I use the sentiment indicators of the articles about the own minister as the dependent variable. In Panel B, I use the sentiment indicators of the articles about ministers from the opposing party as the dependent variable. The coefficients show the increase in the sentiment indicators (as % of a standard deviation) when the advertising variable is increased by one standard deviation.

Politicians might pursue a second objective: Rather than seeking favorable coverage for themselves, they may engage in negative campaigning — lobbying for articles that portray opposing party politicians in a negative light. Using the number of articles about the opposing coalition party's ministers as the dependent variable, I find evidence for *positive* coverage spillover effects (coef.: 0.016; $t = 2.34$). They are sub-

[15] Appendix 1.C.3 provides a detailed explanation on how the sentiment indicators are constructed and provides a validation of the sentiment measure.

stantially smaller in size compared to the direct effect, but demonstrate that payments of one ministry might influence coverage about politicians of opposing parties. Panel B of Figure 1.3 offers an explanation for the *increase* in articles about ministers from the opposing party: The effects on the sentiment indicators lie in contrast to those observed for their own coverage. Specifically, articles about opposing party ministers are significantly more negative, featuring a higher proportion of negative words and fewer positive words. These findings suggest that politicians may in fact use advertisements to engage in negative campaigning.

1.4.5 Identification

The inclusion of newspaper-ministry fixed effect rules out that the observed empirical patterns can be solely explained by ministers funneling funds to connected media outlets providing favorable coverage. Even within newspaper-ministry pairs, the empirical estimates could, however, suffer from a simultaneity bias, where the realized advertisement flows are a function of the political reporting ('reverse causality').

Identifying exogenous variation in payment flows is challenging given that budget allocation is discretionary. To address this, I employ a shift-share instrumental variable design that leverages the *ideological proximity* between parties and newspapers (shares) and changes in *access* to discretionary flows (shifter). This instrument generates variation both cross-sectionally *and* over time. The logic of the shift-share design adheres to recent empirical implementations in political economy (e.g., Beattie, 2020; Ash et al., 2024) and best practice recommendations (Borusyak et al., 2024). I describe in detail how I construct the shares and the shifter, how the exogenous variation arises, and provide a visual validity check of the first stage.

Shifter. The shifter is based on changes in ministerial budget items allocated to third-party work. Specifically, for all ministries I collect the sum of the annually *proposed* budget items for 'Work services by third parties' that are not earmarked in the years 2013 to 2021 (2012 is not included due to missing budget data). Those budget items encompass expenses for public relations and information activities (hence including advertising campaigns in newspapers), but more broadly capture funds to third parties for services. I focus on detail budgets of the 'Central Office', which is responsible for strategic decisions on the ministerial level, preventing funds from being mostly tied to operational purposes. Since these budget items are not earmarked, they serve as a reasonable proxy for available discretionary funding.

Budgets are *not* proposed for ministries, but are structured along subdivisions. Those subdivisions may represent general state functions (such as the Subdivision UG 03 for the Constitutional Court or Subdivision UG 06 for the Court of Auditors) or subdivision that belong to ministries (such as Subdivision 14 for Military Affairs or Subdivision 21 for Social Affairs and Consumer Protection). Each subdivision consists of several 'Detail budgets' containing individual budget items (with tractable budget account numbers).

Variation in access to discretionary funding arises from three sources. First, there are substantial changes in the number of relevant budget items *within* subdivisions and detail budgets over the years. Such adjustments often stem from fluctuations in anticipated fiscal year revenues and may lead to either budget tightening or expansion.

Second, 'Detail budgets' are occasionally reallocated across subdivisions associated with different ministries, without changing the general purpose of the ministries. These shifts often result from administrative restructuring.

Third, larger reshuffles of ministries may result in entire subdivisions being transferred between ministries. A good illustrative example is the Ministry of Labor. From 2012 to 2017, this ministry was called 'Ministry of Labor, Social Affairs, and Consumer Protection' (including Subdivisions UG20 and UG21), from 2017 to 2019, it was called 'Ministry of Labor, Social Affairs, Health, and Consumer Protection' (including Subdivisions UG20, UG21, and UG24), and from 2019 to 2021, it was called 'Ministry of Labor' (including only Subdivision UG20). These budget shifts are part of broader government restructuring processes. A potential concern might be that the creation of additional subdivisions could lead to broader ministerial responsibilities, and in, turn, increasing media coverage. This could pose a threat to identification, as changes in responsibilities are likely to influence political reporting. However, ministries that undergo responsibility changes are treated as *separate* entities. For instance, the 'Ministry of Labor' from 2012 to 2017 is treated separately from the ones in 2017 to 2019 and 2019 to 2021. Thus, any mechanical adjustments in budgets and media coverage resulting from the creation of more subdivisions are accounted for by ministry fixed effects.

The identifying variation therefore arises from changes in expected revenue (which lead to budget adjustments) or administrative restructurings, both of which should be independent of advertising spending and political reporting.

Shares. The shares are based on *parties'* pre-determined advertisement allocations across newspapers. I construct the shares using parties' advertisement decisions be-

tween 2009 and 2012, i.e. *before* the start of my sample. This rules out dynamic effects from the shifter on the shares that could potentially confound the instrument.

For every ministry m (with a minister belonging to party p) and newspaper $n \in (1, ..., N)$ combination, I construct the following share using the FOCUS data:

$$Share_{m_pn} = \frac{Advertising_{pn}}{\sum_{n=1}^{N} Advertising_{pn}} \tag{1.3}$$

The share divides the advertising payments from party p to newspaper n during 2009 to 2012 by the total amount spent by that party to *all* media outlets $n \in (1, ..., N)$ – not just to the five newspapers in my sample – during 2009 to 2012. Intuitively, the share represents the percentage of total spending by parties on a given newspaper. The shares are time-invariant and based on pre-determined advertisement decisions by parties – not by the ministries. This share thus captures *cross-sectional* variation in the ideological closeness between newspapers and the political affiliation of ministers.

Shift-Share Variable. The shift-share instrument is obtained by interacting the shifter and the shares, creating variation across newspaper-ministry pairs and across time:

$$Instrument_{m_pqn} = \text{Share}_{m_pn} \times \text{Shifter}_{m_pq} \tag{1.4}$$

The instrument should be relevant if the pre-determined party advertisement pattern explains future cross-sectional variation in ministries' advertisement allocations and if the amount of the proposed budget items predicts advertisement spending. Figure 1.A.6 illustrates the first stage graphically, showing a binned scatter plot of fixed-effects residualized values of the instrument versus fixed-effects residualized values of advertisement spending by ministries. The figure indicates a strong and robust relationship between the instrument and spending on advertisements by ministries.

Results. Table 1.4 shows the instrumental variable results. Column (1) reports results when using cross-sectional variation (Column 1 of Table 1.2). Column (2) reports results using ministry-newspaper pair variation (Column 2 of Table 1.2), column (3) adds scandal fixed effects. Column (4) reports results using minister-newspaper pair variation (Column 4 of Table 1.2), column (5) again adds scandal fixed effects.

The instrument is a relevant predictor for payment flows with strong F-values in the first stage (see Panel A). Panel B reports the second-stage results. The coefficient

estimates of the advertisement variable are positive and highly statistically significant throughout all specifications. Advertising causally increases coverage in the cross-sectional specification, and within ministry-newspaper and minister-newspaper pairs.

Table 1.4: ADVERTISEMENTS AND COVERAGE — INSTRUMENTAL VARIABLE RESULTS

Dependent variable: Coverage (# of articles)					
	Cross-sectional variation	*Ministry-newspaper pairs*		*Minister-newspaper pairs*	
	(1)	(2)	(3)	(4)	(5)
	Panel A: First stage				
Instrument	0.260***	0.355***	0.355***	0.423***	0.422***
	(0.045)	(0.086)	(0.086)	(0.081)	(0.081)
	Panel B: Second stage				
Advertising	0.545***	0.140**	0.134**	0.144***	0.145***
	(0.090)	(0.065)	(0.065)	(0.052)	(0.052)
F-value	33.8	16.8	16.8	27.3	27.3
Observations	2,045	2,045	2,045	2,045	2,045
Fixed effects:					
Quarter	Yes	Yes	Yes	Yes	Yes
Newspaper	Yes	Yes	Yes	Yes	Yes
Ministry x News.	-	Yes	Yes	-	-
Minister x News.	-	-	-	Yes	Yes
Scandal	-	-	Yes	-	Yes

Notes: The table shows regression results on the relationship between advertisements by ministries and coverage of ministers on the quarterly level, where *Advertisements* are instrumented with the shift-share variable. Robust standard errors that are adjusted to arbitrary heteroskedasticity are reported in parentheses.

Comparing OLS and IV estimates. The coefficients obtained via the IV approach are numerically close to the baseline results, though approximately 50 percent larger than the OLS-coefficients. I perform the state-of-the art decomposition proposed by Ishimaru (2024), which decomposes the OLS-IV gap into a covariate weight difference component, a treatment-level weight difference component, and the marginal effect difference component. The results show that the majority of the OLS-IV gap is explained by the marginal effect difference component (see Appendix Table 1.B.7). This suggests that the OLS estimates represent a lower bound of the true effect.

Robustness: Alternative shares. I alternatively construct time-invariant shares based on the measure of ideological alignment between parties and newspapers (see Section 1.4.2.1). This measure is independent of (past or current) advertising decisions. Table 1.B.8 reports the IV results when using the alternative shares. The qualitative inferences remain the same: Advertising increases coverage in all specifications.

1.4.6 Mechanism: Advertiser Effect or Market-based Explanations?

The causal effect of advertisements on political coverage could derive from two different mechanisms: A *direct* advertiser effect or market-based forces. Specifically, there are two ways how the empirical results could be based on market-based forces: (1) Changes in consumer preferences (demand-driven bias) and (2) complementarities between editorial content and advertisements. As I will argue, both alternative explanations are implausible.

Demand-driven bias. In order for consumer demand (demand-driven bias) to explain the empirical patterns, demand shocks would need to jointly move with advertisement volumes *within ministry-newspaper pairs*. This would e.g. require readers' demand for political slant to increase and decrease over time *within newspapers*, and that those changes are correlated with advertisement flows. Such non-linear behavior seems implausible.

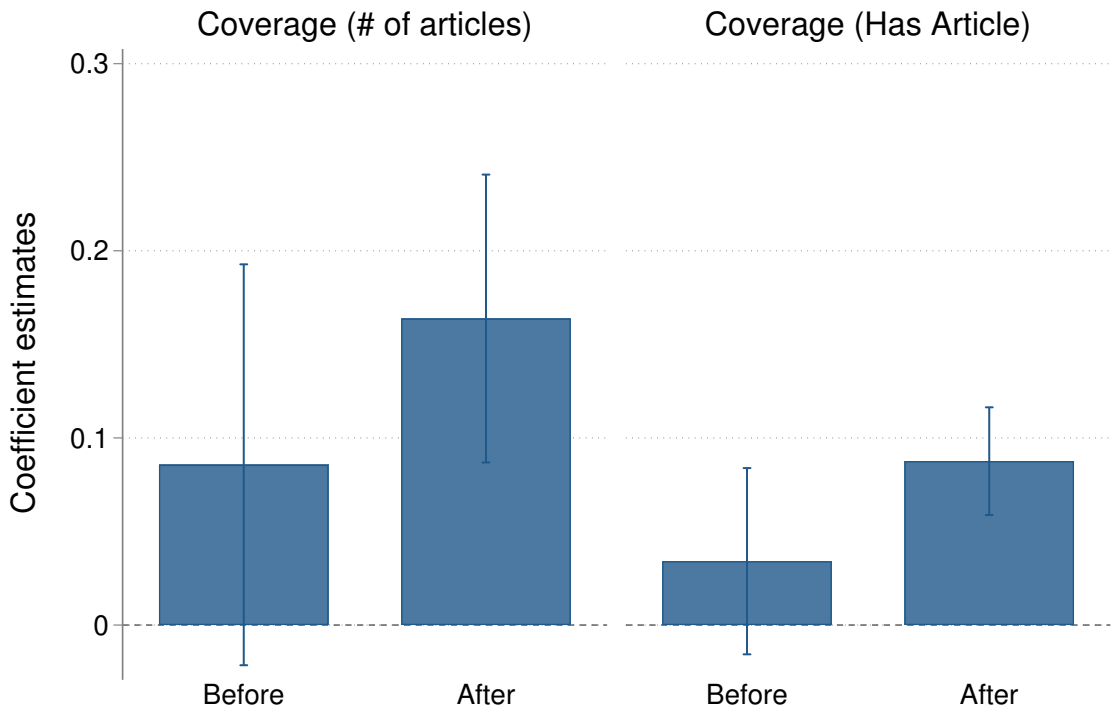
Complementarities. The positive effect of advertising on political coverage could result from complementarities between editorial content and advertisements. Such effects would e.g. materialize when advertisements are strategically placed in (temporal) proximity to articles with similar content (in coordination with newspapers). However, asymmetric effects of advertisements on the individual coverage and on the topic shares make complementarities implausible. Since advertisements increase coverage in good times, but decrease coverage in bad times, complementarities cannot account for such patterns.

I also directly examine the degree of complementarities. Using the online archives of the newspaper 'Kronen Zeitung' and 'Die Presse', I code all advertisements by their slogan and the policy they refer to during the 2020 to 2021 period. Excluding advertisements solely related to COVID-19 (without any policy-connection), I assess whether editorial content in the same week refers to the advertised policy or content

of the ad. I find such complementarities only 18 percent of the time, meaning that complementarities in content cannot plausibly drive the results.

Evidence for an advertiser effect: The 2012 reform. The 'Media Transparency Act' came into effect on July 1, 2012. Prior to the policy change, advertisements by ministries were not subject to restrictions on design or content. Advertisements by ministries could be freely used as quasi election campaigning. Contrary to the law's original intent, the incentives for favor exchanges may have increased following the reform, when coverage favors substitute direct campaigning of advertisements. Therefore, the regulation could act as an amplifying force, intensifying political influence on editorial content.

Figure 1.4: EVIDENCE FOR MECHANISM: ADVERTISER EFFECT



Notes: The figure shows estimated coefficients on a dummy on the existence of an advertisement by ministries using daily data between 2012 and 2013. I interact this variable of interest with a dummy 'after' that takes the value of one after 1 July 2012 and plot the coefficients on this interaction term. Panel A shows coefficients when using the number of articles as the dependent variable. Panel B shows coefficients when using an indicator variable of coverage as the dependent variable.

To provide suggestive evidence that the two-way favor exchange arises only when regulations prevent advertisements from having a direct effect, I use daily data on advertisement and coverage decisions before and after reform (from January 1, 2012, to December 13, 2013; the end of the legislative period). Specifically, I estimate the

same specifications as in Columns (4) and (5) of Table 1.3 (a variant of Equation 1.2), where I interact the advertisement dummy with a variable that equals one after the law came into effect.

Figure 1.4 presents the results. The left column uses the number of articles as the dependent variable, the right column uses a dummy indicating whether a newspaper reported on the respective minister as the dependent variable. Regardless of the specification, the results show that advertisements only significantly increase coverage *after* the law came into effect. The coefficients of the advertisement variable post-reform are positive and statistically significant, while those pre-reform are substantially smaller and not statistically significant. This suggests that, prior to the reform, coverage favors were not necessary because advertisements could be directly used for campaigning. After the reform, advertisements appear to serve as a tool for obtaining coverage favors. This strongly suggests that the primary mechanism driving the results is an advertiser effect.

1.5 Advertising, Media Content, and Households' Beliefs

Media content can shape individuals' political perceptions, creating strong incentives for political actors to influence coverage. However, the extent to which political actors effectively shape public perceptions through media capture remains an open question. Establishing causal effects with observational data is challenging due to the equilibrium nature of media bias. Exogenous variation in either supply- or demand-side factors is necessary to derive causal estimates.

My empirical strategy, combined with two key features of the high-frequency panel structure of the ACPP household survey data, allows to overcome this obstacle. First, the data facilitates the construction of a detailed household *exposure* variable to newspaper content. Second, the panel structure allows for the use of household fixed effects. Under the reasonable and testable assumption that individuals' media demand does not vary systematically over short time periods, individual fixed effects allow to abstract from changes in demand for media content. This alleviates concerns that voters endogenously respond to partisan bias in newspapers (Durante and Knight, 2012). Moreover, as advertisements by ministries cannot be used explicitly for campaigning, it is less likely that such advertisements directly arouse political interest (Canen and Martin, 2023). By holding the demand component for media constant, this approach

isolates the effect of exogenous variation in the supply component driven by government advertising.

1.5.1 Setting and Variables

Setting. Waves 1 to 27 of the ACPP span the period from March 2020 to December 2021, coinciding with the tenure of the Kurz II government. During this time, the ÖVP and the Greens formed a coalition government. Since the survey questions focus on parties rather than individual politicians, I aggregate both media coverage and advertisement data to the party level.

Media exposure. The survey regularly asks for households' consumption of individual newspapers over the previous seven days.[16] Responses are recorded on a five-point scale: One (Several times a day), two (Once a day), three (Several times a week), four (Once a week), and five (Not at all/never). Households are classified as being exposed to a specific newspaper within the last seven days if they report consumption levels corresponding to answer items one to three, and as not exposed if they select items four or five. For each household and survey wave, I calculate the number of articles to which they were 'exposed' during the previous seven days. Notably, due to variation in survey dates, even households that read the same newspaper may have been exposed to a different number of articles *within* the same wave. Using the same approach, I construct a measure of households' exposure to advertisements by ministries during the same period.

Voting Intentions. An individual's voting intention, as a discrete choice among political parties, tends to be relatively stable, offering limited variation. However, the ACPP survey regularly asks households to rate, on a scale from one (not likely at all) to ten (very likely), how likely they are to vote for individual parties. This intensive measure of voting intentions provides much more variation that can be exploited. Figure 1.A.7 demonstrates that changes on the intensive margin are a good predictor of changes on the extensive margin: For voters who change their (extensive) party preference, the gap in the intensive vote intentions between the previously and newly preferred party steadily decreases in the periods before the change, is close to zero in the period of the change, and becomes negative after the change. Intensive margin changes therefore accurately track voters' ideological alignment with parties and map

[16] The survey does not ask about consumption of the newspaper 'Kleine Zeitung'. I therefore restrict the analysis to the other four newspapers in my sample.

into discrete party choices. I use the questions on the likelihood of voting for the ÖVP and the Greens as the dependent variables.

Interpolation. Both the question on media consumption (asked every fourth or fifth wave) and on voting intentions (asked every second or third wave), are posed at regular intervals. Since these questions are often asked in non-overlapping surveys, I interpolate the values for both questions. This results in a total of 31,439 individual observations to be exploited for estimation.

1.5.2 Empirical Strategy

To shed light on the question of how effective governments are in shaping public perceptions via media capture, I link the FOCUS data on advertising and the data on media coverage to the household survey data. Specifically, I estimate the following equation:

$$Y_{iwo} = \beta_o * Coverage^{ex.}_{iwo} + \beta_{\neg o} * Coverage^{ex.}_{iw\neg o} + \phi_i + \mu_{m/w} + \epsilon_{iwo} \tag{1.5}$$

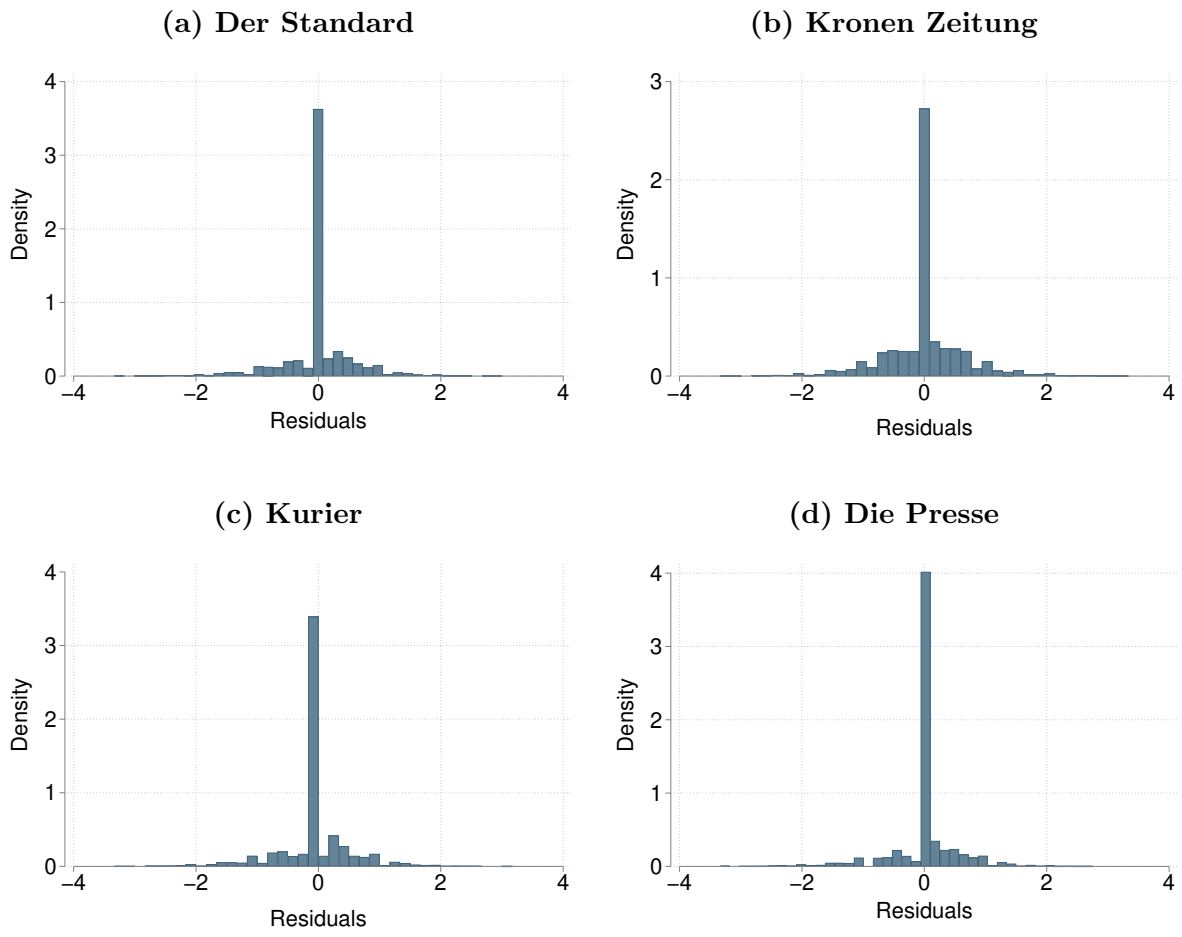
where Y_{iwo} is individual's i voting intention for party o in wave w, and $Coverage^{ex.}_{iwo}$ is the number of articles about party o that individual i has been exposed to (across all newspapers) over the last seven days. Crucially, the individual fixed effects (ϕ_i) absorb time-invariant socio-economic characteristics, but also largely hold individual demand across newspapers constant. In addition, time-period, $\mu_{m/w}$, – either month-of-year or wave – fixed effects control for common shocks to all individuals and newspapers. Finally, I also control for the coverage of the other respective political party ($Coverage^{ex.}_{iw\neg o}$) due to potential spillovers in coverage. Standard errors are clustered at the household level. All regressions use weights accounting for demographic *and* political sampling to ensure representativeness.

As the coverage that individuals are exposed to results from endogenous newspaper consumption decisions, I estimate Equation 1.5 using a 2SLS-design, where I instrument $Coverage^{ex.}_{iwo}$ with $Advertising^{ex.}_{iwo}$ (the number of advertisements by party o across exposed newspapers over the last seven days).

Stability of newspaper consumption. A key argument is that the individual fixed effects control for individual demand across newspapers. This requires demand for newspapers across outlets to be (relatively) time-invariant. While this assumption will

never be perfectly fulfilled, I assess its plausibility by regressing individual newspaper consumption (on the one to five scale) on individual fixed effects. Figure 1.5 shows histograms of the corresponding residuals for the four newspapers. Values close to zero represent stable demand for individual newspapers, while deviations from zero reflect changes in individual demand for newspapers over time. Despite *some* deviations, the figures show that most residuals are near zero. As a first-order approximation, the household fixed effects effectively hold individual demand for newspapers constant.

Figure 1.5: EVIDENCE FOR STABLE INDIVIDUAL NEWSPAPER CONSUMPTION



Notes: The figure shows histograms of residuals obtained by regressing the media consumption variable (measured on a one to five scale) for the four newspapers in the ACPP on household fixed effects. Residuals close to zero indicate that household demand for newspapers remains constant over time.

Validity & Exclusion restriction: The validity and relevance of the design depend on two assumptions:

(i) Advertisements by ministries are a relevant predictor of political reporting, influencing newspapers' coverage decisions (*relevance*).

(ii) Advertisements by ministries have no effect on individuals' perceptions other than through the effect on media content (*exogeneity*).

The first assumption can be tested, building on prior findings regarding the relationship between advertisements and coverage. It is unlikely that the second assumption is violated via an (unobserved) variable that correlates both with ministerial payments (the instrument) and households' voting intentions (the outcome) conditional on fixed effects. However, the second assumption could be violated if advertisements by ministries have a direct impact on individuals' perceptions. Although this cannot be formally tested, three arguments alleviate concerns that the exclusion restriction fails. First, advertisements by ministries cannot be used for campaigning and must remain strictly factual. Second, there is no systematic correlation between the content of the advertisements and the editorial content. Finally, many advertising campaigns by ministries span several weeks, repeatedly exposing individuals to the same advertisements – it is therefore unlikely that the advertisements directly give rise to changes in political preferences when using high-frequency data.

1.5.3 Results

Columns 1 and 2 of Table 1.5 show the results. Panel A demonstrates that the instrument is a relevant predictor of media coverage, with strong F-values in the first stage – confirming the findings from Section 1.4.2. Panel B reports the second stage results. Increased media coverage of a party *increases* the likelihood of voting for that party. This effect is statistically significant for both the Greens and the ÖVP.[17] Reassuringly, the coverage of the opposing party shows the opposite sign in all regressions, further reinforcing the credibility of the results.

In numerical terms, the parameter estimate for the ÖVP implies that a one standard-deviation increase in weekly exposure to coverage of the party (91 articles) raises the

[17]The effect is significantly stronger for the Greens, but the estimation also shows a significantly lower F-value. The Greens carried out significantly fewer advertisements than the ÖVP. The second-stage coefficient estimate for the Greens is therefore identified of a limited variation, which may explain the rather large estimate.

likelihood of voting for the party by 0.55 points on the one to ten scale (15 percent of a standard deviation). This represents a sizable effect. A back of the envelope calculation using the first-stage results implies that the party would need to spend approximately 1.35 million Euros to achieve that treatment effect (45 ads × 30,000 Euros/ad). To achieve the same effect, the Greens would need to spend 500,000 Euros.

Table 1.5: MEDIA AND VOTING INTENTIONS — INSTRUMENTAL VARIABLE RESULTS

Dependent variable: Voting intentions					
	Greens	ÖVP	Low ÖVP support	Mid ÖVP support	High ÖVP support
	Panel A: First stage				
Advertisement	0.465***	2.025***	2.576***	1.775***	1.444***
	(0.144)	(0.088)	(0.173)	(0.235)	(0.140)
	Panel B: Second stage				
Coverage	0.071**	0.006***	-0.002*	-0.001	0.008***
	(0.035)	(0.002)	(0.001)	(0.003)	(0.002)
Fixed effects:					
Individual FE	Yes	Yes	Yes	Yes	Yes
Month-of-year FE	Yes	Yes	Yes	Yes	Yes
F-value	10.38	533	222	56	106
Observations	31,439	31,265	14,848	6,380	9,428
Mean of dep. var	3.35	4.21	0.84	5.21	8.7

Notes: The table shows regression results on the relationship between media coverage and citizens voting intentions, empirically estimating Equation 1.5. Ministerial coverage is instrumented with advertisements by ministries. Column (1) shows results for the Greens, Column (2) shows results for the ÖVP, and Columns (3) to (5) show results for different ranges of ÖVP baseline support. Low support: Voting intentions < 4; mid-support: >= 4 & < 7; and high support: >= 7. Standard errors are clustered at the household level. All regressions use weights accounting for demographic and political sampling.

Robustness and further results. The effects are robust to controlling for party advertisements (see Panel A of Appendix Table 1.B.9) or using more restrictive wave fixed effects instead of month fixed effects (see Panel B of Appendix Table 1.B.9). Inferences are less robust for the Greens – in line with the limited available variation for the Greens.

I do not find any evidence of cross-party effects, i.e. instrumenting party's X coverage but looking at voting intention for party Y (and vice versa) yields insignificant parameter estimates (see Panel C of Appendix Table 1.B.9).

Shortening the 'exposure window' to include only articles and advertisements from the three days preceding households' survey responses yields a parameter estimate that is twice as large as the baseline estimate for the ÖVP (coefficient: 0.012, $t = 2.34$), and renders the parameter estimate insignificant for the Greens. This is consistent with exposure effects decaying over time.

Mechanism: Political perceptions. To examine the mechanism through which media coverage influences voting decision, I replace the dependent variable in Equation 1.5 with households' perceptions of the parties in government. The results, using the same 2-SLS estimation, suggest that the effect of media coverage on voting intentions operates through an increase in perceived competency in government leadership (ÖVP: coefficient 0.002, $t = 1.48$; Greens: coefficient 0.040, $t = 2.36$).

A second potential mechanism through which advertisement-induced coverage can influence citizens' perceptions and voting intentions is through changes in the perceived importance of issues ('priming'). This occurs when the increased salience of certain topics leads to shifts in political preferences. Framing and increases in the salience of topics can significantly impact readers' beliefs (Entman, 1989). While direct priming effects from advertisements are unlikely due to the absence of credible complementarities between the topics of ads and the articles, coverage could plausibly influence political perceptions via a learning and adoption mechanism. In this scenario, the increased salience of political coverage enhances awareness of party positions. More informed readers then tend to adopt these positions, which, in turn, increases the likelihood of voting for the party (Lenz, 2009).

Polarizing effects. The estimates for the parties in Columns 1 and 2 of Table 1.5 reflect the average effect across the entire distribution of households. However, an important question remains: Does media content have a differential impact on voting intentions based on individuals' baseline levels of party support? Given the high predictive power of the instrument for the ÖVP, I can examine whether the effect of media content on voting intentions is linear, or if it varies across different levels of initial party support.

Columns 3 to 5 of Table 1.5 present the results from estimating Equation 1.5 (via the 2-SLS design) for individuals with different levels of baseline support for the ÖVP. The findings suggest that media content may have a polarizing effect: Increased coverage of the ÖVP significantly *decreases* support for the party among individuals with low baseline support (Column 1), has no effect on individuals with intermediate sup-

port (Column 2), but significantly *increases* the likelihood of voting for the party among individuals with high baseline support (Column 3). These results collectively demonstrate that media coverage can exert sizable, polarizing effects on political voting intentions, with government financing of the media contributing to this impact.

1.6 Conclusion

Political actors have strong incentives to influence media coverage. If they successfully capture media outlets, they can shape public opinion and gain support for their policies or political objectives. In the same vein, influencing media coverage can boost the personal reputation of politicians and help to control the narrative around a particular policy issue by ensuring that their message is the dominant one in the public discourse ('salience'). It may also be used to discredit opponents or deflect from criticism to gain an advantage in the political arena.

Public media capture is most likely to occur in authoritarian regimes, where media outlets are frequently controlled by state actors. Evidence of media capture and of its consequences in Western democracies remains limited. In this paper, I examine the relationship between government advertising, media content, and citizens' political perceptions in Austria. I employ a two-step empirical strategy to examine how government advertisements influence media coverage and, in turn, how media coverage influences citizens' political attitudes. My research setting is unique, as it combines highly discretionary payments from ministries to media outlets with high-frequency household panel data.

I provide evidence for a two-way favor exchange between ministries and newspapers. First, I demonstrate that advertisements by ministries increase newspaper coverage of corresponding ministers. Using natural language processing techniques, I also demonstrate that greater levels of advertising crowd in positive reporting about a minister; highlighting how advertising influences the tone of reporting as well. The increase in coverage goes beyond the ideological proximity between ministries and newspapers, and even operates within newspaper-ministry and newspaper-minister pairs. The effect of advertisements on coverage operates on a high-frequency, with consistent results across both aggregated, long-run and fine-grained daily data. The increase in coverage is more pronounced when newspapers are more reliant on public funds. An important finding is that advertising increases coverage only during 'good times'. When ministers face a political scandal, advertisements reduce coverage. This asymmetric response to advertising depending on the state of the world provides evidence for a direct advertiser

effect and rules out alternative market-based explanations. For identification, I use a shift-share instrumental variable design to tackle the potential simultaneity between advertisements and political reporting.

In the next step, I examine the effectiveness of public media capture. I combine the data on media content and advertisements with panel data on households' media consumption and their voting intentions. I demonstrate that exposure to biased media content can shift individuals' beliefs. Leveraging variation in the supply component of media bias that arises from government advertising, while controlling for the demand component via individual fixed effects, I derive estimates for the 'advertising price of a vote'. Media coverage has a sizable, but polarizing, effect on individuals' voting intensions – and media financing by governments contributes to this effect.

My results have important implications for the role of mass media in democratic societies. They suggest that a high level of direct media funding, coupled with considerable discretion in its allocation, can lead to substantial media bias. Overall, the results highlight the need for regulations that ensure an equitable distribution of state advertising contracts among media outlets in order to safeguard the editorial independence of media companies and protect democratic accountability.

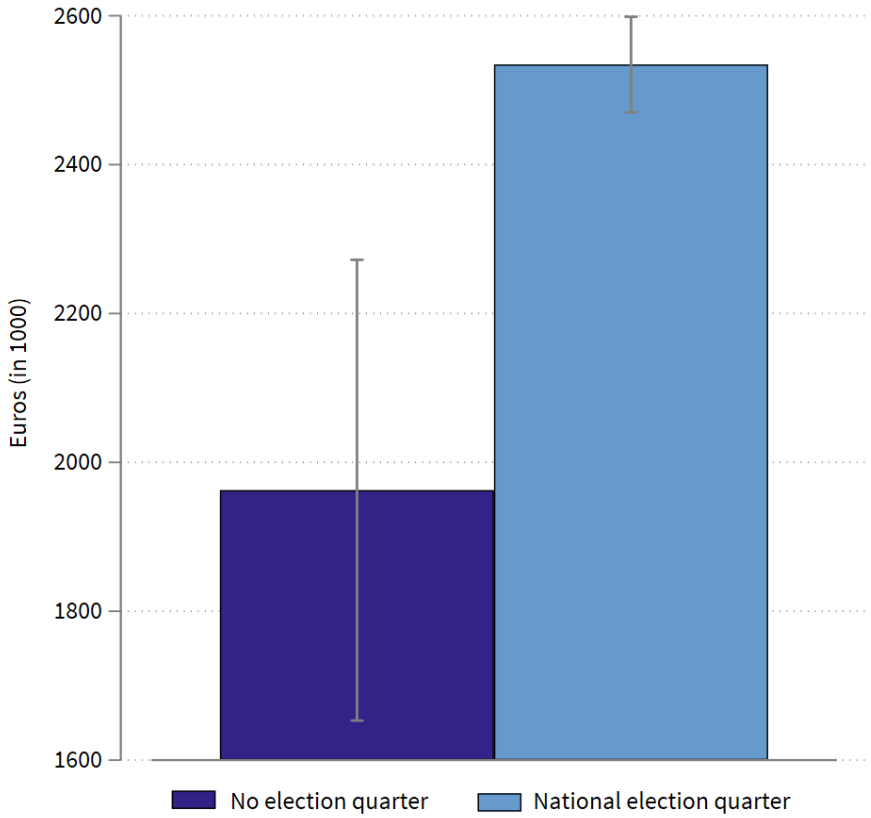
Appendix to Chapter 1

This appendix presents details on methods and additional results:

- Section 1.A provides supplementary figures.
- Section 1.B provides supplementary table results.
- Section 1.C provides further details on the methods.

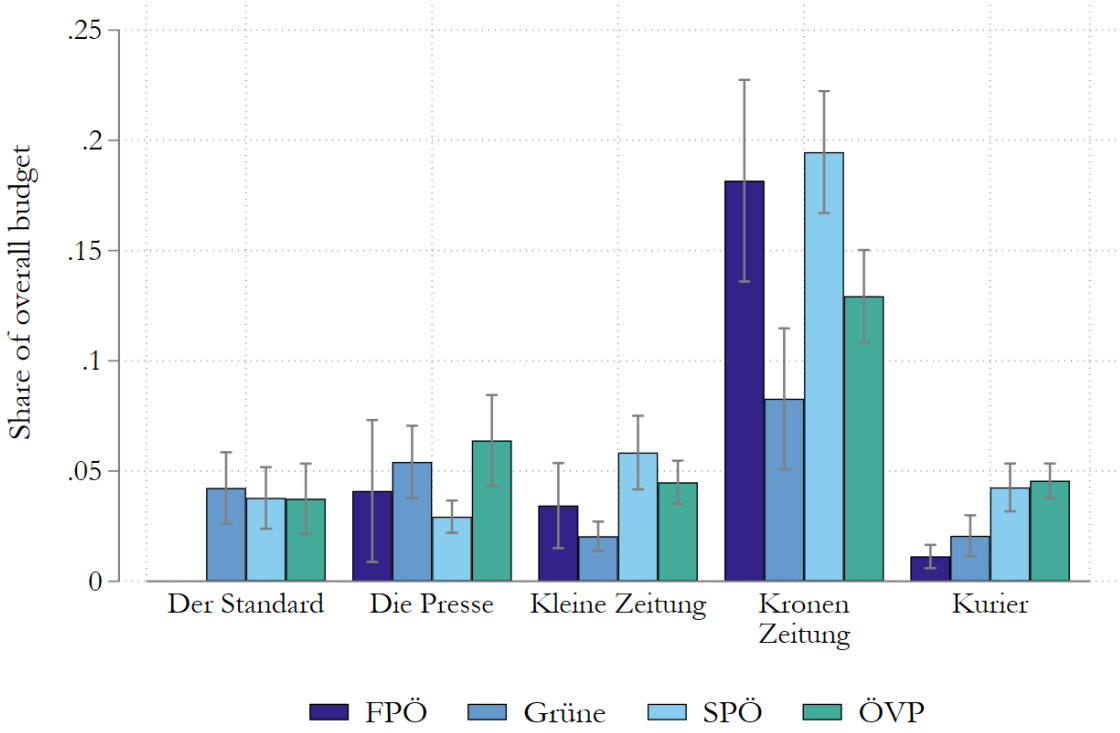
1.A Supplementary Figures

Figure 1.A.1: ADVERTISEMENTS AND NATIONAL ELECTIONS



Notes: The figure shows the expenditure of Austrian ministries on newspaper advertisements, comparing quarters with national elections to quarters without.

Figure 1.A.2: SPENDING ALLOCATIONS OF PARTIES ACROSS NEWSPAPERS



Notes: The figure shows the percentage of the total media budget allocated by ministers from various parties to individual newspapers between 2012 and 2021.

Figure 1.A.3: WORD CLOUDS OF THE SUB-CORPORA ARTICLES

(a) Ministry of Education

(b) Ministry of Defence

(c) Ministry of Agriculture

(d) Ministry of Transport

(e) Chancellor's Office

(f) Ministry of Foreign Affairs

(g) Ministry of Econ. Affairs

(h) Ministry of Public Service

(i) Ministry of the Interior

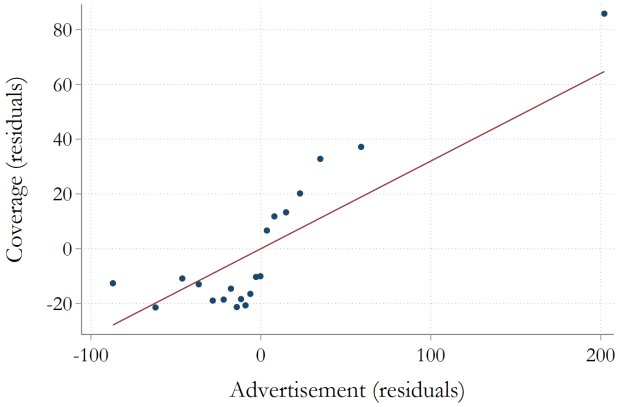
(j) Ministry of Labor and Health

(k) Ministry of Finance

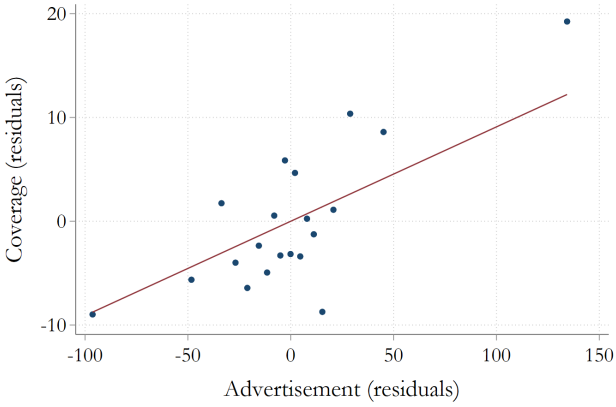
Notes: Each subfigure displays the 50 most common words within the corpus mentioning ministers of the same ministry. I exclude the names of the ministers and a few custom stopwords without substantial meaning (such as 'prozent', 'jahr', or 'uhr').

Figure 1.A.4: VALUE ADDED PLOTS OF THE BASELINE ESTIMATES

(a) *Within newspaper* variation



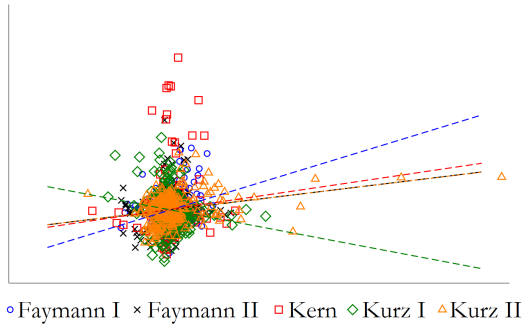
(b) *Within newspaper-ministry* variation



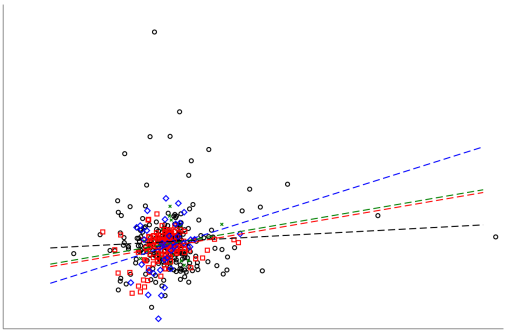
Notes: The figure shows binned scatterplots visualizing the within newspaper (Panel a) and within newspapers-ministry (Panel b) relationship between the residualized advertising and residualized article coverage variable.

Figure 1.A.5: ADVERTISEMENTS AND COVERAGE — HETEROGENEITY

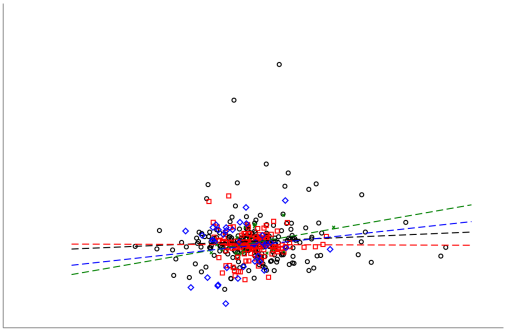
(a) Legislative periods



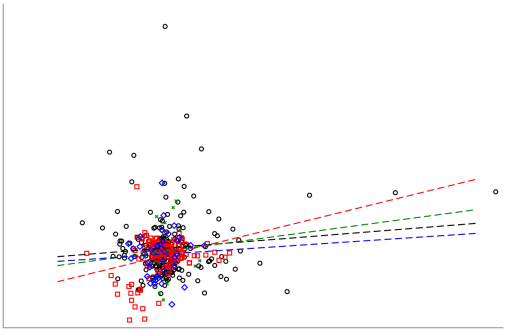
(b) Kleine Zeitung



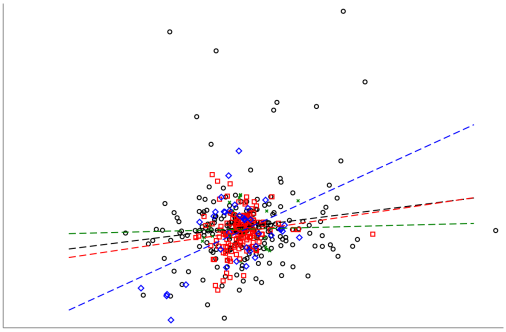
(c) Kurier



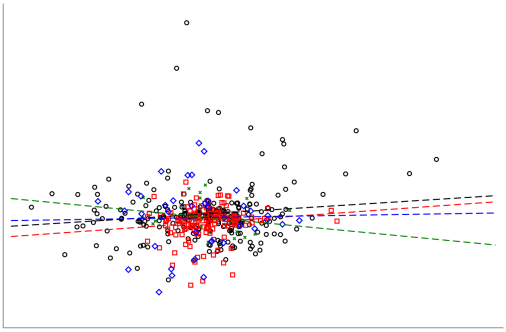
(d) Kronen Zeitung



(e) Die Presse

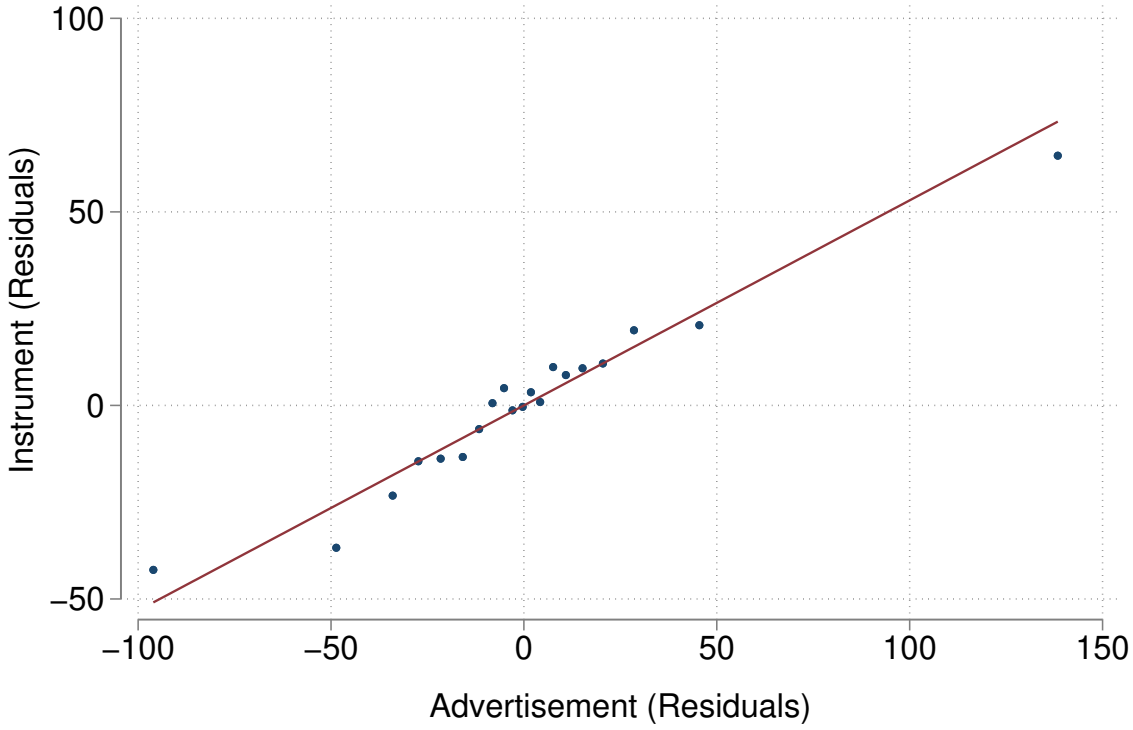


(f) Der Standard



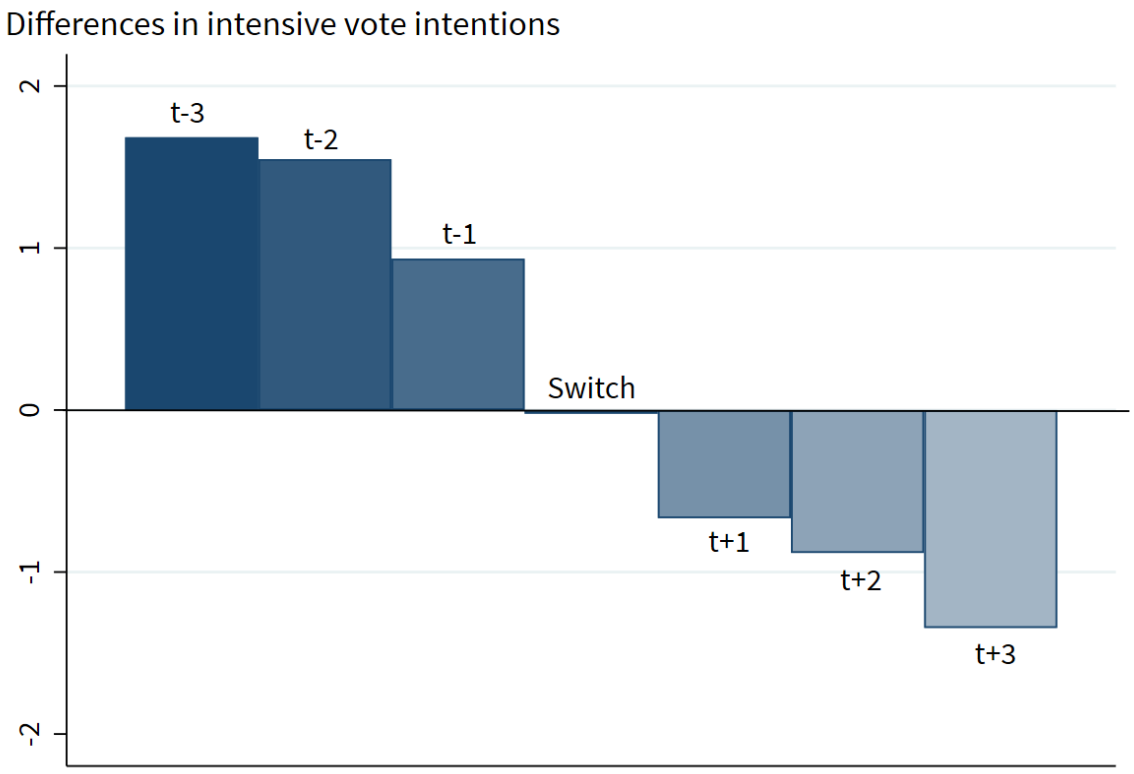
Notes: The figure shows the residuals of the article coverage variable (on the y-axis) and the advertisement payments (on the x-axis) after partialling out quarter and newspaper-ministry interaction fixed effects by legislative period (Panel a) and by individual newspaper (Panel b – f).

Figure 1.A.6: VALUE ADDED PLOT — IV: FIRST STAGE



Notes: The figure shows a binned scatterplot, visualizing the within newspaper-ministry relationship between the residualized advertising and the residualized instrument.

Figure 1.A.7: INTENSIVE VOTING INTENTIONS AND DISCRETE PARTY PREFERENCES



Notes: The figure illustrates the average development of the gap in voting intentions on the intensive margin for voters who change their party preference on the extensive margin. The difference in voting intentions is calculated as the difference between the voting intention for the previously preferred party and the newly preferred party.

1.B Supplementary Tables

Table 1.B.1: DAILY REGRESSION RESULTS 2020–2021 — TIMING

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Advertising	0.0100***	0.0100***	0.0100***	0.0101***	0.0101***	0.0102***	0.0100***
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)
$\text{Advertising}_{t_{-1}}$	-0.0035**						
	(0.0016)						
$\text{Advertising}_{t_{-1}-t_{-2}}$		-0.0015					
		(0.0011)					
$\text{Advertising}_{t_{-1}-t_{-3}}$			-0.0003				
			(0.0008)				
$\text{Advertising}_{t_{-1}-t_{-4}}$				-0.0002			
				(0.0007)			
$\text{Advertising}_{t_{-1}-t_{-5}}$					-0.0001		
					(0.0006)		
$\text{Advertising}_{t_{-1}-t_{-6}}$						-0.0002	
						(0.0005)	
$\text{Advertising}_{t_{-1}-t_{-7}}$							0.0005
							(0.0005)
Fixed effects:							
Day	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Minister x News.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,925	41,850	41,775	41,700	41,625	41,550	41,475

Notes: The table shows results on the timing between advertisements by ministries and coverage about ministers on the daily level from 2020 to 2021, by including (up to seven) lagged aggregated advertisements values. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Table 1.B.2: WEEKLY REGRESSION RESULTS 2020–2021 — TIMING

	(1)	(2)	(3)	(4)
Advertising	0.0142**	0.0145**	0.0153**	0.0149**
	(0.0062)	(0.0062)	(0.0063)	(0.0063)
$\text{Advertising}_{t_{-1}}$	0.0041			
	(0.0054)			
$\text{Advertising}_{t_{-1}-t_{-2}}$		0.0042		
		(0.0037)		
$\text{Advertising}_{t_{-1}-t_{-3}}$			0.0033	
			(0.0028)	
$\text{Advertising}_{t_{-1}-t_{-4}}$				0.0031
				(0.0024)
Fixed effects:				
Week	Yes	Yes	Yes	Yes
Minister x News.	Yes	Yes	Yes	Yes
Observations	5,925	5,850	5,775	5,700

Notes: The table shows results on the timing between advertisements by ministries and coverage about ministers on the weekly level from 2020 to 2021, by including lagged aggregated advertisements values. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Table 1.B.3: MONTHLY REGRESSION RESULTS 2020–2021 — TIMING

	(1)	(2)	(3)
Advertising	0.0241*	0.0318**	0.0316**
	(0.0133)	(0.0132)	(0.0127)
$\text{Advertising}_{t_{-1}}$	0.0093		
	(0.0112)		
$\text{Advertising}_{t_{-1}-t_{-2}}$		0.0109	
		(0.0080)	
$\text{Advertising}_{t_{-1}-t_{-3}}$			0.0137**
			(0.0066)
Fixed effects:			
Month	Yes	Yes	Yes
Minister x News.	Yes	Yes	Yes
Observations	1,305	1,230	1,160

Notes: The table shows results on the timing between advertisements by ministries and coverage about ministers on the monthly level from 2020 to 2021, by including lagged aggregated advertisements values. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Table 1.B.4: QUARTERLY REGRESSION RESULTS 2012–2021 — CONTROLLING FOR PARTY ADVERTISEMENTS

Dependent variable: Coverage (# of articles)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Advertising	0.320***	0.092***	0.016	0.050*	0.077***	0.060**	0.069***
	(0.042)	(0.022)	(0.033)	(0.025)	(0.024)	(0.025)	(0.025)
Party advert.	0.001	0.024	0.029	0.027	-0.005	0.027**	0.025**
	(0.032)	(0.022)	(0.022)	(0.019)	(0.034)	(0.011)	(0.011)
Advertising × Dependency			0.058**				
			(0.024)				
Advertising × Scandal							-0.066*
							(0.039)
Fixed effects:							
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Newspaper	Yes	-	-	-	-	-	-
Ministry x News.		Yes	Yes	-	Yes	Yes	Yes
Minister x News.				Yes	-	-	-
Newspaper x Quarter					Yes	-	-
Ministry x Quarter						Yes	Yes
Observations	2,165	2,165	2,165	2,165	2,165	2,165	2,165

Notes: The table shows regression results on the relationship between advertisements by ministries, party advertisements, and coverage about ministers on the quarterly level, empirically estimating equation (1.1) using data from 2012 to 2021. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Table 1.B.5: QUARTERLY REGRESSION RESULTS 2012–2021 — ROBUSTNESS

	(1) Auto-correlation	(2) Party level	(3) Time trend	(4) Stand. errors	(5) News. charact.	(6) # of mentions	(7) Page posit.
Advertising	0.050***	0.127***	0.095***	0.091***	0.093***	0.182***	0.001
	(0.019)	(0.048)	(0.023)	(0.029)	(0.024)	(0.057)	(0.001)
Fixed effects:							
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ministry x News.	Yes		Yes	Yes	Yes	Yes	Yes
Ministry x News. x Trend			Yes				
Party x News.		Yes					
$Coverage_{t-1}$	Yes						
Observations	2,020	365	2,165	2,165	2,165	2,165	1,731

Notes: The table shows regression results on the relationship between advertisements by ministries and coverage of ministers on the quarterly level, empirically estimating equation (1.1) using data from 2012 to 2021. Column 1 includes a lagged outcome variable to assess the degree of autocorrelation, Column 2 present results when aggregating the data to the party level, Column 3 interacts the ministry-newspaper pairs with a linear time trend, Column 4 clusters standard errors at the ministry-newspaper level instead of using Newey–West standard errors, Column 5 controls for newspaper time-variant characteristics, Column 6 uses the number of mentions as the dependent variable, and Column 7 uses the average page positioning of articles as the dependent variable.

Table 1.B.6: REGRESSION RESULTS — ADVERTISEMENTS AND MINISTRY TOPIC SHARES

Dependent variable: Topic share$_{mnt}$ (in %)			
	(1) Quarterly	(2) Quarterly	(3) Daily
Advertising	0.001***	0.001***	
	(0.0003)	(0.0003)	
Advertising ×		-0.001	
Scandal		(0.001)	
Has ad			0.441***
			(0.138)
Fixed effects:			
Ministry x Newspaper	Yes	Yes	Yes
Ministry x Quarter	Yes	Yes	-
Ministry x Day	-	-	Yes
Observations	2,165	2,165	42,000

Notes: The table shows regression results on the relationship between advertisements by ministries and corresponding covered topic shares in newspapers. In Column 1, I use quarterly data from 2012 to 2021. In Column 2, I use daily data from 2020 to 2021. In parentheses, I report Newey–West standard errors with one lag to allow for autocorrelation and heteroskedasticity.

Table 1.B.7: ADVERTISEMENTS AND COVERAGE — DECOMPOSITION OF THE IV-OLS GAP

	Coefficients			Decomposition		
	OLS	IV	IV-OLS	Δ_{cw}	Δ_{tw}	Δ_{me}
Advertising	0.087	0.140	0.052	-0.132	-0.013	0.198
	(0.021)	(0.063)	(0.056)	(0.033)	(0.003)	(0.066)

Notes: The first column reports the OLS estimate, the second column reports the IV estimate, and the third column reports the OLS-IV gap. The next three columns report the estimates of the covariate weight difference, the treatment-level weight difference, and the marginal effect difference components. By construction, these three components sum to the IV–OLS gap. For computational details on the decomposition see Ishimaru (2024).

Table 1.B.8: ADVERTISEMENTS AND COVERAGE — INSTRUMENTAL VARIABLE RESULTS WITH ALTERNATIVE SHARES

	Cross-sectional variation	*Ministry-newspaper pairs*		*Minister-newspaper pairs*	
	(1)	(2)	(3)	(4)	(5)
	Panel A: First stage				
Instrument	0.030***	0.026***	0.026***	0.035***	0.035***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
	Panel B: Second stage				
Advertising	0.947***	0.571***	0.564***	0.387***	0.388***
	(0.142)	(0.160)	(0.158)	(0.098)	(0.098)
F-value	49.6	28.6	28.5	29.8	29.7
Observations	2,045	2,045	2,045	2,045	2,045
Fixed effects:					
Quarter	Yes	Yes	Yes	Yes	Yes
Newspaper	Yes	Yes	Yes	Yes	Yes
Ministry x News.	-	Yes	Yes	-	-
Minister x News.	-	-	-	Yes	Yes
Scandal	-	-	Yes	-	Yes

Notes: The table shows regression results on the relationship between advertisements by ministries and coverage of ministers on the quarterly level, where Advertisements are instrumented with the shift-share variable. The shift-share IV is constructed using the alternative shares. Robust standard errors that are adjusted to arbitrary heteroskedasticity are reported in parentheses.

Table 1.B.9: MEDIA AND VOTING INTENTIONS — INSTRUMENTAL VARIABLE REGRESSION RESULTS ROBUSTNESS

Dependent variable: Voting intentions		
	(1) Greens	(2) ÖVP
	Panel A: Controlling for Party ads	
Coverage	0.025	0.006***
	(0.019)	(0.002)
	[0.199]	[0.007]
	Panel B: Including wave FEs	
Coverage	0.054	0.003
	(0.034)	(0.002)
	[0.116]	[0.106]
	Panel C: Cross-party effects	
Coverage$^{\neg o}$	0.042	0.001
	(0.034)	(0.002)
	[0.213]	[0.567]

Notes: The table shows regression results on the relationship between media coverage and citizens voting intentions, empirically estimating Equation 1.5. Panel A shows results when additionally controlling for party advertisements. Panel B shows results when using wave fixed effects instead of month fixed effects. Panel C shows cross-party effects instrumenting party's X coverage, but using voting intention for party Y as the dependent variable. All panels show second-stage coefficients. Coverage is instrumented with advertisements by ministries. Standard errors are clustered at the household level. Values in brackets denote p-values. All regressions use weights accounting for demographic and political sampling.

1.C Details on Methods

1.C.1 Political Scandals

I self-collect detailed information on political scandals of Austrian ministers between 2012 and 2021. I identify scandals of individual ministers in a series of steps. I first search for the name of the politician in Google News, and then add the keywords 'scandal' OR 'affair'. Next, I search for the name of the position (e.g., Minister of Education) and keywords 'scandal' OR 'affair'; and lastly for the name of the ministry (e.g., Ministry of Finance) and keywords 'scandal' OR 'affair'. I classify an event as a scandal when multiple sources report on it, when it originates from a breach of norms (versus a controversy), and has likely some direct impact on the ministers, i.e. it destroys some kind of political capital for the involved actors. In total, I classify 48 scandals; ranging from cases involving abuse of office or power, corruption, misuse of public funds, or nepotism. In later analyses, I exclude seven scandals which are directly related to media advertising. In those cases, political actors likely cannot use further advertising to reduce scandal coverage by media outlets. Including those scandals does, however, not change qualitative inferences. Table 1.C.1 provides short information on the individual scandals and involved actors.

Table 1.C.1: SCANDALS OF AUSTRIAN MINISTERS

Minister	**Time**	**Short Scandal Description**
Werner Faymann	2012–3	Incriminating findings in advertisement affair.
Nikolaus Berlakovich	2012–4	Investigation on suspicion of embezzlement and dubious advertising practices.
Maria Fekter	2013–3	Revelation of the sale of apartments through the Ministry of Interior during Fekter's tenure, selling social housing for 7,000 euros to an investor.
Andrä Rupprechter	2014–2	As the opening speaker at the Boku Ball, Rupprechter attacks the deputy chairman with xenophobic statements.
Reinhold Mitterlehner	2014–4	Report about an unusually high number of members of the Catholic student fraternity in Mitterlehner's ministry and very early promotions of fraternity members.
Gerald Klug	2015–2	Private use of an official vehicle, including a chauffeur.
Reinhold Mitterlehner	2015–3	Public funds being used for election advertising.
Sophie Karmasin	2015–3	Public funds being used for election advertising.

Rudolf Hundstorfer	2015–3	Suspicions of nepotism. The Minister promotes personnel due to questionable contract awards to the top of the labor agency.
Wolfgang Sobotka	2016–3	Announces the demolition of Hitler's birthplace. Heated protests and criticism follow.
Jörg Leichtfried	2016–4	Suspicion of abuse of office and cover-up in the Ministry of Transport. The Kurier newspaper publishes internal documents.
Wolfgang Sobotka	2017–1	Misuse of public funds; charters private jets for very short distances.
Sonja Hammerschmid	2017–1	The Minister of Education served as a proponent of the state development bank from 2003 to 2010, responsible for innovation and technology. Hammerschmid granted subsidies to companies that later went bankrupt. The Public Prosecutor's Office investigates.
Wolfgang Sobotka	2017–3	Report about instructions regarding a boating accident involving an acquaintance.
Christian Kern	2017–4	Silberstein affair: Adviser Tal Silberstein created dirty-campaigning Facebook pages that affected Kern's election campaign.
Christian Kern	2017–4	The SPÖ suspends advertisements to a tabloid newspaper, after they strongly criticized Kern ("Princess Kern" affair), leading to criticism that the SPÖ uses advertisements strategically.
Heinz-Christian Strache	2018–1	Accusation of attending far-right fraternity meetings. Intends to use the right-wing Academic Ball to make a statement.
Herbert Kickl	2018–1	BVT affair: Austrian constitutional protection agency issued blank North Korean passports to South Korean spies. Suspicion of partisan overstepping of competence, searches in various private homes by BVT (subordinate to the Ministry of Interior). Kickl faces allegations of abuse of office.
Herbert Kickl	2018–1	Use of phrases with Nazi connotations in the context of migration. Referring to refugees as "concentrated" in one place. This also causes significant international media attention.
Heinz-Christian Strache	2018–1	Personal attack on journalist via discrediting Facebook post against ORF moderator.
Norbert Hofer	2018–1	Nepotism: Hofer appoints individuals close to the Freedom Party (FPÖ) and, in some cases, significantly underqualified, to positions in the state-owned enterprise ÖBB.

Beate Hartinger-Klein	2018–3	The Minister withholds evaluation studies of the actions of the previous government.
Beate Hartinger-Klein	2018–3	Accusations of using grossly misleading numbers and significant calculation errors in calculations related to the new basic income.
Herbert Kickl	2018–3	A minister's associate poses as a journalist and infiltrates a press room reserved for journalists reporting on the BVT affair.
Herbert Kickl	2018–3	Kickl's spokesperson suggests excluding certain newspapers from police briefings.
Karin Kneissl	2018–3	Wedding dance with Putin and subsequent connections to Russia become public. In addition, it becomes public that the security costs for her wedding exceed 200,000 euros.
Herbert Kickl	2018–4	Publication of journalist correspondence to defame journalists. The press release is deleted after a week due to extensive criticism.
Beate Hartinger-Klein	2018–4	Appearance in a racist video promoted by the FPÖ party, which was ultimately deleted.
Norbert Hofer	2019–1	Nepotism: Hofer's flight instructor is appointed to a position at Austro Control.
Mario Kunasek	2019–2	Revelation of photos showing meetings with Identitarian Movement members.
Heinz-Christian Strache	2019–2	Ibiza affair: Corruption, attempt to circumvent party funding laws, establish control over non-partisan media.
Heinz-Christian Strache	2019–2	Claims that Strache has a deeper neonazi past than known, talks about incriminating evidence.
Sebastian Kurz	2019–3	Shredder affair: A staff member of Kurz, social media consultant, destroyed 5 hard drives from the Federal Chancellery.
Sebastian Kurz	2019–4	Casino affair: Agreements were made that led to the appointment of an underqualified individual to the executive board of the state-owned Casinos Austria. The affair became known in August 2019 through several house searches.
Gernot Blümel	2020–2	Questionable statement in Ibiza affair committee: claims to not have a work laptop. Evidence appears that this is a lie.
Karl Nehammer	2020–2	Renovation plans for Hitler's birthplace, moving a memorial stone to the House of History, leading to significant public criticism.
Elisabeth Köstinger	2020–3	High costs for COVID-19 tests and allegations of substantial payments to McKinsey.
Margarete Schramböck	2021–1	Spectacularly failed project by the minister at exorbitant costs becomes known ('Kaufhaus Österreich').

Gernot Blümel	2021–1	Casino affair: Agreements were made that led to the appointment of an underqualified individual to the executive board of the state-owned Casinos Austria. The affair became known in August 2019 through several house searches.
Gernot Blümel	2021–2	Incriminating chats related to strategic political appointments are made public.
Sebastian Kurz	2021–2	ÖVP boycotts the commemoration of the liberation of the Mauthausen concentration camp.
Sebastian Kurz	2021–2	Revelation of an investigation against Kurz in connection with corruption allegations. Suspected of making false statements in front of the Ibiza corruption committee.
Sebastian Kurz	2021–2	Incriminating chats related to strategic political appointments are made public.
Werner Kogler	2021–2	Nepotism: Kogler accused of granting positions to trusted individuals.
Elisabeth Köstinger	2021–4	Suspected unlawful party financing and image promotion of the Ministry of Agriculture.
Gernot Blümel	2021–4	Continued collaboration with a polling institute that produced manipulated surveys, leading to contract terminations after the revelations.
Martin Kocher	2021–4	In 2017, Kocher, then head of the Institute for Advanced Studies (IHS), sent an email to two high-ranking employees of the Ministry of Finance and the Ministry of Family. The Industrial Association (IV) promised the IHS research group Insight Austria an annual subsidy of 35,000 euros for three years. However, there was a condition: Clemens Wallner, then economic policy coordinator of IV, should become deputy head of Insight Austria.

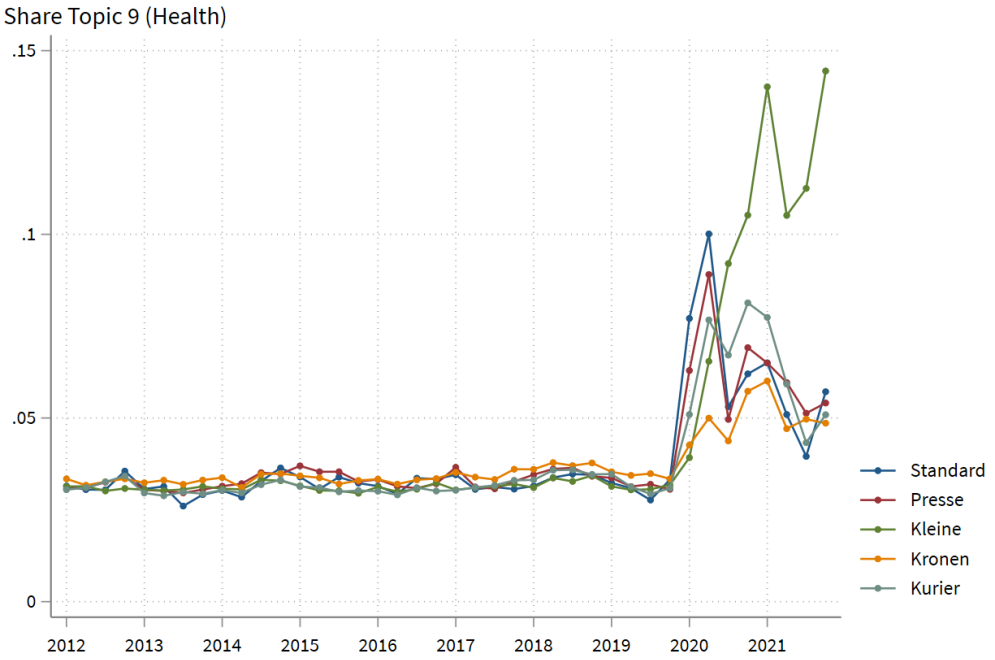
Notes: The table lists all self-collected scandals involving Austrian ministers between 2012 and 2021. Details on how the scandals were selected are described in Section 1.C.1.

1.C.2 Latent Dirichlet Allocation (LDA)

Latent Dirichlet Allocation is a probabilistic generative model that is commonly used to identify latent topics in a corpus of text. It is based on the idea that each document in a corpus can be represented as a mixture of latent topics, and that each topic is characterized by a distribution over words (see Blei et al., 2003).

I harmonize the category labels for each newspaper and filter articles that are classified as about the 'economy' or 'politics'. This yields a corpus of 323,723 articles. After employing part-of-speech tagging, removing stop words, and tokenizing the words, I trim the document-term matrix to select informative words and ease computational demand. Specifically, I keep tokens that are in the upper 50% of the frequency distribution *and* are observed in a maximum of 5 percent of all documents. I set the number of topics to $k = 25$ and use the remaining document-term matrix as input to train the model. The unsupervised LDA model yields informative words-over-topics distributions that allow to clearly assign labels to the topics (presented below).

Figure 1.C.1: LDA VALIDATION — HEALTH TOPIC SHARE OVER TIME



Notes: The figure plots the average quarterly predicted share of the 'Health' related topic (topic 9) over time for the five newspapers. The labeled tick for each year represents the first quarter.

I predict the topic distribution for each article and calculate the share (in percent of total political coverage in a given newspaper and time period) that each topic receives, weighting by article length. Three research assistants were then asked to map the topics to the corresponding ministries.

Validation. To further validate the model, I conduct a visual check. Figure 1.C.1 shows the distribution of the health-related topic (Topic nine) over time, highlighting a sharp increase in its share of coverage in the first quarter of 2020 across all newspapers — consistent with expectations following the outbreak of the COVID-19 pandemic in March 2020. Similar, intuitive patterns are observed for other identified topics.

Topic 1 [**ENERGY**]: projekt, energie, förderung, strom, öl, ausbau, gas, investition, anlage, industrie
Topic 2 [**SOCIETY**]: standard, leute, gesellschaft, idee, rolle, demokratie, meinung, bevölkerung, angst, system
Topic 3 [**JUSTICE**]: recht, regel, regeln, antrag, klage, behörde, rechnungshof, strafe, urteil, regelung
Topic 4 [**JUSTICE**]: ausschuß, staatsanwaltschaft, vorwurf, verfahren, ermittlung, prozeß, anwalt, justiz, verdacht, gericht
Topic 5 [**EDUCATION**]: schule, lehrer, schüler, eltern, studie, bildung, universität, basis, studium, jugendliche
Topic 6 [**ECONOMY/FINANCE**]: wachstum, inflation, eurozone, prognose, ökonom, plus, investition, konjunktur, rückgang, anstieg
Topic 7 [**DIGITALIZATION**]: datum, internet, plattform, online, anbieter, system, gerät, gründer, digitalisierung, netzwerk
Topic 8 [**TOURISM**]: haus, gemeinde, wohnung, gast, hotel, tourismus, sommer, region, österreicher, ort
Topic 9 [**HEALTH**]: arzt, pandemie, patient, gesundheit, situation, test, spital, pflege, hilfe, virus
Topic 10 [**CHANCELLOR**]: abgeordnete, strache, kern, nationalrat, bundeskanzler, minister, bundespräsident, koalition, vizekanzler, opposition
Topic 11 [**DEFENCE**]: sanktion, außenminister, konflikt, krieg, kampf, region, soldat, armee, westen, weste
Topic 12 [**ECONOMY/FINANCE**]: kredit, hypi, raiffeisen, schuld, anleihe, institut, gläubiger, insolvenz, pleite, höhe
Topic 13 [**LABOR**]: gewerkschaft, job, betrieb, stelle, stunden, wirtschaftskammer, arbeitsplatz, arbeitnehmer, arbeitsmarkt, beschäftigter

Topic 14 [**MEDIA**]: medium, tv, rede, journalist, zeitung, bild, spiel, wort, auftritt, interview
Topic 15 [**TRANSPORT**]: auto, flughafen, fahrzeug, zug, e, bahn, autos, modell, flugzeug, kilometer
Topic 16 [**OTHER**]: verhandlung, lösung, punkt, reform, einigung, forderung, druck, streit, finanzminister, treffen
Topic 17 [**INTERIOR**]: polizei, protest, opfer, straße, anschlag, gewalt, polizist, demonstrant, bewegung, angst
Topic 18 [**CAMPAIGNING**]: stimme, wahlkampf, wähler, kandidat, umfrage, stimmen, liste, mehrheit, koalition, grüne
Topic 19 [**AGRICULTURE**]: produkt, handel, bauer, händler, konsument, post, marke, lebensmittel, branche, betrieb
Topic 20 [**ECONOMY**]: gewinn, aktie, umsatz, standort, anteil, verkauf, tochter, investor, verlust, übernahme
Topic 21 [**FAMILY**]: familie, leben, geschichte, vater, kirche, buch, sohn, mutter, papst, freund
Topic 22 [**FINANCE**]: steuern, reform, pension, steuerreform, budget, bund, steuer, einkommen, mindestsicherung, einnahme
Topic 23 [**FOREIGN**]: kommission, regierungschef, gipfel, brexit, abkommen, abkomme, mehrheit, kanzlerin, vertrag, premier
Topic 24 [**ECONOMY**]: vorstand, aufsichtsrat, spitze, mitglied, manager, vertrag, nachfolger, posten, vorsitzende, stiftung
Topic 25 [**INTERIOR**]: flüchtling, grenze, migranten, innenminister, innenministerium, verteidigungsminister, asylwerber, sicherheit, migration, leitner

1.C.3 Sentiment Analysis

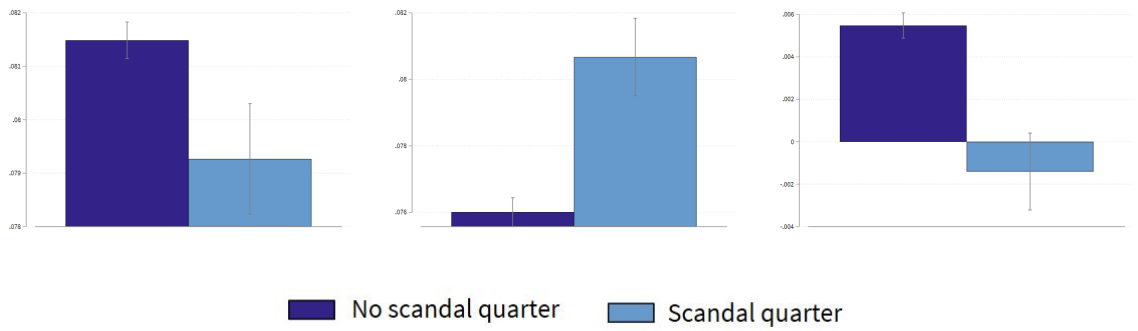
The *coverage* variable does not distinguish between different types of coverage, as it does not measure whether the articles speak favorable or unfavorable about an individual minister. Dictionary or lexical approaches are commonly used in economics to calculate sentiment scores from text data (see e.g., Shapiro and Wilson, 2022b). These approaches rely on provided scores of polarity for each word included in the dictionary. Those scores can hence be used to compute the overall sentiment of an article based on the occurrence of individual words, as well as negative and positive word occurrence separately. Previous research has shown that custom-made dictionaries outperform more general sentiment dictionaries (Shapiro et al., 2020). I therefore rely on a sentiment dictionaries specifically tailored for German political language (Rauh, 2018). The performance of the dictionary has been assessed against human coding on parliamentary speeches, party manifestos, and media coverage and has been shown to provide valid measurements of sentiment. I do not rely on more advanced ways of measuring sentiment because the underlying concept of sentiment can differ from the concept of stance against specific entities (Bestvater and Monroe, 2023). I hence focus on how articles are framed, particularly through the use of words with positive or negative connotations.

Once I have classified a sentiment score for each newspaper article, I calculate the average quarterly sentiment indicators for each newspaper-minister combination. These indicators include the share of positive and negative words, as well as the compound sentiment score, which is the difference between the share of positive and negative words. To compute these indicators, I weigh the sentiment scores of individual articles by their relative length, measured in terms of characters, within each quarter. This ensures that longer articles, which may provide more comprehensive coverage, have a greater influence on the overall sentiment measure.

Validation. Figure 1.C.2 provides a validation for the sentiment measure. When a minister experiences a scandal, the share of positive words in media coverage significantly decreases (Panel A), and the share of negative words significantly increases (Panel B). This shift in sentiment is also evident in the compound sentiment score (Panel C).

Figure 1.C.2: SENTIMENT VALIDATION — SENTIMENT SCORES AND SCANDALS

(a) Positive sentiment. **(b) Negative sentiment.** **(c) Compound sentiment.**



Notes: The figure shows the validation of the sentiment indicators by relating the occurrence of a political scandal within a ministry during a given quarter to three sentiment measures. Panel A depicts the positive sentiment score, Panel B illustrates the negative sentiment score, and Panel C shows the compound sentiment score.

1.C.4 Search strings

I employ Apache Lucene search strings to filter the media corpus. The Apache engine uses a Query 'lucene' Parser that allows for a sophisticated combination of search operators. It allows for fuzzy searches (discovering terms that are similar to a specified term without necessarily being an exact match), proximity searches (identifying terms that are within a specific distance from one another) and term boosting among many other options. It also allows to use various Boolean operators to combine parts of search strings. All the search strings are either taken directly from the AUTNES documentation reports or I apply the same logic and syntax when constructing new search queries. I also present the precision and recall values for the search strings (where available) from AUTNES.

Search strings for Faymann I:

Faymann: faymann* *[precision: 1, recall: 0.93]*
Spindelegger: spindelegger* OR spindi *[precision: 1, recall: 0.94]*
Hundstorfer: hundstorfer* *[precision: 1, recall: 1]*
Fekter: fekter* *[precision: 1, recall: 1]*
Heinisch-Hosek: heinisch-hosek OR "heinisch (gabriele OR frauenminister* OR sp OR spö OR beamtenminister* OR minister* OR hossek OR hosek)" 5 *[precision: 1, recall: 1]*
Stöger: "stöger* (alois OR gesundheitsminister* OR sp-* OR spö* OR bundesminister* OR minister* OR regierung*)" 5 NOT "peter stöger" *[precision: 1, recall: 1]*
Mikl-Leitner: mikl-leitner OR "mikl* (johanna OR innenministerin* OR bundesministerin* OR vp OR övp OR volkspartei OR vp-* OR *leitner)" 5 *[precision: 1, recall: 0.78]*
Karl: "karl (beatrix OR justizminister* OR minister* OR bundesminister*)" 5 *[precision: 1, recall: 0.75]*
Berlakovich: "nikolaus b" OR "berlakovich (nikolaus OR umweltminister* OR agrarminister* OR landwirtschaftsminister* OR minster* OR niki OR vp-* OR vp OR volkspartei OR övp* OR biene* OR pestizid* OR agrar* OR umwel* OR landwirtschaft* OR bauer*)" 5 *[precision: 0.96, recall: 1]*
Klug: "klug (gerald OR verteidigungsminister* OR minister* OR sportminister* OR sp OR spö)" 5 *[precision: 1, recall: 0.83]*
Schmied: "schmied (claudia OR bildungsminister* OR minister* OR kulturminister* OR unterrichtsminister* OR sp-* OR spö* OR gesamtschul*)" 5 *[precision: 1, recall: 1]*
Bures: bures NOT "bures radim" 5 *[precision: 1, recall: 1]*

Mitterlehner: "mitterlehner (reinhold OR wirtschaftsminister* OR minister* OR familienminister* OR vp-* OR vp OR övp* OR volkspartei OR energieminister*)" 5 *[precision: 1, recall: 1]*
Töchterle: "töchterle karl heinz" 5 OR "töchterle (wissenschaftsminister* OR bundesminister* OR minister* OR forschungsminister* OR övp* OR vp OR vp-* OR volkspartei OR karlheinz)" 5 *[precision: 1, recall: 0.84]*
Darabos: "darabos (norbert OR bundesminister* OR minister* OR verteidigungsminister* OR sp OR spö OR sp-*)" 5

Search strings for Faymann II: *[No AUTNES recall or precision values. The cabinet is, however, very similar to Faymann I. Hence, accuracy is likely to be very high.]*
Faymann: faymann*
Spindelegger: spindelegger* OR spindi
Mitterlehner: "mitterlehner (reinhold OR wirtschaftsminister* OR minister* OR forschungsminister* OR vp-* OR vp OR övp* OR volkspartei OR wissenschaftsminister* OR vizekanzler*)" 5
Mikl-Leitner: mikl-leitner OR "mikl* (johanna OR innenministerin* OR bundesministerin* OR vp OR övp OR volkspartei OR vp-* OR *leitner)" 5
Hundstorfer: hundstorfer*
Stöger: "stöger* (alois OR arbeit* OR sozial* OR konsumentenschutz* OR sp-* OR spö* OR bundesminister* OR minister* OR regierung* OR verkehr* OR technologie* OR innovation* OR gesundheitsminister*)" 5 NOT "peter stöger"
Klug: "klug (gerald OR verteidigungsminister* OR minister* OR sportminister* OR sp OR spö)" 5
Bures: bures NOT "bures radim" 5
Heinisch-Hosek: heinisch-hosek OR "heinisch (gabriele OR frauenminister* OR sp OR spö OR bildungsminister* OR minister* OR hossek OR hosek)" 5
Kurz: "sebastian kurz" 4 OR "kurz (övp OR volkspartei OR vp)" 5 OR "(minister OR bundesmin-ister* OR europa* OR integration* OR aussenminister* OR äusseres*) kurz" 5
Sobotka: "wolfgang sobotka" 4 OR "innenminister* sobotka" 5 OR ("minister* sobotka" 5 NOT tschechi)
Brandstetter: "wolfgang brandstetter" 4 OR "brandstetter minister*" 5 OR "(bundesminister* OR justiz*) brandstetter" 5
Schelling: "hans jörg schelling" OR "johann georg schelling" OR "hans jörg schelling" 4 OR "(minister* OR finanzminister*) schelling" 5

Karmasin: karmasin
Rupprechter: "andrä rupprechter" 4 OR "(minister* OR landwirtschaftsminister* OR umwelt-minister*) rupprechter" 5
Doskozil: doskozil
Oberhauser: "sabine oberhauser" 4 OR "(minister* OR bundesminister* OR gesundheitsminister*) oberhauser" 5

Search strings for Kern:
Kern: "kern christian" 4 OR "kern (spö OR kanzler OR sozialdemokrat* OR spitzenkandidat*)" 5 OR "bundeskanzler* kern" 5 *[precision: 0.99, recall: 0.85]*
Kurz: "sebastian kurz" 4 OR "kurz (övp OR volkspartei OR vp)" 5 OR "(minister OR bundesminister* OR europa* OR integration* OR aussenminister* OR äusseres*) kurz" 5 *[precision: 0.98, recall: 0.87]*
Stöger: "alois stöger" 4 OR "(minister* OR bundesminister* OR gesundheit* OR sozial* OR arbeit*) stöger" 5 *[precision: 1, recall: 1]*
Brandstetter: "wolfgang brandstetter" 4 OR "brandstetter minister*" 5 OR "(bundesminister* OR justiz*) brandstetter" 5
Doskozil: doskozil
Hammerschmid: "sonja hammerschmid" 4 OR "(minister* OR bundesminister* OR bildung*) hammerschmid" 5
Karmasin: karmasin
Leichtfried: "jörg leichtfried" 4 OR "leichtfried minister*" 5 OR "verkehrsminister* leicht-fried" 5
Mahrer: "harald mahrer" 4 OR "(minister* OR bundesminister* OR wirtschaft* OR wissenschaft* OR forschung* OR staatssekretär*) mahrer" 5
Rendi-Wagner: rendi-wagner OR "(minister* OR bundesminister* OR gesundheitsminister* OR frauenminister* OR pamela* OR staatssekretär* OR rendi) wagner" 5
Rupprechter: "andrä rupprechter" 4 OR "(minister* OR landwirtschaftsminister* OR umwelt-minister*) rupprechter" 5
Schelling: "hans jörg schelling" OR "johann georg schelling" OR "hans jörg schelling" 4 OR "(minister* OR finanzminister*) schelling" 5
Sobotka: "wolfgang sobotka" 4 OR "innenminister* sobotka" 5 OR ("minister* sobotka" 5 NOT tschechi)
Mitterlehner: "mitterlehner (reinhold OR wirtschaftsminister* OR minister* OR forschungsminister* OR vp-* OR vp OR övp* OR volkspartei OR wissenschaftsminister* OR vizekanzler*)" 5
Oberhauser: "sabine oberhauser" 4 OR "(minister* OR bundesminister* OR gesund-

heitsminister* OR frauenminister*) oberhauser" 5

Search strings for Kurz I:

Kurz: "sebastian kurz" 4 OR "kurz (kanzler OR bundeskanzler OR övp OR vp OR volkspartei OR parteiobmann OR spitzenkandidat)" 5 *[precision: 0.94, recall: 0.91]*

Hofer: ("hofer* (fpö OR freiheitlich* OR chef OR spitzenkandidat* OR partei OR kickl OR strache OR rendi OR Reisinger OR kurz OR pilz OR fpö OR spö OR övp OR neos OR wahl OR wahlkampf OR duell)" 5 NOT "homas hofer") OR "norbert hofer*" *[precision: 0.96, recall: 0.93]*

Strache: strache *[precision: 0.99, recall: 0.99]*

Löger: "(hartwig OR vizekanzler* OR minister* OR bundesminister* OR finanzminister*) lö-ger" 5

Bogner-Strauß: bogner-strauß OR "bundesminister* bogner*" 5 OR "minister* bundesminister bogner*" 5 OR "(bundesmnister* OR minister*) strauß" 5

Kneissl: "(karin OR bundesminister* OR minister* OR außenminister*) kneissl" 5

Kickl: kickl

Ratz: "(eckart OR minister* OR bundesminister* OR innenminister*) ratz" 5

Moser: "(josef OR minister* OR bundesminister* OR justizminister* OR reformminister*) moser" 5

Hartinger-Klein: hartinger-klein OR "(beate OR minister* OR bundesminister* OR sozialminister* OR arbeitsminister* OR gesundheitsminister*) hartinger" 5

Pöltner: "(walter OR minister* OR bundesminister* OR sozialminister* OR arbeitsminister* OR gesundheitsminister*) pöltner" 5

Köstinger: köstinger

Kunasek: kunasek

Luif: "(johann OR minister* OR bundesminister* OR verteidigungsminister* OR heeresminister*) luif" 5

Hackl: "(valerie OR minister* OR bundesminister* OR verkehrsminister* OR infrastrukturminister*) hackl" 5

Faßmann: "(heinz OR minister* OR bundesminister* OR bildungsminister*) faßmann" 5

Schramböck: "(margarethe OR minister* OR bundesminister* OR wirtschaftsminister*) schramböck" 5

Blümel: "(gernot OR minister* OR bundesminister* OR kanzleramt* OR kulturminister*) blümel" 5 OR "blümel (vp OR volkspartei*)" 5

Search strings for Kurz II:

Kurz: "sebastian kurz" 4 OR "kurz (kanzler OR bundeskanzler OR övp OR vp OR volkspartei OR parteiobmann OR spitzenkandidat)" 5

Faßmann: "(heinz OR minister* OR bundesminister* OR bildungsminister* OR forschungsminister* OR wissenschaftsminister*) faßmann" 5

Schramböck: "(margarete OR minister* OR bundesminister* OR digitalisierung* OR wirtschaftsstandort*) schramböck)" 5

Blümel: "(gernot OR minister* OR bundesminister* OR finanzminister*) blümel" 5 OR "blümel (vp OR volkspartei*)" 5

Kogler: "(werner OR minister* OR bundesminister* OR vizekanzler* OR kulturminister* OR sportminister*) kogler" 5 OR "kogler grüne" 5

Schallenberg: "(alexander OR minister* OR bundesminister* OR aussenminister* OR europa-minister*) schallenberg" 5 OR "schallenberg (övp OR vp OR volkspartei*)" 5

Anschober: "(rudolf OR minister* OR bundesminister* OR sozialminister* OR gesundheitsminister* OR konsumentenschutz*) anschober" 5 OR "anschober grüne" 5

Mückstein: "(wolfgang OR minister* OR bundesminister* OR sozialminister* OR gesundheits-minister* OR konsumentenschutz*) mückstein" 5

Nehammer: "(karl OR minister* OR bundesminister* OR innenminister*) nehammer" 5 OR "nehammer (övp OR vp OR volkspartei*)" 5

Tanner: "(klaudia OR minister* OR bundesminister* OR verteidigungsminister*) tanner" 5 OR "tanner (övp OR vp OR volkspartei*)" 5

Köstinger: "(elisabeth OR minister* OR bundesminister* OR landwirtschaftsminister* OR tourismusminister*) köstinger" 5 OR "köstinger (övp OR vp OR volkspartei*)" 5

Aschbacher: "(christine OR minister* OR bundesminister* OR arbeitsminister* OR familienminister*) aschbacher" 5 OR "aschbacher (övp OR vp OR volkspartei*)" 5

Kocher: "(martin OR minister* OR bundesminister* OR arbeitsminister* OR familienminister*) kocher" 5

Gewessler: "(leonore OR minister* OR bundesminister* OR umweltminister* OR energieminister*) gewessler" 5 OR "gewessler grüne" 5

Search strings for Schallenberg:

Schallenberg: "(alexander OR kanzler OR bundeskanzler) schallenberg" 5 OR "schallenberg (övp OR vp OR volkspartei*)" 5

Faßmann: "(heinz OR minister* OR bundesminister* OR bildungsminister* OR forschungsminister* OR wissenschaftsminister*) faßmann" 5

Schramböck: "(margarete OR minister* OR bundesminister* OR digitalisierung* OR wirtschaftsstandort*) schramböck)" 5

Blümel: "(gernot OR minister* OR bundesminister* OR finanzminister*) blümel" 5 OR "blümel (vp OR volkspartei*)" 5
Kogler: "(werner OR minister* OR bundesminister* OR vizekanzler* OR kulturminister* OR sportminister*) kogler" 5 OR "kogler grüne" 5
Anschober: "(rudolf OR minister* OR bundesminister* OR sozialminister* OR gesundheitsminister* OR konsumentenschutz*) anschober" 5 OR "anschober grüne" 5
Mückstein: "(wolfgang OR minister* OR bundesminister* OR sozialminister* OR gesundheitsminister* OR konsumentenschutz*) mückstein" 5
Nehammer: "(karl OR minister* OR bundesminister* OR innenminister*) nehammer" 5 OR "nehammer (övp OR vp OR volkspartei*)" 5
Tanner: "(klaudia OR minister* OR bundesminister* OR verteidigungsminister*) tanner" 5 OR "tanner (övp OR vp OR volkspartei*)" 5
Köstinger: "(elisabeth OR minister* OR bundesminister* OR landwirtschaftsminister* OR tourismusminister*) köstinger" 5 OR "köstinger (övp OR vp OR volkspartei*)" 5
Aschbacher: "(christine OR minister* OR bundesminister* OR arbeitsminister* OR familienmin-ister*) aschbacher" 5 OR "aschbacher (övp OR vp OR volkspartei*)" 5
Kocher: "(martin OR minister* OR bundesminister* OR arbeitsminister* OR familienminister*) kocher" 5
Gewessler: "(leonore OR minister* OR bundesminister* OR umweltminister* OR energieminister*) gewessler" 5 OR "gewessler grüne" 5
Linhart: "(Michael OR minister* OR bundesminister* OR aussenminister* OR europaminister*) linhart" 5 OR "linhart (övp OR vp OR volkspartei*)" 5

Chapter 2

Gambling for Re-election

This chapter is based on co-authored work with Alastair Langtry, Niklas Potrafke, and Marcel Schlepper (see Langtry et al., 2024a).

2.1 Introduction

Very few real-world decisions are risk-free. Huge bodies of work have been devoted to better understand how people respond to risk. Within this, there is a well-established intuition that risk-neutral agents sometimes behave as if they are risk-seeking when they do not bear all of the costs of a bad outcome. This behaviour is called 'gambling for resurrection', and has been documented in a wide range of settings. But there is a distinct lack of empirical evidence as to whether this intuition holds for political decision-making. Political decisions are high-stakes and risky, the outcomes are often judged discretely – as success or failure, victory or defeat – and have enormous welfare implications.

We provide novel empirical evidence that gambling-style behaviour is present in one of the most important political decisions: choosing political leaders. We do so in the context of the 2021 leadership election by the German centre-right parties. Specifically, we show that members of parliament (MPs) gamble for re-election. That is, MPs are more likely to vote for the risky leadership candidate when facing a lower chance of re-election.

Providing direct evidence of risk-taking behaviour in politics has proved difficult. When politicians' decisions are observable, analysis can be muddied by strong signalling motives – where politicians make decisions to send a signal to the public rather than make decisions in line with what they would do in private. It is theoretically well established that observability of decisions changes individuals' behaviour (Levy, 2007; Mattozzi and Nakaguma, 2023) – a finding that has also been empirically documented for important decision-makers (see, e.g., the evidence on Federal Open Market Committee members (Meade and Stasavage, 2008; Swank et al., 2008; Hansen et al., 2018)). Signalling motives and audience costs can therefore obscure the actual preferences of MPs when decisions are observed by voters. When decisions are not observed ('secret ballots'), data availability issues typically prevent empirical analysis.

Unique access to a leak of internal party voting data from the 2021 leadership election held by Germany's centre-right parties allows us to overcome these difficulties. We can therefore take a rare peek behind the curtain of the secret ballot and study MPs' individual behaviour in leadership elections for the first time. While the process of the German centre-right parties choosing their leader does not include a formal vote, in this particular instance, signing a letter supporting one of the two candidates became a *de facto* vote. The existence of this letter became known to the public when it was leaked to a leading German newspaper. But even now, the identities of the signatories

are not publicly known. Our empirical analysis is only possible because we have access to the identities of the signatories.

Our main finding is that MPs who (at the time of the leadership election) were predicted as *less* likely to win re-election in the upcoming German national elections were *more* likely to vote for the riskier leadership candidate. This relationship remains robust across a range of empirical specifications and to the inclusion of a wide battery of control variables on MP- and constituency-level as well as state fixed effects. The effect is sizeable: when MPs were 10 percentage points *less* likely to be re-elected, they were 2.9 percentage points *more* likely to vote for the riskier candidate. Notably, we only find this behaviour among MPs who are running for re-election. We are the first to document evidence of this type of risk-taking behaviour in intra-party selections, and in political decision-making more generally.

Our second finding is that ideological alignment with leadership candidates only influences MPs' decisions when they are *not* running for re-election. For MPs seeking re-election, ideological alignment does not play an important role. This is consistent with MPs being primarily – but not solely – re-election motivated.

The link between MPs' re-election prospects in national elections and their choices in the leadership election is in line with our model of rational risk-taking. Our model is a variant of the classic 'gambling for resurrection' models, adapted to the setting of political leader selection. In the model, there are two elections. First, MPs in one party elect a party leader. Second, voters elect MPs in national elections – and the quality of the party leader affects voters' preferences. MPs care about their leader's ideology and about their own re-election in upcoming national elections. Importantly, there is some uncertainty over a potential party leader's quality in the first election that has been resolved by the time of the second election. Voters have more information about the party leader's quality in the second election than MPs did in the first. Specifically, when there is a benefit of being re-elected regardless of the margin of loss/victory, the incentive structure implies that some MPs 'gamble for re-election'. MPs want to choose a higher risk option (i.e. a leader whose *ex ante* quality is less certain) when their re-election prospects are poor, because there is a discontinuous jump in their payoff at the threshold of winning re-election. Politicians' behaviour is therefore equivalent to 'gambling for resurrection' by managers of firms, an idea first suggested by Jensen and Meckling (1976).

We take several steps to examine the scope for potential confounding. First, we show that our results are not sensitive to a particular specification of the regression model and survive a range of robustness tests. Second, we show that any selection

on unobservables relative to selection on observables would have to be substantial to cancel out our results. Third, we discuss some alternative stories that would generate similar empirical patterns, but would not reflect a causal effect running from MPs' electability to their choices in a leadership election.

We address these potential endogeneity concerns in a series of empirical exercises. First, we demonstrate that our results are robust to controlling for voters' preferences over leadership candidates and for voters' party preferences. This provides evidence that MPs are mostly influenced by their re-election probabilities rather than merely following their constituents' preferences. Second, we show that our results are unlikely to be due to expressive voting by MPs. Third, we collect data on the level of party assistance that MPs received from the incumbent party leader during the 2021 national elections campaign. We find no evidence that party assistance explains the pattern in the data.

Finally, we exploit a large shock that created plausibly exogenous variation in MPs' re-election chances. At the beginning of March 2021 – just a few weeks before the leadership election – it became publicly known that several CDU/CSU politicians abused their positions to make personal financial gain from the procurement of Covid-19 face masks. None of the MPs in our sample were directly involved in the scandal, but the impact was party-wide and drastic. Within two weeks the CDU/CSU polling vote share dropped by almost 10 percentage points. The scandal affected the CDU and CSU similarly and hence did not change voters' relative preferences over the CDU and CSU (and their leadership candidates). We construct an electoral 'shock' variable – using the difference in MPs' predicted re-election chances in January 2021 (*before* the scandal) and their predicted re-election chances in April 2021 (just before the leadership election and *after* the scandal). Our empirical design exploits a quasi-random component that created variation in MPs' *exposure* to the scandal: their distance to the constituencies of the involved politicians. The scandal had a geographic contagion effect: the closer the MPs were geographically to the involved actors, the larger were their electoral shocks. Using the distance as an instrumental variable for MPs' re-election shocks, we derive causal estimates how the shock to MPs' predicted re-election chances influenced their leadership choices. The results strongly corroborate our previous findings. MPs who experienced larger electoral shocks were more likely to vote for the risky leadership candidate.

Our results have important implications for the selection of political leaders, and in turn for voters' welfare. MPs may be willing to choose the worse (in expected value terms) leadership candidate because they are willing to trade off expected quality

against riskiness. This is akin to the 'asset substitution problem' in Corporate Finance, where company managers make a high-stakes decision in the context of a potential insolvency. Managers might choose riskier, but also objectively worse, investments when facing a potential insolvency because they benefit from the investments' success but do not bear the costs of failure ('gambling for resurrection').

From a welfare perspective, such behaviour is worrisome because managers do not consider the costs of bankruptcy to debt-holders. Similarly, a majority of MPs may prefer a candidate who they all expect to be of lower 'quality', as long as that candidate is sufficiently risky, i.e., performing either very poorly or being a political superstar. The consequences are even more severe in the political context: even MPs who are predicted to very likely win re-election may prefer a worse candidate – as long as that candidate comes with sufficiently low risk. This offers a complementary explanation to Carrillo and Mariotti (2001) and Caselli and Morelli (2004) and Mattozzi and Merlo (2015) as to why political parties sometimes choose mediocre candidates – despite the availability of better ones.

Our results further imply that MPs' individually rational choices could seriously undermine their party's success. This behaviour also explains why MPs support different candidates, and provides a new explanation for the emergence of intra-party polarisation – driven by the differing incentives faced by individual MPs.[1]

Related literature. Our paper's core contribution is to provide novel empirical evidence of risk-taking behaviour by politicians. We show this in the important setting of a political leadership selection. As such, this paper relates to three strands of literature.

First, there is a vast literature on gambling style behaviour that follows Jensen and Meckling (1976). This behaviour has been shown to be important in a wide range of contexts, including banking, conflict, and government finance, among others.[2] We provide novel evidence within the realm of political decision-making.

Second, there is a literature on risk-taking in politics, both by politicians and by voters. Within this, Panunzi et al. (2024) and Bernecker et al. (2021) are closest in spirit to our paper. Panunzi et al. (2024) find that, when voters have reference-dependent preferences, those currently experiencing outcomes below their reference point can

[1] In contrast, previous explanations are based on the idea that parties benefit from implementing competitive selection processes that involve *some* degree of intra-party polarisation. This is because competition can serve as a signal for the quality of the party platform and boosts candidates' incentives to provide individual effort (Caillaud and Tirole, 2002; Crutzen et al., 2010, 2020).

[2] See, for example, White (1989), Downs and Rocke (1994), Hellmann et al. (2000), Carrillo and Mariotti (2001), Majumdar and Mukand (2004), Albornoz and Hauk (2014), and Ben-David et al. (2019).

prefer riskier political parties/policies. This is due to the convexity of preferences below the reference point. They also provide survey evidence in support of this mechanism. In contrast, our paper presents evidence about the behaviour of professional politicians (rather than of voters). Furthermore, our mechanism does not rely on non-standard preferences. Rather, it stems from the inherent win-or-lose nature of elections.

Perhaps closer to our focus, Bernecker et al. (2021) find that US state governors with stronger electoral support are less likely to experiment with (risky) welfare reforms. This is similar in spirit to our headline finding that MPs with strong re-election chances opt for a lower risk leader. Beyond focusing on a different decision, we also have a setting where choices were not meant to be observable, and so decisions are likely freer from signalling motives.

The remainder of this literature is theoretical and largely focused on policy experimentation. Within it, one part considers the role of risk-preferences directly (Rose-Ackerman, 1980, 1991; Buisseret and Van Weelden, 2022), and the other part considers how experimentation allows learning about the efficacy of policies (Majumdar and Mukand, 2004; Callander, 2011; Callander and Harstad, 2015; Grunewald et al., 2020).

Finally, we add to the literature on political selection. The larger part of this strand focuses on how intra-party factors, such as internal resource allocation, ideological cohesion, or nomination systems, influence political selection (Cirone et al., 2021; Hansen, 2022; Fiva et al., 2024; Matakos et al., 2024). Less work is dedicated to the selection of political leaders. Existing work has mostly focused on innate characteristics of candidates (O'Brien, 2015; Jeffery et al., 2018; Yu and Jong-A-Pin, 2020), and on how parties create rules for selecting leaders (Kemahlioglu et al., 2009; Snyder and Ting, 2011). We examine a novel dimension: the role of risk. We show that risk is an important factor for political selection.

Furthermore, we are the first to present direct evidence on decision-making in political selections based on the actual voting behaviour of MPs. This is typically either prevented by 'candidate selection being a highly secretive procedure' (Besley, 2005), involving secret ballots and decisions behind closed doors, or by the signalling motives that accompany publicly observable votes. Our unique data and setting are able to overcome these constraints by allowing us to observe MPs' decisions despite the secret ballot.

Organisation. The remainder of this paper is organised as follows. Section 2.2 sets out our theoretical model and establishes key predictions. We describe our data and the setting of our empirical case in Section 2.3. Section 2.4 describes our empirical

strategy and Section 2.5 presents the main empirical results. Section 2.6 provides a supplementary empirical analysis exploiting the re-election shock. Finally, Section 2.7 concludes.

2.2 Theory

2.2.1 Model

Agents. There are three types of agents: a unit mass of voters, indexed j, finitely many members of parliament (MPs), $m \in M = \{1, ..., \mathcal{M}\}$, and two leadership candidates, $\ell \in \{1, 2\}$. There are two political parties (X and Y) and all MPs and leadership candidates belong to party X.[3] There is one MP per constituency (also indexed m), and each voter j is assigned to exactly one constituency.

Elections and strategies. Two elections happen sequentially. In the first, each MP votes for leadership candidate 1 or 2. In the second, each MP stands for election in a single constituency, and each voter j votes in her constituency for party X or for party Y. For clarity, we call the first (intra-party) *leadership election* and the second *national elections*. Abstentions are not allowed, and elections are by majority rule. So strategies for MPs are $s_m \in \{1, 2\}$, and strategies for voters are $s_j \in \{X, Y\}$. We assume that agents naively vote for their most preferred choice in both leadership and national elections. This assumes that they never play a weakly dominated strategy.

Endowments and information. Each MP is endowed with an *electability*, $Q_m \in \mathbb{R}$, and each leadership candidate is endowed with an electability, $Q_\ell + \nu_\ell$, where $Q_\ell \in \mathbb{R}$ and ν_ℓ is a random draw from a distribution with Cumulative Distribution Function (CDF) $F_\ell(\cdot)$. We assume the distribution is continuous and unimodal, has finite support and is symmetric about zero. Each voter j is endowed with a preference for party X, ϵ_j, which is a random draw from a symmetric, mean zero distribution with CDF $G(\cdot)$. All of $Q_m, Q_\ell, F_1(\cdot), F_2(\cdot), G(\cdot)$ are common knowledge. But ν_ℓ is only known by voters at the time of the national elections – *not* by MPs when electing a leader. ϵ_j is known only to voter j.

[3] We leave MPs for party Y unmodelled as they make no decisions here.

Preferences. The utility of voter j in constituency m when the MP from party X wins is:

$$u_j = Q_m + Q_\ell + \nu_\ell + \epsilon_j, \tag{2.1}$$

and we normalise her utility from party Y's MP winning to zero.[4] MPs care about their own re-election and the ideology of their leader – both are specific to the individual MP. For convenience, we assume the payoff from re-election does not depend on the identity of the leader. So the utility of MP m is:

$$u_m(\ell) = I_{\ell,m} + R_m \cdot P_m(\text{win}|\ell), \quad \text{for } \ell \in \{1, 2\}, \tag{2.2}$$

where $I_{\ell,m}$ is the MP's ideological alignment with leadership candidate ℓ, $R_m \geq 0$ is the MP's re-election motivation, and $P_m(\text{win}|\ell)$ is the probability that she is elected given ℓ is the leader. For convenience, we also assume that voters break ties in favour of party X and MPs break ties in favour of leadership candidate $\ell = 2$.

2.2.2 Discussion

Interpreting parameters. Q_m captures how 'electable' MP m is *in her constituency* (it is specific to an MP-constituency pair). This covers a wide range of factors that affect the MP's popularity with voters from her constituency, for example; charisma, competence, name recognition, or ability to secure government spending for the constituency. Similarly, Q_ℓ and ν_ℓ capture how 'electable' leadership candidate ℓ is.[5] This could include the popularity of her policy platform, perceived competence, charisma, ability to raise campaign funds, etc. What separates Q_ℓ and ν_ℓ is what is known at the time of the leadership election. Q_ℓ captures factors already known before the leadership election, and ν_ℓ captures things that happen or are learned between the leadership election and the national elections. For example, corrupt business dealings, extramarital affairs, or good crisis management may only come to light with greater scrutiny following the leadership election.

The leader's electability appears in voters' preferences for two reasons. First, the leader may improve an MP's re-election campaign in the constituency, for example

[4] We only specify voters' preferences over national election outcomes in her own constituency. This is because she can only vote in her own constituency. In Appendix 2.E we introduce network effects to allow for complex spillovers across constituencies. Adding these does not influence our core theoretical predictions.

[5] Our assumptions restrict these to factors that affect all constituencies equally. We could relax this, but doing so would not affect the insights.

through a popularity spill-over or raising campaign funding. Then, $Q_\ell + \nu_\ell$ captures a popularity component which arises from the leader's campaign and benefits or harms all MPs from party X similarly ('coattail effects'). Second, voters may care directly about the leader of party X becoming head of government. Casting their vote, they take into account that voting for an MP from party X makes it more likely for its leader to become head of government. Here, $Q_\ell + \nu_\ell$ can be interpreted as the utility voters receive from the leader's policy platform, perceived competence or character attributes.

The CDF $F_\ell(\cdot)$ captures how uncertain MPs are about candidate ℓ's 'true' electability at the time of the leadership election. A natural expectation is that an incumbent candidate (if there is one) will be lower risk, as she has already been subject to greater scrutiny by MPs, the media, and the voting public. An incumbent will also have shown how well she manages activities at the core of an election campaign, such as speaking in public, interacting with voters, and presenting policy proposals. MPs have had more opportunities to learn about an incumbent's quality, reducing uncertainty.

Applicability. While we will test this model in the context of a specific leadership election among Germany's centre-right parties, the theory applies to majoritarian voting, closed and open lists, and mixed systems alike. In fact, the key mechanism applies far more widely. Gambling-style behaviour in politics may arise whenever politicians (1) choose between risky options and (2) care about the outcome being on one side of a threshold. This is the same as in the wider gambling-style literature that follows Jensen and Meckling (1976). Other political contexts where this will apply include, for example, choosing party manifestos, allocating public funds for research or voting on legislation.

Extension: Party list elections. Voters in our model elect a single MP for each constituency. But, in practice, German voters also cast a second vote – choosing a party at the national level. This second vote elects additional MPs from party lists. In the Appendix, we extend our model to include party list elections and to also allow voters to make different choices for their first and second vote. We show that adding these features does not affect qualitative behaviour (see Appendix 2.D).

Extension: Spillovers across constituencies. Our model assumes that there are no spillovers across constituencies. That is, an MP does not care about how other MPs vote or their probability of winning in national elections. This is because our empirical focus is on risk, ideology, and re-election motives – not on spillovers. Nevertheless, in

Appendix 2.E we allow for a complex network of spillovers – both for how MPs vote and for their chances of winning in the national elections. Those additions introduce multiple equilibria and require additional machinery to deal with network effects. Importantly, however, we show that the core insights and predictions of the model remain unchanged.

2.2.3 Equilibrium and Theoretical Predictions

First, it is straightforward to see that there will be a unique Nash Equilibrium. Voter's decisions are simple: j votes for party X if and only if $u_j \geq 0$. Whether an MP wins or loses in the national elections depends on the *median* voter in her constituency. If ν_ℓ was known, then re-election would be deterministic from the MP's point of view. But an MP does *not* observe ν_ℓ at the time she chooses her leader. An MP hence calculates the *perceived* probability that the median voter will vote for her (the MP), and then votes for a leadership candidate accordingly.

Remark 1. *There exists a unique Nash Equilibrium.*

With this result in place, we now turn to the comparative statics. These will shed light on how changes in parameters – risk, ideology, re-election motives, and electability – influence an MP's behaviour in the leadership election. In an abuse of terminology, we will phrase these results in terms of an MP becoming more or less 'inclined' to vote for a particular leadership candidate.[6]

Our first comparative static result concerns risk. Here, we say a candidate is more risky when they have a larger probability of having more extreme realisations of ν_ℓ. We show how increased riskiness of a candidate has starkly different impacts on an MP's behaviour depending on the MP's underlying electability. The headline result is that 'low electability' MPs prefer a *riskier* candidate, while 'high electability' MPs prefer a *safer* candidate.

Proposition 1. *Suppose that larger shocks become more likely for candidate ℓ. Then an MP m becomes more inclined to vote for candidate ℓ if and only if she (the MP) is insufficiently electable.*

Formally, suppose that $Pr(|\nu_\ell| \geq k)$ weakly increases for all $k > 0$. Then an MP becomes more inclined to choose $s_m^* = \ell$ if and only if $Q_m \leq \bar{Q}$.

[6] In our model MPs' decisions are deterministic, so the statement that an MP is 'more inclined to vote for ℓ' more accurately captures that the MP 'receives a higher net utility from voting for ℓ as compared to $-\ell$'. Nevertheless, it would be straightforward to add a taste shock to MPs' preferences over leadership candidates. That would allow us to talk formally about the probability that an MP votes for a particular candidate. But doing so would clutter the notation without adding insight.

This prediction is similar in spirit to corporate finance models of 'gambling for resurrection'. Within the confines of our model, MPs care about whether or not they win re-election, but not the vote share conditional on winning. So more variable, but symmetrically distributed, outcomes are good for an MP if she needs a sufficiently large positive shock (i.e. positive realisation of ν_ℓ) to win, but bad if she only needs 'not too large a negative shock' to win. In other words, an MP facing a tough re-election campaign finds higher risk attractive, but one in a commanding position prefers a less risky leadership candidate.

An implication of Proposition 1 is that even though MPs are risk neutral, they may be willing to trade off electability against riskiness. Importantly, the way MPs value risk depends on their own electability (and hence their probability of re-election). All else equal, MPs who have low electability prefer the riskier candidate, while MPs with high electability prefer the safer candidate. A simple, yet important, implication is that some MPs will support candidate 2, while others will support candidate 1.

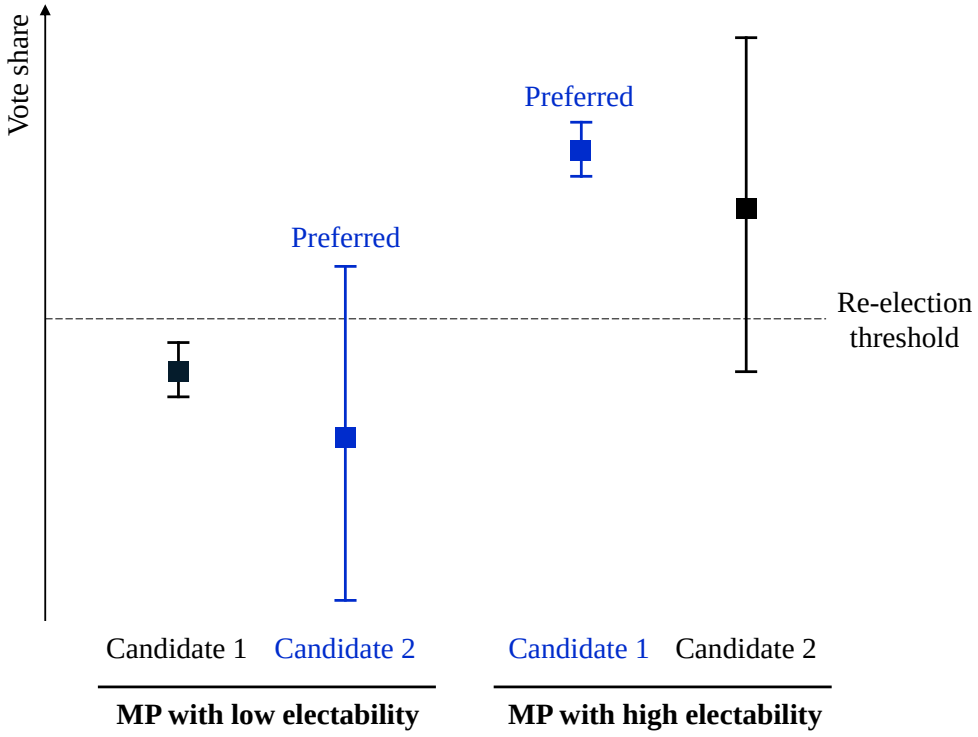
This implies that MPs with low electability may prefer the riskier candidate even when the riskier candidate is *worse* in terms of having lower expected electability than the less risky candidate. This is because MPs with low electability need a sufficiently large positive draw of ν_ℓ in order to win re-election – and a riskier candidate is by definition more likely to deliver this. The fact that a riskier candidate is also more likely to deliver a large negative draw does not matter to these MPs – as they lose with *any* negative draw, regardless of its size. Figure 2.1 shows this intuition graphically. The same gambling for re-election behaviour is possible for high electability MPs, but these MPs would prefer the *safer* candidate – even when that safer candidate is worse (see Appendix Figure 2.B.1).

We next consider the impact of MPs' ideological alignment with the leadership candidates. Intuitively, the result is very straightforward – MPs like to be ideologically aligned with a candidate, and so will be more inclined to vote for a candidate with whom they are more closely aligned.

Proposition 2. *If an MP becomes more ideologically aligned to a candidate ℓ (i.e. if $I_{\ell,m}$ rises), then she becomes more inclined to vote for that candidate.*

The intuition that politicians care about ideology is well established within political science. Closely related, the strength of re-election motives affects how much MPs care about ideology.

Figure 2.1: GAMBLING FOR RE-ELECTION BEHAVIOUR BY LOW ELECTABILITY MPS



Notes: The figure illustrates the range of potential vote shares under two leadership candidates for two MPs. The MP on the left has a lower re-election probability Q_m than the MP on the right. The leadership candidates differ both in electability and riskiness. A higher electability is illustrated by the solid square being further to the top and a higher riskiness is illustrated by a larger distance between the solid square and the whiskers. Here, candidate 1 is more electable ($Q_1 > Q_2$) and less risky ($\lambda_1 < \lambda_2$) than candidate 2. The MP with low electability gambles for re-election by choosing candidate 2 who compensates the lower electability with a high riskiness. The case where candidate 1 is less electable and less risky is shown in Appendix Figure 2.B.1.

Proposition 3. *If an MP m cares less about re-election (i.e. R_m falls), this can only induce her to switch her vote towards the leadership candidate she is more ideologically aligned with (i.e. ℓ s.t. $I_{\ell,m} > I_{-\ell,m}$).*

The intuition behind this result is straightforward: weaker re-election motivation makes the ideological differences between the candidates more important. So MPs who were not voting for candidate 2 due to being focused on re-election may switch to candidate 2 because the ideological preferences become relatively more important. The same holds symmetrically for candidate 1. In the most extreme scenario, an MP who has no re-election motivation at all (R_m=0) would only make a decision based on ideological alignment.

The final component of the model is electability; both of the leadership candidates and of an individual MP. The impact of the first is straightforward: if a leadership candidate is more electable (i.e. Q_ℓ is higher), then MPs are more inclined to vote for her. In contrast, the impact of an MP becoming more electable is not so clear-cut. As Q_m increases, the probability that an MP m wins her election race rises under *both* leadership candidates. What matters for the MP however, is the change in the *relative* probability of winning under leadership candidates 1 and 2. In general, this does not need to always go in the same direction.

In order to showcase a similar intuition to Proposition 1 – that MPs with lower electability are more inclined to vote for the risky candidate – we add an assumption that the risky candidate has a similar risk profile to the safe candidate, *except* that extreme outcomes are more likely. Formally, let F_E be the CDF of a distribution with point mass of 0.5 at τ and at $-\tau$, where $\tau = \inf\{t : F(t) = 1\}$. And assume that $F_2(x) = \alpha F_1(x) + (1-\alpha)F_E(x)$ for some $\alpha \in (0, 1]$. In this case, an increase in electability pushes MPs towards the safer candidate.

Proposition 4. *(i) If a candidate ℓ becomes more electable (i.e. if Q_ℓ rises), then an MP m becomes more inclined to vote for that candidate.*

(ii) Suppose $F_2(x) = \alpha F_1(x) + (1-\alpha)F_E(x)$ for some $\alpha \in [0, 1)$, and $Q_1 = Q_2$. If an MP becomes more electable (i.e. Q_m rises), then generically she becomes less *inclined to vote for the risky candidate ($\ell = 2$).*

This result predicts that, all else equal, MPs who have a higher chance of winning in the national elections will be less inclined to vote for the riskier candidate. So a negative shock to an MPs' electability feeds through into more votes for the riskier candidate. The flavour is the same as Proposition 1: MPs with higher electability (smaller negative shocks) should be less inclined to vote for the riskier candidate.

Key predictions. Overall, these results provide three key theoretical predictions that we take to the data: (#1) MPs are more inclined to vote for the riskier candidate when they face a lower probability of being re-elected in the national elections, (#2) MPs are more inclined to vote for the candidate with whom they are more ideologically aligned, and (#3) ideological alignment matters more when MPs are *not* motivated by re-election.

2.3 Institutional Background and Data

2.3.1 Setting

Our empirical setting is the April 2021 leadership election of the German centre-right sister parties. Angela Merkel's decision to step down as Chancellor required the formally independent parties Christian Democratic Union (CDU) and Christian Social Union (CSU) to select a joint Chancellor candidate.[7] Contested by Armin Laschet, Chairman of the CDU, and Markus Söder, Chairman of the CSU, the leadership election was held in the run-up to German national elections scheduled for 26 September 2021.

Four institutional details are important. First, the leadership election was not for a party leader. The elected candidate would become the German Chancellor (head of government) if the CDU/CSU won the upcoming national elections.

Second, there is no predefined process for the leadership election between the CDU and CSU.[8] But we are able to capture a crucial aspect of the decision-making process that acted as a *de facto* vote. Once both party chairmen had announced their interest in the chancellor candidacy on 11 April 2021, the CDU board unanimously backed its Chairman Armin Laschet. However, on 12 April 2021, 70 out of 200 CDU MPs had signed an internal letter opposing their own party board. This was seen as an expression of support for a chancellor candidacy of the CSU candidate, Markus Söder (see for example, The Guardian (2021) and The Pioneer (2021)). This demonstrates the third important institutional feature: the CDU MPs had to decide whether to sign the letter quickly, as the letter was presented to a crucial party presidium meeting on 13 April 2021. This likely constrained the scope for coordination.[9] Figure 2.2 shows a timeline of the key events.

The final institutional detail is that decisions taken by individual MPs were not observable (to voters). The letter was confidential. While it was leaked to the German

[7]The CDU and CSU are legally two separate political parties with independent members, decision bodies, and chairmen. However, the parties do not compete in elections as the CSU only runs in the state of Bavaria, while the CDU runs outside of Bavaria. During the national election campaign, both parties unite behind one candidate for chancellor. Their MPs form a joint faction in the German parliament. Hence the two parties de facto operate as a single parliamentary party.

[8]The two previous occasions that required a leadership election took place in 1980 and 2002. Such long intervals prevented the establishment of a formal process.

[9]Note that this lack of coordination is ultimately not important for our analysis. Section 2.E extends our theory model to allow for strategic interactions and coordination between MPs and shows that the key predictions are unaffected. It, however, allows us to abstract from network structures in the empirical analysis.

Figure 2.2: TIMELINE OF KEY EVENTS

newspaper FAZ, which reported on the existence of the letter, the identities of the signatories were *never* made public. We were able to obtain the identities directly from the FAZ; a critical step that allows analysis of this leadership election. The confidentiality is important for our analysis. As MPs made their choices unobserved, their vote only impacts the outcome of the contest, and does not serve as a signal to voters.

The letter played a key role. It allowed MPs both to enter the parties' selection process and to express their preferences over the two leadership candidates. Hence, we interpret an MP's decision of whether or not to sign the letter as a *de facto* vote.

Media outlets conjectured that the signatories were afraid of an impending defeat in their constituency and hoped for an improvement in electoral support following the nomination of Markus Söder (FAZ, 2021). As Chairman of the larger party, Armin Laschet had generally been considered the natural Chancellor candidate (BBC, 2021). His position as a quasi-incumbent was weakened because, prior to the leadership election, the CDU had just lost two state elections and performed six percentage points below the 2017 national election result in opinion polls.

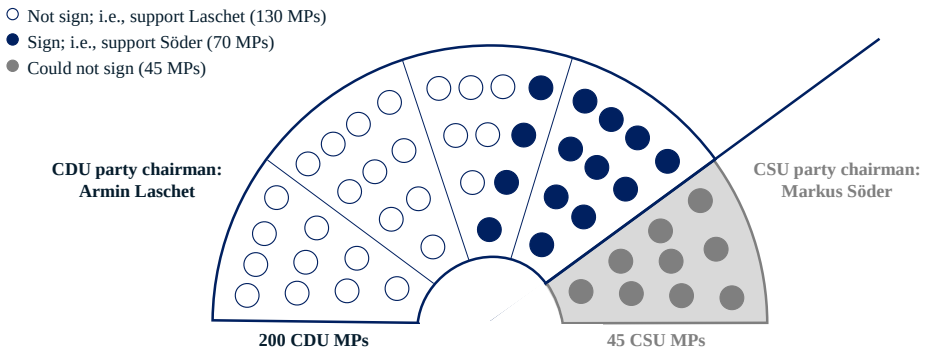
Equipped with stronger approval ratings, Markus Söder provided a viable alternative (InfratestDimap, 2021). However, selecting Söder as the joint Chancellor candidate was associated with higher risk – as is usual for a challenger (seee.g., Panunzi et al., 2024). First, betraying their own Chairman and selecting a Chancellor candidate from the smaller sister party would have pushed the CDU into deep turmoil, with just a few months to the national elections. This argument demonstrates that voting for the challenger *per se* was a risky gamble as MPs went against their party board – potentially facing political repercussions. Second, and related, Armin Laschet was seen as the incumbent – a signal of 'continuity'. Third, the CSU Chairman had a reputation for being a 'political shape-shifter' whose political platform was unpredictable due to frequent flip-flopping (Financial Times, 2021).

Collectively, these reasons demonstrate that Söder was the riskier candidate. In terms of our theory, this would show up as more dispersion in the realisations of ν_ℓ

(the second period electability that is not observed by MPs) for Söder. A quote by the German newspaper Die Zeit (2021) captures well how the risk component from our theoretical model fits Söder's position in our empirical setting: '*Söder, the favorite in all the polls, who is willing to take risks in terms of content for the future leadership role, is the outsider. Armin Laschet, whom only a few trust with the top office, seems to be relying on reaching his goal without any innovative efforts, solely through the logic of the situation.*'

The 70 CDU MPs who opposed their own party Chairman were a minority. Even combined with the 45 CSU MPs, this group was not large enough to form a majority in the joint faction of 245 MPs (see Figure 2.3). Once the CDU board had renewed its support for CDU Chairman Armin Laschet on 19 April 2021, his challenger Markus Söder from the CSU had to withdraw from the race.

Figure 2.3: SUPPORT FOR LEADERSHIP CANDIDATES WITHIN CDU/CSU PARLIAMENTARY FACTION



Notes: Dark blue dots denote MPs who signed the letter (i.e. voted for Söder). White dots denote MPs who did not sign the letter (i.e. voted for Laschet). Gray dots denote CSU MPs who were ineligible to sign the letter. One dot represents 5 MPs.

German national elections in 2021 and risk of defeat for CDU MPs. In German national elections, MPs can get elected through two routes. First, they can win a First-Past-The-Post election in their constituency. In 2021, 299 MPs (out of 736) were elected this way. Second, parties receive non-constituency seats, which are allocated to potential MPs through party lists on the state level.[10] The allocation of these seats follows a complex formula to ensure that each party's number of MPs is proportional to its vote share, for each of the 16 states. As the formula incorporates various factors which are difficult to forecast, both the size of parliament and the allocation of seats are highly uncertain (Vehrkamp, 2021).

[10]National parties have state-level parties in all 16 German states. These state parties independently create the lists for MP candidates running in their state.

CDU MPs are in general unlikely to get re-elected unless they win their constituency. In the 2017 national elections, 185 out of 200 of CDU MPs (92.5 %) were directly elected in their constituency. In ten out of 15 states, not a single CDU MP entered parliament through state lists – in the 16th state (Bavaria) the CDU does not run. At the time of the leadership election, the CDU was polling six percentage points below the previous election result in 2017, implying that the CDU would potentially lose a substantial share of their parliamentary seats. The 2021 national elections results show that this concern was well founded: the party lost 48 of their initial 200 mandates. Even though one third of CDU MPs in 2021 were elected through state lists (54 out of 152), due to the complex allocation formula, eligibility for those seats could not be predicted around the time of the leadership election – this was not even the case in the weeks before the national elections (Der Spiegel, 2021b; Johanssen+Kretschmer, 2021). The state party lists were also compiled between late April and June 2021, *after* the leadership election.[11] These features suggest that our model of constituency-based elections in Section 2.2.1 is a good fit for CDU MPs. The extended model in Appendix 2.D shows that incorporating second votes and party lists MPs does not change our results.

2.3.2 Data

All CDU MPs – but not CSU MPs – were eligible to sign the letter to the CDU board. We collect a rich dataset covering all 195 CDU MPs with a constituency sitting in the German parliament in April 2021.[12] The dataset is constructed from a wide range of sources. Summary statistics for all variables are shown in Appendix Table 2.C.1.

Leadership votes. Our dependent variable – how MPs voted in the leadership election – was obtained from the German newspaper FAZ.[13] This is in the form of the names of the letter's signatories supporting the candidacy of the CSU Chairman, Markus Söder. There is ample variation in the support for Söder – also across states (see Appendix Figure 2.B.2).

[11] Three small states (totalling 16 CDU MPs) are an exception to this. Comparing the 2017 and 2021 state lists, we do not see any effect of whether an MP signed the letter either on her placement nor her movement on the state list. This is as expected because the state lists are compiled by state parties without interference by the national party or its Chairman Armin Laschet.

[12] 200 CDU MPs were elected in the 2017 national elections and all were assigned a constituency. Nine MPs resigned from parliament prior to April 2021. Only four of the nine replacements had been assigned a constituency.

[13] The data was legally acquired and fulfils the AEA Data Legality Policy and the relevant COPE guidelines. We face no restrictions regarding the shareability of the data for reproducing all results of the paper.

Electability. As a proxy for MPs' 'electability', we use re-election probabilities (on a 0-100% scale) calculated by *election.de* as of 9 April 2021 – a few days *before* the letter and *before* Laschet and Söder announced interest in the nomination.[14] They provide up-to-date information on the current status of the race within the constituency. This measure is highly predictive of actual outcomes: 93% of all constituency results were forecasted correctly for the 2017 national elections, and similar accuracy was achieved for two state elections in March 2021 (Moehl, 2021). MPs also consult the website. Politicians in one third of constituencies paid for a 'premium' version of *election.de* (FAZ, 2021; Lutz, 2021). Appendix Figure 2.B.3 shows how the variable is distributed across MPs.

Ideology. Various measures for ideology exist at the party level, but measuring ideology at the individual MP level is more difficult and hence much rarer.[15] We use a supervised machine learning model called 'Wordscores', which is an established tool in the political science literature for extracting political positions from text data (Laver et al., 2003; Lowe, 2008).[16] The model provides ideology scores based on similarities of texts of interest ('virgin texts') with a set of labelled texts ('reference texts'). Specifically, the method first estimates scores for each word that occurs in the reference texts and secondly employs these to generate a score for each virgin text. We use 2017 manifestos from all major German political parties as our set of reference texts. The labels are expert assessments of each manifesto's ideological positions, taken from Chapel Hill (Bakker et al., 2019). Our virgin texts are the parliamentary speeches made by each MP during the 19th parliamentary term (2017–2021). We validate the model's output in Appendix 2.F.1. For our empirical analysis, we only use the ideology scores for CDU MPs.[17] As ideology scores have no natural units, we normalise the scores to have a mean of zero and a standard deviation of one to aid interpretation.[18]

[14]The predicted probability for winning the constituency is generated by a data-driven projection model which takes into account among others the candidates in the constituencies, current demoscopic trends, and the likely vote splitting (Moehl, 2021).

[15]The DW-NOMINATE scores are an exception, which provide ideology scores for political actors in the US based on roll-call voting (Lewis et al., 2023). Deviations from the party line occur too rarely in Germany so as to follow their methodology.

[16]For a full discussion on the strengths and weaknesses of different text scaling approaches see Grimmer and Stewart (2013) and Egerod and Klemmensen (2020).

[17]We transform the raw ideology scores according to the method suggested by Martin and Vanberg (2008).

[18]Our theoretical model uses ideological alignment (i.e. a difference in ideology between an MP and a leadership candidate), while our data captures MPs' ideologies. Under a mild assumption, differences in ideological alignment are linear in MPs' ideology. So our measure of MPs' ideology is perfectly correlated with the theoretical object of interest. Appendix 2.F provides a more extensive discussion.

Re-election motivation. We determined whether MPs were seeking re-election based on local newspaper reporting. This process classified 42 MPs (22% of the sample) as either not seeking re-election or having lost their local party's nomination for re-election before the date of the leadership election. Hence we measure re-election motivations on the extensive margin.

MP-level control variables. We obtained MPs' socio-economic characteristics, such as gender, education, religious affiliation, tenure, party or government positions from the Federal Returning Officer (2017) and MPs' personal webpages. We also use confidential information on MPs' membership in the largest partisan faction 'Parlamentskreis Mittelstand' (PKM) to capture potential network effects. Finally, we construct an indicator measuring the general tendency of individual MPs to rebel by calculating the share of roll-call votes in which the individual MPs deviated from the party line during the 19th parliamentary term.

Constituency-level control variables. We collect macro-economic variables on MPs' constituencies, including unemployment, income, and urbanisation. This data comes from the Regional Database Deutschland & Federal Employment Agency (2022). We also calculate the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD) as a proxy for right-wing pressure (Federal Returning Officer, 2017).

2.4 Empirical Analysis

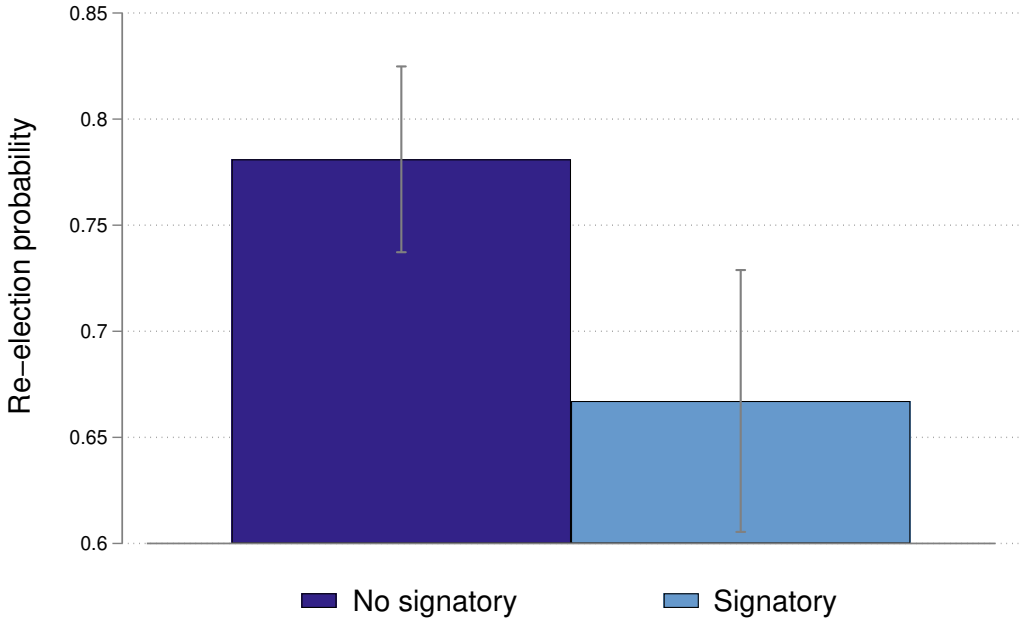
2.4.1 Descriptive Analysis

The raw data is suggestive of gambling for re-election behaviour by MPs. Figure 2.4 shows that MPs who voted for the risky challenger Markus Söder (i.e., who signed the letter) were predicted to be significantly less likely to win re-election than MPs who did not (by a 11 percentage points difference).[19] This pattern cannot be explained by differences in the electability ('quality') of the leadership candidates. If MPs were to choose based solely on candidates' electability, there should be no relationship between

[19]Appendix Figure 2.B.6 demonstrates that MPs' decisions in the leadership election also relate to *realised* election outcomes. Among those MPs who were seeking re-election, MPs who signed the letter were 25 percentage points (!) less likely to get re-elected than MPs who did not sign the letter. This illustrates that supporting the risky candidate, i.e. gambling, was a rational choice by MPs.

MPs' choice of leadership candidate and their re-election probabilities. Figure 2.4, in contrast, suggests that risk matters for MPs.

Figure 2.4: RE-ELECTION PROBABILITY AND SUPPORT FOR RISKIER CANDIDATE



Notes: The figure relates the predicted winning likelihoods of MPs to their decision to sign the letter (90% confidence intervals).

2.4.2 Econometric Model

Our empirical framework follows the logic of our theoretical set-up very closely. The main variables each capture a key element from the theory model: electability (proxied by re-election probability), ideological alignment (proxied by MPs' ideological positioning on a left-right scale), and importance of re-election motivation (proxied on the extensive margin by an indicator of whether or not an MP is running for re-election).[20] The equilibrium characterisation in Section 2.2.3 shows that the re-election motivation modulates the impact of electability and ideology (it appears multiplicatively, rather than additively; see Equation 2.4). It is therefore important to interact the re-election motivation with the other two key variables. Our specification allows us to examine

[20]Our theoretical model also makes an intuitive prediction regarding the leadership candidates' electabilities: a candidate with higher electability will attract more votes. We cannot test this prediction empirically because the leadership candidates' electabilities are fixed across all MPs and therefore do not offer any identifying variation. This also implies that our empirical results cannot be explained by differences in leadership candidates' electabilities.

how support for the risky candidate (Söder) depends on the factors highlighted by our theoretical model. Specifically, we estimate the following regression model:

$$\begin{aligned} Y_m = \alpha + \beta\ Poll_m + \gamma\ ID_m + \delta\ N\text{-}run_m + \zeta\ (Poll_m \times N\text{-}run_m) \\ + \eta\ (ID_m \times N\text{-}run_m) + \mathbf{X}_m\theta + \mathbf{B}_s\mu + \varepsilon_m. \end{aligned} \tag{2.3}$$

Y_m denotes whether CDU MP m signed the letter in support of Markus Söder. $Poll_m$ is the predicted probability of a CDU MP m winning her constituency. ID_m captures MPs' ideological leaning on a left-right-scale based on our supervised machine learning model. $N\text{-}run_m$ is a dummy that takes the value one for MPs who are not standing for re-election in the September 2021 national elections. The two interaction terms ($Poll_m \times N\text{-}run_m$ and $ID_m \times N\text{-}run_m$) are key parts following directly from the theoretical model. They allow us to test whether the effect of electability and ideology are conditional on the re-election motivation.

Beyond the factors highlighted by our theory model, our empirical specification includes a comprehensive battery of MP-specific and constituency-specific controls in the matrix $\mathbf{X}_m$. State- and location-specific factors are absorbed by state fixed-effects ($\mathbf{B}_s$).[21] This allows us to account for a wide range of other factors that might affect MPs' decisions. We estimate the model by OLS and use standard errors robust to arbitrary heteroskedasticity.

2.5 Results

2.5.1 Main Results

Table 2.1 reports our main regression results. Column (I) is parsimonious and includes only the MPs' predicted likelihood of winning, the re-election motivation, and the interaction of the two terms. Column (II) adds constituency- and MP-specific controls. Column (III) adds state fixed effects. In column (IV), we include MPs' ideology. Finally, column (V) adds the interaction term between ideology and the re-election motivation. This yields the full specification from Equation (2.3).

[21] MP-specific controls include: tenure, education levels, gender, religious affiliation, general rebel tendency, faction membership, and party elite dummy. Constituency-specific controls include; unemployment rate, population density, average private households' income, and AfD second vote share in 2017. Some of the factors absorbed by state fixed effects include: state party ideology, state election schedule, and geographical proximity to the home states of the two leadership candidates (Bavaria and North Rhine-Westphalia).

Gambling for re-election. The theory model's key prediction is that MPs are more likely to vote for the riskier candidate (in our setting, the challenger Markus Söder) if they themselves are less 'electable', i.e. have a lower expected probability of being re-elected (Proposition 2). In our empirical model, this implies a negative coefficient β. Table 2.1 shows that the data bear out this prediction. Throughout all specifications, the coefficients on the *Poll* variable are negative and statistically significant when MPs stood for re-election. In other words, MPs exhibit 'gambling for re-election' style behaviour.

Table 2.1: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — BASELINE REGRESSION RESULTS

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.285**	-0.261*	-0.286*	-0.288*	-0.294*
	(0.128)	(0.139)	(0.168)	(0.170)	(0.167)
Not-run × Poll	0.042	0.035	0.150	0.147	0.077
	(0.290)	(0.273)	(0.309)	(0.308)	(0.307)
Ideology				0.009	-0.024
				(0.034)	(0.036)
Not-run × Ideology					0.175*
					(0.098)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.034	0.244	0.307	0.307	0.322

Notes: The table shows the results from estimating Equation (2.3). We show results from five specifications. We start with a parsimonious model that examines how MPs' electability (*Poll*), and the interaction of electability with the re-election motive, relate to MPs' leadership choices. We gradually augment this model by introducing MP- and constituency- controls (Column II), state fixed effects (Column III), ideology (Column IV) and the interaction term of ideology with the re-election motive (Column V). All specifications also include the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Importantly, our point estimates suggest that this relationship is economically significant. A ten percentage points *decrease* in the predicted re-election probability for an MP is associated with a 2.9 percentage points *increase* in the probability that she supports the risky candidate. The point estimates stay remarkably constant across specifications, supporting the argument that the link between re-election probabilities

and MPs' choices does not depend on control variables. Furthermore, we find this relationship only for MPs seeking re-election (i.e. who are re-election motivated) – exactly as predicted by our model. For MPs not seeking re-election, the marginal effect, i.e. the sum of coefficients of the *Poll* variable and the *Not-run* × *Poll* interaction, does not turn out to be statistically different from zero ($t = 0.71$ in the most comprehensive specification).

Result #1 *MPs gamble for re-election: A ten percentage points decrease in the predicted re-election probability for an MP is associated with a 2.9 percentage points increase in the probability of supporting the risky candidate.*

Ideology. The second theoretical prediction is that MPs who are more ideologically aligned with a candidate are more likely to vote for that candidate (Proposition 3). In the empirical application, this corresponds to the prediction that $\gamma > 0$. This is because Markus Söder (the CSU Chairman) is the more right-wing of the two candidates. The CSU is traditionally regarded as more conservative than the CDU, which is also reflected in the Chapel Hill rating (Bakker et al., 2019).

Our empirical results do *not* support this prediction. In both columns (IV) and (V), the point estimates on the *Ideology* variable are not statistically different from zero. Note that these coefficients are a precisely estimated zero and rule out even modest associations of ideology with MPs' choices. This suggests that MPs (at least those seeking re-election) are primarily motivated to choose a leader who will help them win re-election, rather than one who they find ideologically appealing.

Result #2 *For MPs seeking re-election, ideological alignment does not play an important role for their decision-making in a leadership election. This is consistent with MPs being primarily – but not solely – re-election motivated.*

The role of the re-election motive. A third important prediction from our model is that a reduced re-election motive makes ideology relatively more important for MPs' choice of leadership candidate – and consequently reduces the importance of their re-election probability (Proposition 4). In the extreme case, where MPs have no re-election motive at all, it is only ideology that matters. With our data, we test for extensive margin changes — whether or not MPs are seeking re-election at all -– instead of 'marginal' changes in the re-election motive.

Within our empirical framework, this amounts to the prediction that for MPs who are not seeking re-election, ideology matters for their choice *and* that their re-election probability does not. In other words, the marginal effect of the *Poll* variable is zero, while the marginal effect of *Ideology* is non-zero. This is exactly what we find in our regression results. Column (V) shows that the coefficient on the interaction term between the re-election motive and MPs' ideology is positive and statistically significant (the marginal effect of ideology for MPs not seeking re-election yields a t-statistic of 1.67). Among MPs not seeking re-election, more conservative MPs were more likely (all else equal) to support Markus Söder. This matches our view that Markus Söder is the more conservative of the two candidates. In contrast, our previous finding showed that for those MPs not seeking re-election, the marginal effect of the *Poll* variable is not statistically different from zero ($t = 0.71$). More conservative MPs were thus more likely to support the more conservative CSU Chairman, but *only* when they were not seeking re-election.

This result is suggestive that our inability to find a relation between ideological alignment and MPs' choices in the full sample of MPs (Result #2) may be because the risk channel dominates the ideology motive, rather than because the ideology motive does not exist at all. It also demonstrates the importance of the interaction between re-election motivation and ideological alignment highlighted by the theoretical model.

Result #3 *Importance of the risk channel: risk preferences of MPs dominate the importance of ideological alignment for MPs selecting a political leader. MPs only care about ideological alignment when not seeking re-election.*

2.5.1.1 Robustness

Next, we demonstrate that our empirical results survive a wide range of robustness checks. For briefness, data tables are relegated to the Appendix.

Functional form and sample restrictions. Our results are not driven by the use of an OLS model: changing to a Probit Model has no impact on inferences (Appendix Table 2.C.2). The results are also not driven by outliers or by the inclusion of state fixed effects – both of which could be a concern given our modest sample size. Jackknife regressions, which exclude one MP at a time, show that results do not rely on individual MPs (Appendix Figure 2.B.7). Inferences do not change when we account for cultural and geographical differences with an East-Germany dummy, rather than full state fixed effects (Appendix Table 2.C.3).

Testing our key variables. The polls and ideology variables are the main explanatory variables in our analysis. Our measure of ideology is derived from MPs' speeches, and so is likely noisy (i.e. it may suffer from measurement error). To address any resulting attenuation bias, we include the standard errors of the ideology variable as an additional control. Inferences stay qualitatively the same (see Appendix Table 2.C.4). Even though our polling data is a very accurate predictor of election outcomes (see Section 2.3.2), the variable might still suffer from measurement error. The data generating process of the polling data, however, involves simulations of more than 30 million possible election results (Moehl, 2021). This bootstrapping-type procedure hence reduces concerns about measurement error.

COVID-19. The leadership election took place amid the Covid-19 pandemic. We rule out that geographical differences in the severity of the pandemic situation, and hence potentially different policy preferences of MPs, influence our results. Controlling for constituency-level infection cases (relative to the population), either cumulatively to 12 April 2021 (the day the letter was sent), or just in the seven days prior to 12 April 2021, does not change our results (see Appendix Table 2.C.5).

Second route into parliament – the state list. Finally, we show that controlling for whether MPs were placed on the 2017 state lists (*ex-ante*) or the 2021 state lists (*ex-post*) does not change inferences (Appendix Table 2.C.6). This is in line with the arguments in Section 2.3.1.

Selection on unobservables. While we have controlled for a wide range of potential confounders, we cannot completely rule out the possibility that there is some selection on unobservables. We therefore use the Oster (2019) test to estimate how large the selection on unobservables would have to be in order to drive the estimated effects on the *Poll* variable. We find that selection on unobservables would have to be substantial – 1.9 times larger than the selection on controls and state fixed effects – to cancel out our estimated effects.

2.5.2 Alternative Explanations

The empirical results confirm our model's predictions about gambling style behaviour. However, a natural concern is that the results might be driven by some other mechanism, which would generate the same empirical patterns, but without re-election chances having a *causal* effect on MPs' leadership decisions. We consider four leading alternatives and show that addressing them does not alter our results.

Misspecified preferences: following constituents. A first alternative story is that MPs do not care about re-election and the ideology of their leader (which are both assumed in our model), but instead follow their constituents' preferences over the two leadership candidates. If MPs from constituencies with low re-election probabilities are, on average, those whose constituents relatively prefer the policy platform of the challenger over the policy platform of the incumbent, then those MPs would be more likely to support the challenger in the leadership election. If this were the case, voters' preferences for a party/MP and for a leadership candidate would generate a corresponding association between MPs' re-election chances and their choice of leadership candidate.

Misspecified preferences: expressive voting. A second alternative story is that MPs' voting decisions were not reflective of their true preferences over leadership candidates, but were instead due to expressive voting. MPs could have voted for Söder not because they preferred him as a chancellor candidate but instead to push Laschet into adjusting the party's policy platform.

An omitted variable: party assistance. A third alternative story is that CDU MPs receive assistance from their party leader, Armin Laschet, and that this assistance drives both their re-election chances and their inclination to vote for Laschet. Such assistance might take the form of channelling party or donor funds to MPs, more frequent visits by senior politicians to the constituencies, or extra government spending in the constituencies – factors that could make MPs receiving the assistance more electable. Receiving this assistance would also likely make MPs more reliant on Armin Laschet and hence would create a spurious correlation between MPs' choice of leader and their probability of re-election.

Reverse causality: support for Markus Söder *caused* lower poll numbers. A fourth alternative story is that MPs' support for Markus Söder entered voters' preferences directly. That is, voters punished CDU MPs for backing the non-CDU leadership candidate. This would create a negative association between MPs' choice of leader and their probability of re-election; but one where their choice of leader *causes* their re-election probability.

This possible explanation is immediately ruled out because (1) the re-election probabilities (the *Poll* variable) are taken from one week *before* MPs voted for the leadership

candidate, and (2) MPs' votes were secret (and despite the leak of the letter, voters never observed MPs individual votes – only the total number voting for Söder).

2.5.2.1 Assessing the alternative explanations

Following constituents. To address the first possibility of misspecified preferences, we disentangle voters' preferences over leadership candidates from MPs' preferences. We measure voters' preferences using high-quality, geo-referenced, and representative household survey data from Infratest Dimap (2022). The geo-referencing is critical – it allows us to aggregate these preferences to the constituency level. The data captures two measures of voters' feelings towards the two leadership candidates: (1) satisfaction with the quality of their political work, and (2) perceptions of them as a suitable Chancellor.[22] The data allows us to examine whether voters' support for the incumbent Laschet, voters' support for the challenger Söder, or the relative support for Laschet vs. Söder explains MPs' decisions. If MPs base their leadership decisions not on own objectives but simply follow their constituents' preferences, then it should be these measures, and not the *Polls* variable, that have predictive power.

A second major advantage of the data is that it allows us to measure the *current* support for the CDU at the constituency level. Using the question that asks which party the respondents would vote for if the election were held the next Sunday, we construct CDU party vote shares for the upcoming national elections at the constituency level. These measures are more suited to capture up-to-date party preferences than past party vote shares. This is particularly relevant here, as the CDU faced a major scandal regarding corruption in the procurement of Covid-19 facemasks starting in the beginning of 2021. The survey-based party preferences thus allow us to focus on the margin of MPs' electability that is specific to the 2021 cycle.[23]

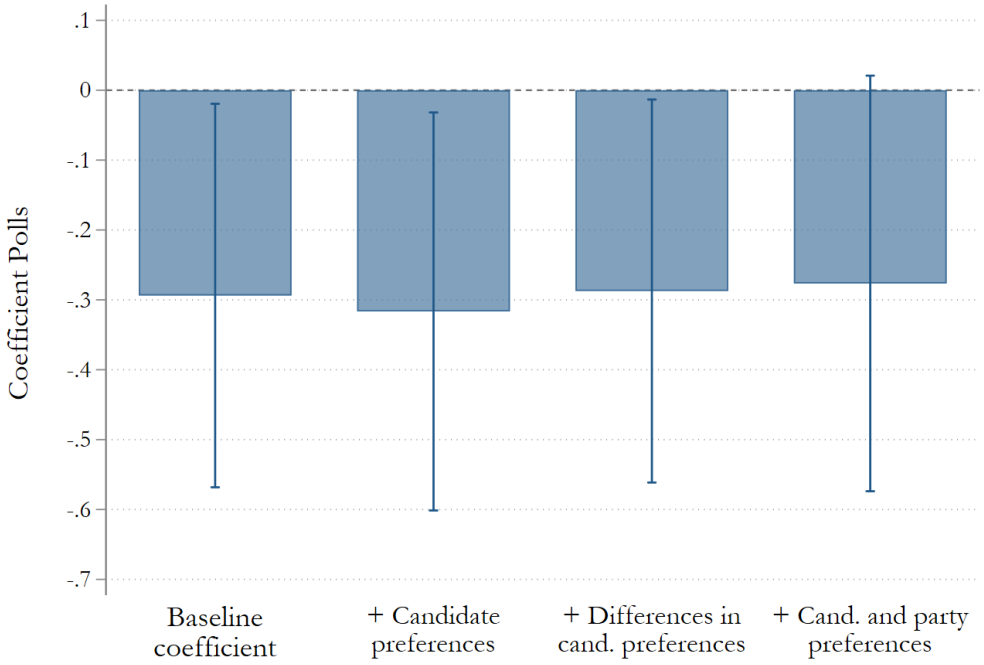
Figure 2.5 shows the coefficients of the *Polls* variable when considering voters' preferences. The figure demonstrates that adding: (1) all four measurements of voters' preferences over leadership candidates, (2) the relative support of voters for the can-

[22] The exact wording of the questions is *'How satisfied are you with [Markus Söder/Armin Laschet]'s political work?'* and *'The next general election will take place in September. The CDU/CSU candidates for Chancellor are Markus Söder and Armin Laschet. What do you think: Would [Markus Söder/Armin Laschet] be a good candidate for chancellor or not a good candidate for chancellor of the CDU/CSU?'*. We pool all waves of the survey in the 12 months prior to the leadership election containing these two questions (from April 2020 to April 2021), yielding 8,380 observations for the first question, and 6,233 observations for the second question. Appendix Figure 2.B.8 shows that there is ample variation in both measures across constituencies.

[23] Appendix Figure 2.B.9 shows that the survey-based vote shares of the CDU closely track the results in the 2021 national elections.

didates, and (3) controlling additionally for the CDU vote share in the constituency ('voters' party preferences') does not qualitatively change our results. The coefficient on the *Polls* variable remains stable (and turns just insignificant in the most restrictive specification, $t = 1.53$).

Figure 2.5: ACCOUNTING FOR VOTERS' PREFERENCES



Notes: The figure shows the coefficients on the *Polls* variable with 90% confidence intervals when (a) estimating our full baseline specification, (b) additionally accounting for voters' candidate preferences via the share of voters who are satisfied with the quality of Markus Söder/Armin Laschet's political work and the share of voters who consider Markus Söder/Armin Laschet to be a suitable chancellor candidate, (c) accounting for the *relative* support over the leadership candidates (for both shares) and (d) for voters' candidate *and* party preferences.

The full table of results is provided in Appendix Table 2.C.7. The results show that voters' preferences *do* have an influence on MPs' leadership decisions: MPs were less likely to vote for Söder when their constituents were more satisfied with the political work of Laschet (Column 2, $t = 1.67$). The same results hold when considering the relative support over candidates: a higher relative support for Laschet versus Söder decreased MPs' likelihood of voting for Söder (Column 3, $t = 1.61$). Lastly, there is suggestive evidence that voters' party preferences also mattered: MPs in constituencies with a higher CDU vote share where less likely to vote for the CSU candidate Söder (Column 4, $t = 1.36$). Importantly, however these variables do *not* impact our finding that MPs gamble for re-election. The point estimates on the pre-existing variables remain stable throughout all specifications. Combined, this presents strong evidence

that MPs' decisions are reflective of risk-taking behaviour and are not merely based on their constituents' preferences.

Expressive voting. The alternative story that MPs made their choices in the leadership election with the sole intent to influence the policy platform of Laschet, applies *only* to MPs that would gain from shifting the policy-platform to the right (as voting for Söder would signal the desire for a more conservative policy platform). This alternative story is directly weakened by the fact that our regressions control for (i) MPs' own ideology and (ii) for the second vote share of the AfD in the constituency (a proxy for right-wing pressure). The effect of MPs' re-election probabilities persists beyond those controls.

We directly test whether right-wing pressure for MPs was predictive of MPs' choices. Specifically, we interact our *Poll* variable with a dummy whether the predicted front-runner or runner-up MP in the constituencies were AfD candidates (based on the predictions by *election.de*). If CDU MPs voted for Söder with the intent of moving Laschet's platform to the right, then the coefficient on the interaction term should be positive and statistically significant. Results are reported in Appendix Table 2.C.8. Estimating the full-specification shows that the base-effect of the *Poll* variable stays qualitatively unchanged (coef.: -0.270, se: 0.175), while the coefficient on the interaction term is close to zero (coef.: 0.064, se: 0.388) and does not turn out to be statistically significant ($t = 0.16$). This indicates that MPs' voting behaviour is unlikely to be due to expressive voting.

Party assistance. To address the concern of party assistance being an omitted variable, we collect data on which constituencies (and local CDU MPs) the incumbent party head, Laschet, visited during the 2021 national elections campaign. We retrieve this information via searches for local newspaper articles, social media posts, and posts on MPs' webpages. 20% of the MPs in our sample had joint campaigning events with Laschet in their constituencies. Campaign assistance does correlate with MPs' leadership decisions ($\rho = -0.18$), but controlling for dummies whether MPs have received party assistance during the election campaign does not change our qualitative results (see Appendix Table 2.C.9).

2.6 Exploiting a Large Shock

Thus far, we have shown that our empirical results are robust to a wide range of potential confounders in our baseline model, survive an extensive list of robustness exercises, and cannot be explained by a range of potential alternative stories. We now exploit a sudden and drastic shock that created plausibly exogenous variation in MPs' re-election chances.

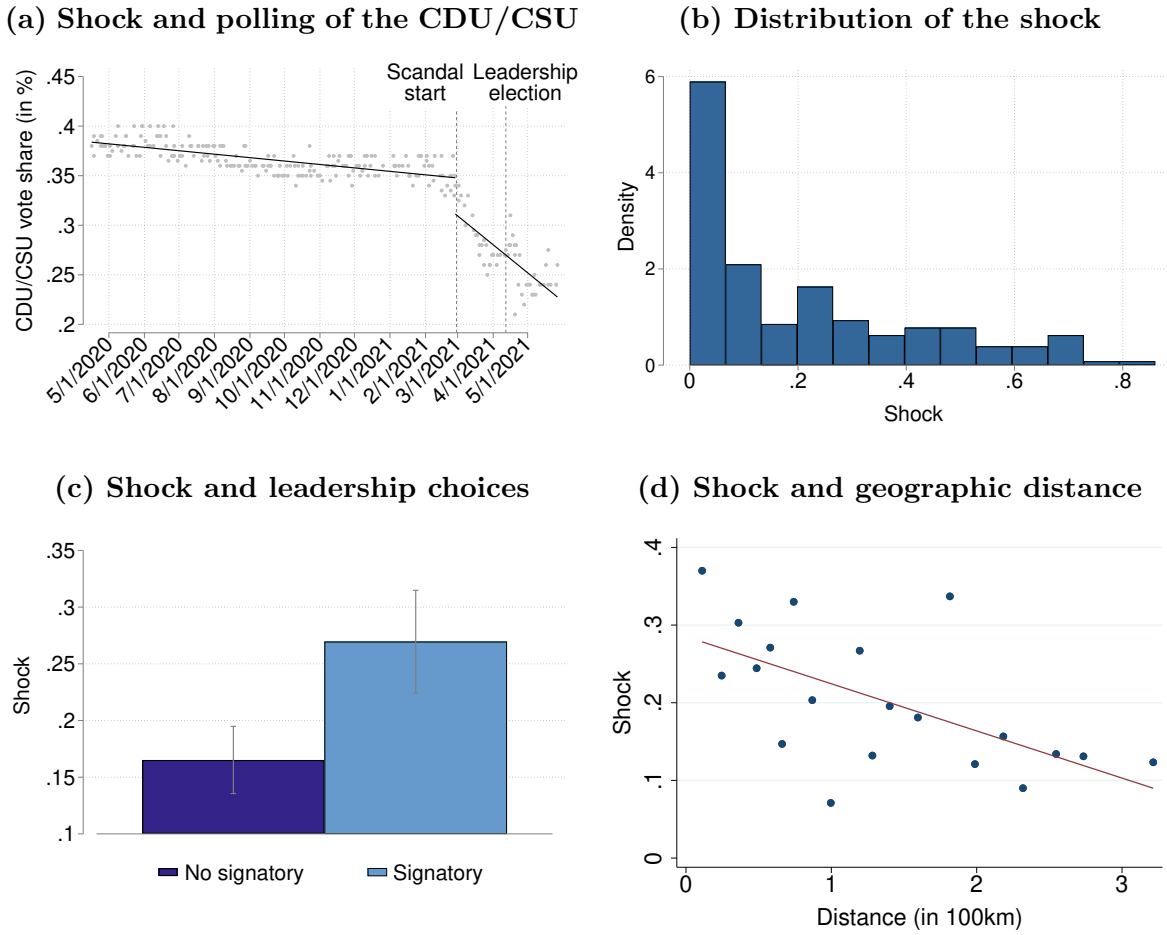
2.6.1 The Shock and Descriptives

The Covid-19 face mask scandal. In the beginning of March 2021 – just a few weeks before the leadership election – the CDU/CSU experienced a major scandal regarding corruptive behaviour in the procurement of Covid-19 face masks. The German newspaper 'Der Spiegel' first reported on the scandal, involving several CDU/CSU politicians who were accused of benefiting personally from government mask procurement deals. The scandal involved both CDU *and* CSU politicians – but not a single MP in our dataset was directly involved in the scandal. The scandal subsequently impacted the CDU and CSU to the same extent. Importantly for our setting, this also shows that the scandal did not shift voters' relative preferences over the CDU and the CSU (and hence over their leadership candidates).

The scandal fuelled public outrage, raising concerns about ethical standards and transparency within the CDU/CSU. We collect all opinion polls in the year before the leadership election from the eight leading public opinion polling institutes (Allensbach, Verian, Forsa, Forschungsgruppe Wahlen, GMS, Infratest, dimap, INSA, YouGov) from *wahlrecht.de*. The response of voters to the scandal was swift and drastic. Before the scandal, the CDU/CSU was polling between 35-37% (above their previous results of 32.9% in the 2017 national elections). The support for the CDU/CSU dropped by almost 10 percentage points within just a few weeks (see Panel (a) of Figure 2.6).

Constructing a shock variable. The scandal allows us to construct a 'shock' variable on the individual level of the MPs. We take the re-election chances of MPs in January 2021 – well before the scandal – and calculate the differences to our main poll data of MPs just before the leadership election in April 2021 (after the scandal). Panel (b) of Figure 2.6 shows the distribution of this measure, where larger values indicate larger shocks (i.e. higher drops in the personalized re-election chances). The figure demonstrates that (i) not a single MP improved their re-election chances when

Figure 2.6: IMPACT OF THE SCANDAL, RE-ELECTION SHOCKS, AND LEADERSHIP DECISIONS



Notes: Panel (a) shows the polling vote share of the CDU/CSU in the year before the leadership election. Data comes from *wahlrecht.de* and includes the eight leading public opinion polling institutes (Allensbach, Verian, Forsa, Forschungsgruppe Wahlen, GMS, Infratest, dimap, INSA, YouGov). Panel (b) shows the distribution of the shock variable. Panel (c) relates the shock variable to MPs' leadership decisions (90% confidence intervals). Panel (d) shows a binned scatter plot between the shock variable and the minimum geographic distance to one of the exposed constituencies.

comparing their April predictions against the predictions pre-shock and (ii) that the distribution of the shock variable has a long tail; showing that the shock had a substantial impact on a good share of the CDU MPs.

Panel (c) of Figure 2.6 relates the shock variable to MPs' leadership choices. Consistent with the previous findings about the *level* of re-election chances, we find that MPs who experienced larger shocks to their re-election chances were significantly more likely to support the risky candidate Söder.

2.6.2 Empirical Strategy and Results

Why is the shock well-suited? An ideal shock should move individual re-election chances of MPs without moving voters' preferences. More specifically, the shock should not move voters' *relative* preferences over the CDU versus the CSU (and their leadership candidates).

In an electoral system where voters cast two votes simultaneously (one for an MP, one for a party), MPs' re-election chances are – among others – a function of voters' current (local) *candidate* preferences and voters' current (general) *party* preferences. Both measures will often co-move – our theory model illustrates for example how a better chancellor candidate benefits individual MPs.

The scandal provided a shock to voters' party and candidate preferences. Two features are important. First, the scandal affected voters' CDU and CSU party preferences to the same extent – Appendix Figure 2.B.10 demonstrates this by plotting the CSU polling data in Bavaria pre- and post-shock. The CSU experienced a similar 10 percentage points drop in their polling vote shares. Second, the scandal had a party-wide impact, but none of the MPs in our dataset were directly involved in the scandal. Nevertheless, the scandal lead to heterogenous impacts on the MPs. We now exploit a quasi-random factor in MPs' exposure to the shock to derive causal estimates how the shock to MPs' re-election chances influenced their leadership choices.

Identification strategy. In the first step, we replace the *Poll* variable in Equation (2.3) with our *shock* variable. This allows us to examine how the shock-induced *change* in the *Poll* variable influenced MPs' voting decisions. For identification, we exploit MPs' geographic distance to involved actors/MPs in the scandal, leading to quasi-random *exposure*. There were five MPs that were directly involved in the scandal: Alfred Sauter (CSU), Georg Nüßlein (CSU), Nikolas Löbel (CDU), Niels Korte (CDU), and Mark Hauptmann (CDU). These MPs came from four different constituencies, spread across Germany.[24] The public outrage was especially strong in those districts – ultimately forcing the MPs to resign (or to exit the party) after the scandal became public (Der Spiegel, 2021a; DW, 2021).

However, the scandal had a geographic contagion effect. We calculate the minimum geographic distance of all 195 constituencies to the four involved constituencies. Panel

[24]Korte was an MP in district 84 (Berlin-Treptow-Köpenick), Sauter and Nüßlein were both MPs (one in the Bavarian state parliament, one in the Bundestag) in district 255 (Neu-Ulm), Löbel was an MP in district 275 (Mannheim), and Hauptmann was an MP in district 196 (Suhl–Schmalkalden-Meiningen–Hildburghausen).

(d) of Figure 2.6 shows the logic of our identification approach: the closer the MPs were geographically to the exposed constituencies, the larger were the shocks to their re-election chances.

Table 2.2: RE-ELECTION SHOCK AND CANDIDATE CHOICE — REGRESSION RESULTS

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
	Panel A: OLS				
Shock	0.539***	0.379**	0.329	0.330	0.342*
	(0.178)	(0.188)	(0.206)	(0.208)	(0.205)
Not-run × Shock	-0.131	-0.108	-0.131	-0.131	-0.043
	(0.377)	(0.380)	(0.404)	(0.404)	(0.400)
Ideology				0.009	-0.025
				(0.034)	(0.036)
Not-run × Ideology					0.178*
					(0.097)
	Panel B: First stage				
Distance (in 100km)	-0.058***	-0.053***	-0.120***	-0.120***	-0.120***
	(0.017)	(0.019)	(0.033)	(0.034)	(0.034)
	Panel C: Second stage				
Shock	1.640**	1.352	1.352*	1.365*	1.361**
	(0.700)	(0.888)	(0.690)	(0.700)	(0.685)
Ideology				0.016	-0.024
				(0.037)	(0.039)
Not-run × Ideology					0.212**
					(0.094)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
Kleibergen-Paap F-stat.	11.79	7.83	13.04	12.69	12.65
Anderson-Rubin p-val	0.014	0.111	0.041	0.041	0.038

Notes: The table shows the OLS and instrumental variable results using the shock variable as main regressor and the distance to the exposed constituencies as an instrument. Panel A shows the OLS results, Panel B the first stage results, and Panel C the second stage results. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Results. Table 2.2 shows the results. In Panel A we report the OLS-results: when MPs experienced a larger shock to their re-election chances, they were more likely to vote for the risky candidate Söder.

The first stage results show that our instrument is a relevant predictor for the shock (Panel B). An increase in the distance to the exposed constituencies by 10km relates to a 1.2 percentage points decrease in the shock variable. The Kleibergen-Paap F-stats are generally above the required level of ten, indicating that our instrument is a strong predictor. The second stage results in Panel C strongly corroborate our OLS results throughout all specifications: MPs with higher shocks were more likely to support the riskier candidate, and ideological alignment only matters for MPs who are not seeking re-election. In the full specification, these results are statistically significant at the 5% level; and the low p-values of the Anderson-Rubin test indicate that they are robust to weak-IV inferences.

Result #4 *MPs who experienced larger negative shocks to their predicted re-election probabilities were more likely to support the risky candidate.*

This result is perfectly in line with our first key prediction from the theoretical model (Section 2.2.3). More specifically, it bears out the prediction of Proposition 4(ii): a negative shock to an MP's electability makes them more likely to vote for the riskier candidate.

Robustness. One possible concern might be that the distances correlate (by chance) with voters' preferences. To address this concern, we additionally control for voters' party preferences and voters' preferences over leadership candidates (using the survey data introduced in Section 2.5.2). The results are shown in Appendix Table 2.C.10. Consistent with the random component of the geographic distance, inferences do not change.

2.7 Conclusion

Politicians often have to choose between riskier and safer options. And, as in the case of selecting political leaders, the stakes are often high. But insights into risk-taking behaviour in politics are difficult for one of two reasons. First, when behaviour is publicly observable, politicians may be concerned about public perceptions, which can affect their decisions. This makes it difficult to disentangle signalling incentives to voters from inherent risk preferences. Second, when behaviour is not observable, there are clear data availability issues. In the case of selecting political leaders, secret ballots have proved the key barrier – they do not reveal individual decisions. We overcome these issues through unique access to a leak of party-internal data, allowing us to observe MPs' leadership decisions for the first time.

We set out a theoretical model of rational risk-taking by MPs, and derive predictions on the factors that drive MPs' decisions in leadership elections. Our main prediction is that MPs 'gamble for re-election'. That is, they value the degree of uncertainty over the leadership candidates' electability differently. MPs predicted to fall short of re-election prefer a riskier candidate, while MPs above the margin for re-election prefer a candidate with lower risk. More generally, the situation can make MPs behave *as if* they are risk-averse or risk-loving – even if they have risk-neutral preferences.

We document exactly this 'gambling for re-election' behaviour in the leadership competition of the German centre-right parties before the 2021 national elections. We show that – even after accounting for a battery of potential confounders – MPs with a lower predicted re-election probability were more likely to support the riskier candidate. Specifically, a 10 percentage points reduction in the re-election probability is associated with a 2.9 percentage points increase in the likelihood of voting for the riskier candidate. We find this behaviour only for MPs who are running for re-election. We also show evidence that risk preferences dominate ideological alignment when MPs select political leaders. Ideological alignment with leadership candidates only matters for those MPs who are not running for re-election. We obtain these results both in OLS regressions and confirm them via an instrumental variable approach that induces plausibly exogenous variation in the level of electoral shocks that MPs faced after a major scandal.

Our results carry a pair of important implications. First, MPs may be willing to choose the worse leadership candidate (in expected value terms). This because they are willing to trade off expected quality against riskiness. This is akin to the 'asset substitution problem' in Corporate Finance, but with more severe consequences:

even MPs who face good re-election prospects would be willing to choose the worse candidate if that candidate is less risky. This offers a new explanation as to why political parties sometimes choose mediocre candidates – despite the availability of better ones. Second, MPs' individually rational choices can seriously undermine their party's success. This behaviour also explains why MPs support different candidates – and provides a new explanation for the emergence of intra-party polarisation. In the same spirit, many commentators argued that the fierce competition between the two candidates and the subsequent divide within the party were important factors in the CDU/CSU performing poorly in the 2021 national elections. Understanding the implications gambling behaviour has for policy-making and voters' welfare remains a promising avenue for future research.

Appendix to Chapter 2

This appendix presents proofs of our theoretical results, details on the theory extensions, validation and additional results:

- Section 2.A provides proofs to our theoretical results.
- Section 2.B provides additional figures.
- Section 2.C provides additional tables.
- Section 2.D provides additional details on the theoretical party list extension.
- Section 2.E provides additional details on the theoretical network structure extension.
- Section 2.F provides a discussion and validation of our ideology scores.

2.A Proofs

Characterising equilibrium behaviour. First, it is immediate from the voter's preferences and our tie-breaking assumption that $s_j^* = X \iff u_j \geq 0$. Second, it follows from MPs' preferences and our tie-breaking assumption that $s_m^* = 2$ if and only if $U_m \geq 0$, where we set $U_m \equiv u_m(2) - u_m(1)$ (so U_m is the *net* utility of voting for leadership candidate 2).

It follows from the majoritarian voting assumption that MP m wins if and only if the *median* voter in her constituency chooses party X. Because there are a mass of voters, the median voter has $\epsilon_j(\text{median}) = 0$. Therefore $P_m(\text{win}|\ell) = P_m(u_j \geq 0|\ell, \epsilon_j = 0) = P_m(Q_m + Q_\ell + \nu_\ell > 0)$. Straightforward algebra yields $P_m(\text{win}|\ell) = F_\ell(Q_m + Q_\ell)$.[25] So we have $u_m(\ell) = I_{\ell,m} + R_m \cdot F_\ell(Q_m + Q_\ell)$. Finally, straightforward rearranging yields

$$(s_m^* = 2 \iff)\ U_m \geq 0 \iff R_m F_2(Q_m + Q_2) - R_m F_1(Q_m + Q_1) + I_{2,m} - I_{1,m} \geq 0. \tag{2.4}$$

Remark 1. Follows immediately from the discussion above. □

Proposition 1. By definition, $Pr_F(|\nu_\ell| \geq k) = F(-k) + (1 - F(k))$. By symmetry of $F(\cdot)$, $Pr_F(|\nu_\ell| \geq k) = 2(1 - F(k)) = 2F(-k)$. Then our assumption that $Pr(|\nu_\ell| \geq k)$ increases for all $k > 0$ implies that $F_\ell(k)$ increases for all $k < 0$, and decreases for all $k > 0$. Maintaining the symmetry and mean-zero assumptions on $F_\ell(\cdot)$ requires that $F_\ell(0) = 0.5$. This means that $F_\ell(Q_\ell + Q_m)$ increases if and only if $Q_\ell + Q_m < 0 \iff Q_m < -Q_\ell$.

For clarity of exposition, suppose that the assumed change in the CDF happens to candidate 2. Recall that U_m (which captures how 'likely' an MP is to vote for candidate 2) is increasing in $F_2(Q_2 + Q_m)$. Finally, $F_2(Q_2 + Q_m)$ increases if and only if $Q_m < -Q_2$. So the MP is 'more likely' to vote for candidate 2 if and only if $Q_m < \bar{Q}$ (where $\bar{Q} = -Q_2$). The steps are the same when the assumed change in the CDF happens to candidate 1.[26] □

Proposition 2. It is clear that $\frac{\partial U_m}{\partial I_2} > 0$ and $\frac{\partial U_m}{\partial I_1} < 0$. So an increase in I_2 [resp. I_1] can only induce MPs to switch towards [resp. away from] candidate 2. □

[25] To see this: $P_m(Q_m + Q_\ell + \nu_\ell > 0) = 1 - P_m(\nu_\ell < -Q_m - Q_\ell) = 1 - F_\ell(-Q_m - Q_\ell) = F_\ell(Q_m + Q_\ell)$ using the fact that $F_\ell(-x) = 1 - F_\ell(x)$ by the symmetry of the distribution.

[26] There is an extra step: an increase in $F_1(Q_1 + Q_m)$ reduces U_m – making the MP 'less likely' to vote for candidate 2, and hence more likely to vote for candidate 1.

Proposition 3. Recall that $s_m^* = 2$ if and only if $U_m \geq 0$, which is equivalent to

$$F_2(Q_m + Q_2) - F_1(Q_m + Q_1) \geq \frac{1}{R_m} (I_{1,m} - I_{2,m}) . \tag{2.5}$$

If MPs $m \in M'$ find candidate 1 more ideologically appealing, then $I_{1,m} - I_{2,m} > 0$. So an increase in R_m decreases RHS of Equation (2.5). This must weakly increase the number of MPs who vote for candidate 2. Conversely, if MPs $m \in M'$ find candidate 1 more ideologically appealing, then $I_{1,m} - I_{2,m} < 0$. So an increase in R_m increases RHS of Equation (2.5). This must weakly decrease the number of MPs who vote for candidate 2. □

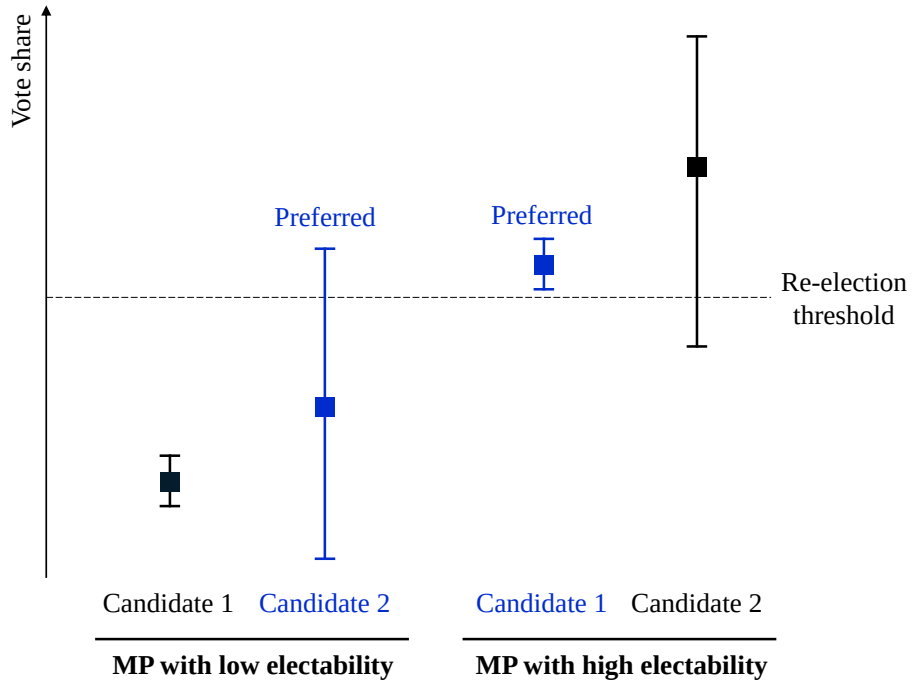
Proposition 4. **(i)** It is clear that $\frac{\partial U_m}{\partial Q_2} = R_m f_2(Q_m + Q_2) \geq 0$ and $\frac{\partial U_m}{\partial Q_1} - R_m f_2(Q_m + Q_2) \leq 0$. So an increase in Q_2 [resp. Q_1] can only induce MPs to switch towards [resp. away from] candidate 2.

(ii) We have $\frac{\partial U_m}{\partial Q_m} = R_m[f_2(Q_m + Q_2) - f_1(Q_m + Q_1)]$. Then use the assumption: $F_2(x) = \alpha F_1(x) + (1-\alpha)F_E(x)$ for some $\alpha \in [0, 1)$. This implies $f_2(x) = \alpha f_1(x) \leq f_1(x)$ since $\alpha < 1$, except at $x \in \{-\tau, \tau\}$.[27] Finally, use the assumption that $Q_1 = Q_2$, and let $x = Q_m + Q_1$. Then we have $[f_2(Q_m + Q_2) - f_1(Q_m + Q_1)] = \alpha f_1(x) - f_1(x) \leq 0$. Therefore, $\frac{\partial U_m}{\partial Q_m} \leq 0$. □

[27] That the result is stated 'generically' means we can ignore these two points.

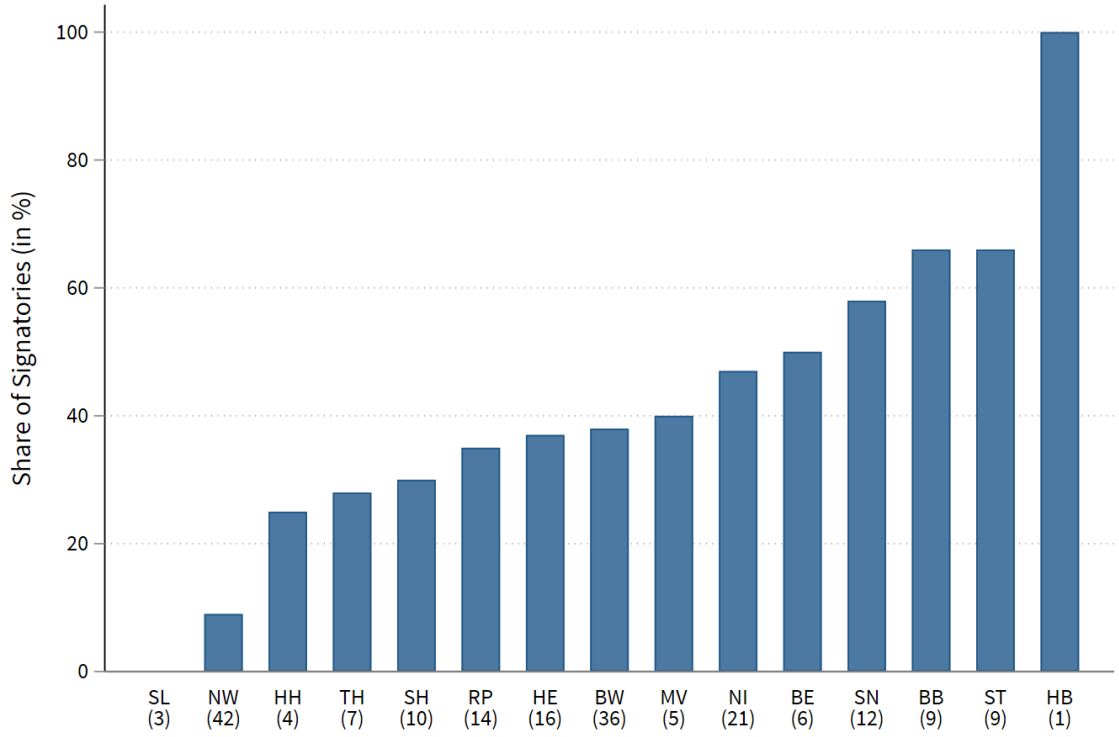
2.B Supplementary Figures

Figure 2.B.1: GAMBLING FOR RE-ELECTION BEHAVIOUR BY HIGH ELECTABILITY MPS



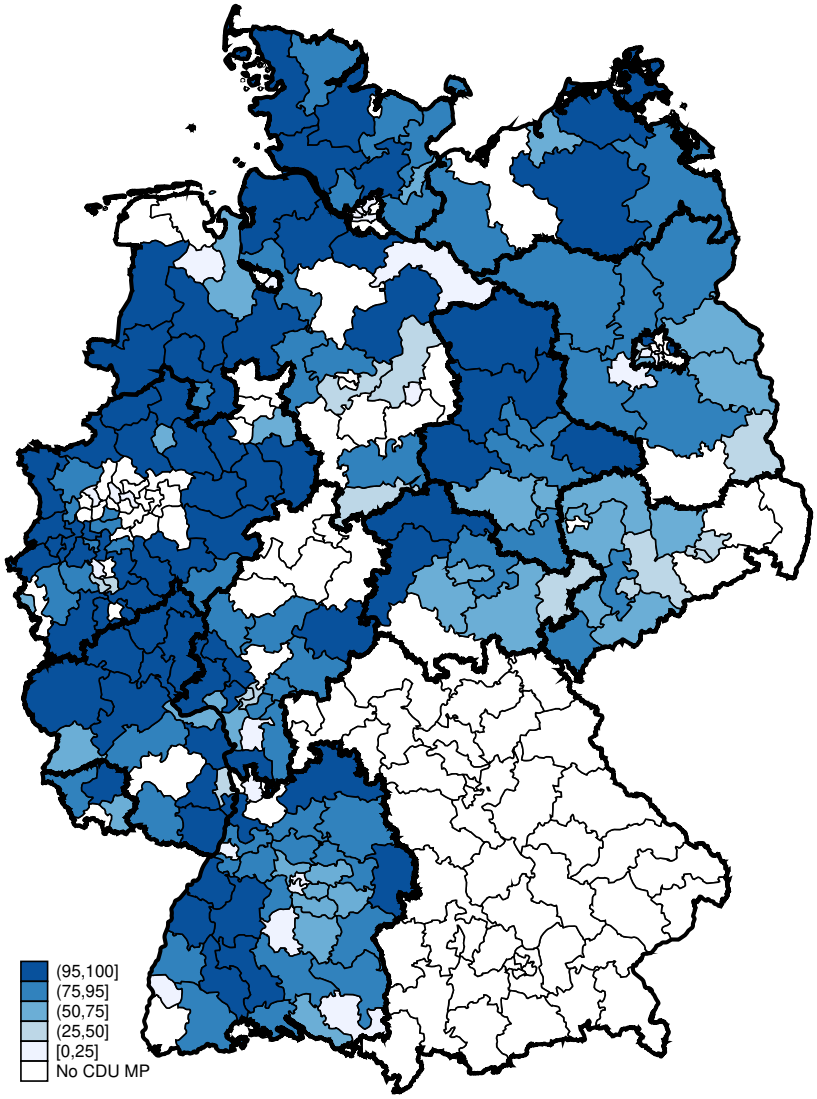
Notes: The figure illustrates for two MPs the range of potential vote shares under two leadership candidates. The MP on the left has a lower re-election probability Q_m than the MP on the right. The leadership candidates differ both in electability and riskiness. A higher electability is illustrated by the solid square being further to the top and a higher riskiness is illustrated by a larger distance between the solid square and the whiskers. Here, candidate 1 is less electable ($Q_1 < Q_2$) and less risky ($\lambda_1 < \lambda_2$) than candidate 2. The MP with high electability gambles for re-election by choosing candidate 1 who compensates the lower electability with lower riskiness. The case where candidate 1 is more electable, but less risky is shown in Figure 2.1.

Figure 2.B.2: SHARE OF SÖDER SUPPORTERS IN GERMAN STATES



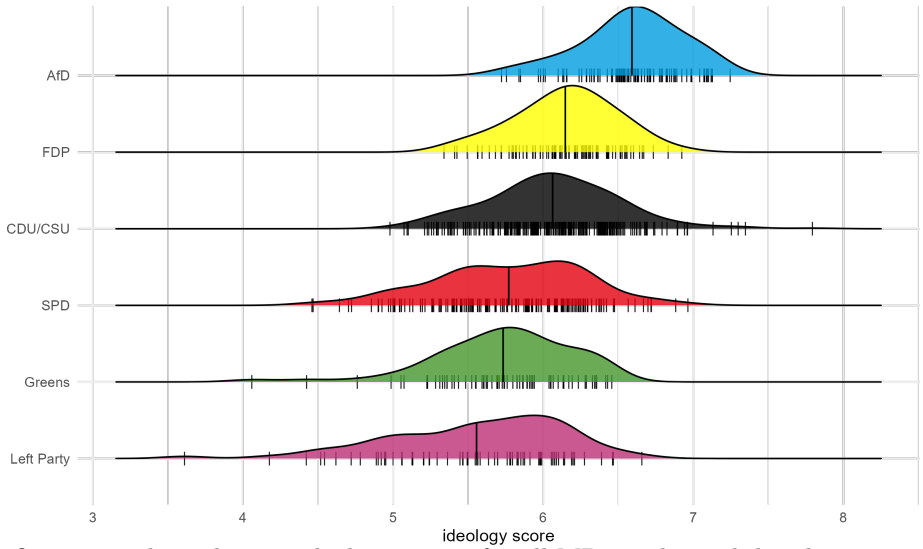
Notes: The figure shows the share of CDU MPs who signed the letter in the 15 German states (excluding Bavaria). The number in parentheses shows the total number of CDU MPs in the states. Data comes from the FAZ.
SL: Saarland, NW: North Rhine-Westphalia, HH: Hamburg, TH: Thuringia, SH: Schleswig-Holstein, RP: Rhineland-Palatinate, HE: Hesse, BW: Baden-Württemberg, MV: Mecklenburg-Vorpommern, NI: Lower Saxony, BE: Berlin, SN: Saxony, BB: Brandenburg, ST: Saxony-Anhalt, HB: Bremen.

Figure 2.B.3: PREDICTED WINNING PROBABILITIES OF CDU MPS



Notes: The figure shows the predicted CDU winning probabilities for the personalized vote in each constituency from *election.de* on 9th April 2021. Constituencies shaded in white indicate that no elected CDU MP had been running there. CSU MPs (Bavaria) are excluded. The highlighted lines represent state boundaries.

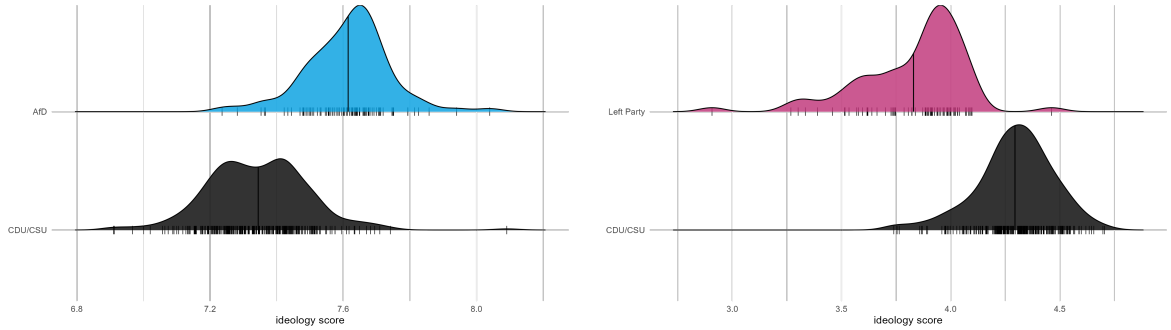
Figure 2.B.4: FACE VALIDITY: VISUALISATION OF IDEOLOGY ESTIMATES



Notes: The figure visualises the raw ideology scores for all MPs in the 19th legislative period derived from our supervised machine learning model. The figure highlights that the model accurately captures ideology across parties. The distributions of left-wing parties (Left party, Greens, SPD) are to the left of the spectrum, while more right-wing parties (CDU/CSU, FDP, AfD) are to the right of the spectrum.

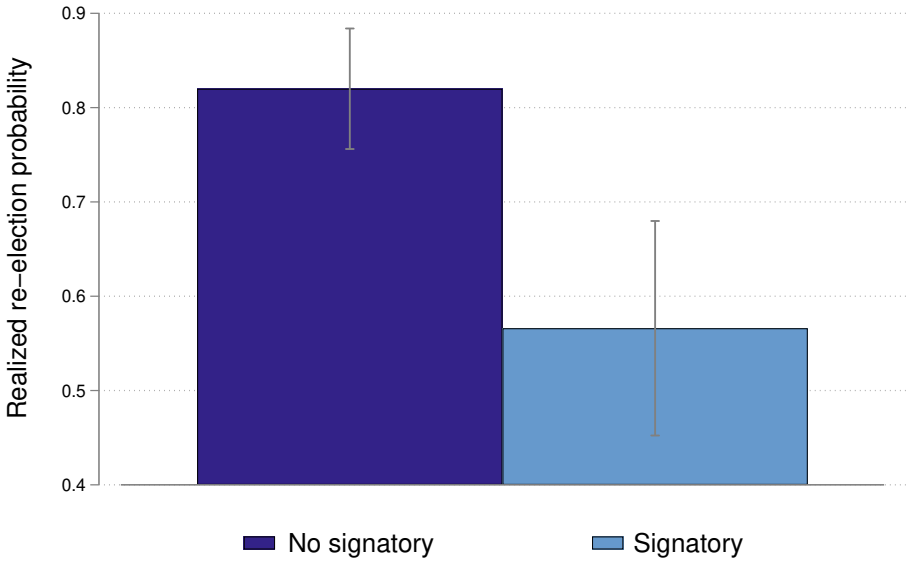
Figure 2.B.5: VALIDATION EXERCISE: PARTY PAIRWISE COMPARISONS

(a) CDU/CSU MPs versus AfD MPs **(b) CDU/CSU MPs versus Left MPs**



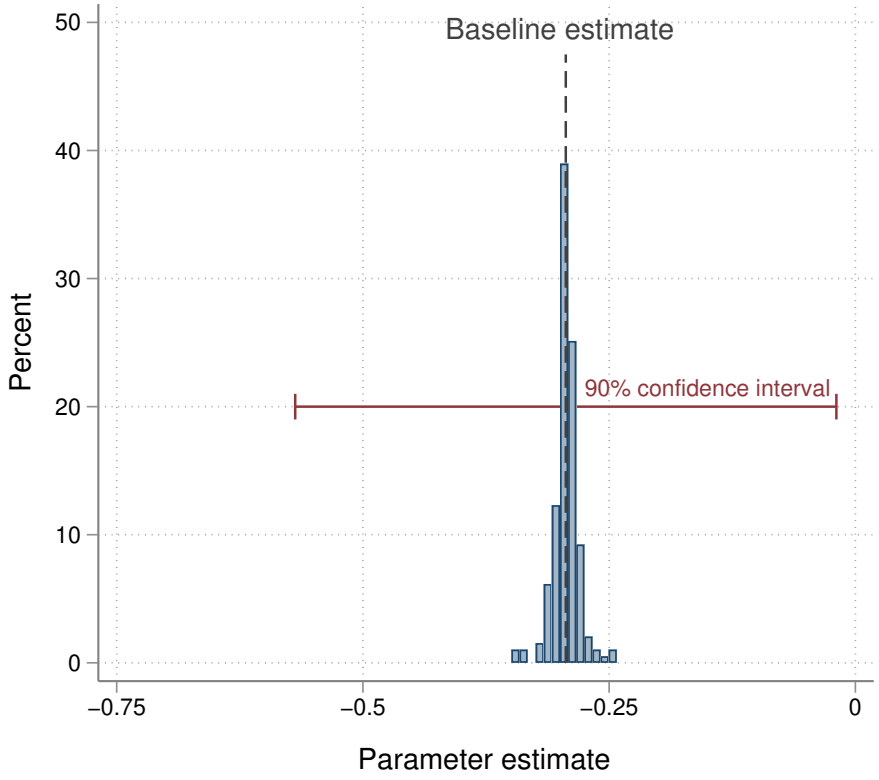
Notes: The figure visualises our validation exercise, in which we restrict the sample in panel (a) to MPs from the CSU/CDU and the AfD and in panel (b) to MPs from the CSU/CDU and the Left party. The resulting distributions show that the model can meaningfully differentiate between MPs from the respective two parties.

Figure 2.B.6: REALISED ELECTION OUTCOMES AND SUPPORT FOR RISKIER CANDIDATE



Notes: The figure shows the average likelihood to getting re-elected in the 2021 national elections (ex post) depending on MPs' support for the riskier candidate.

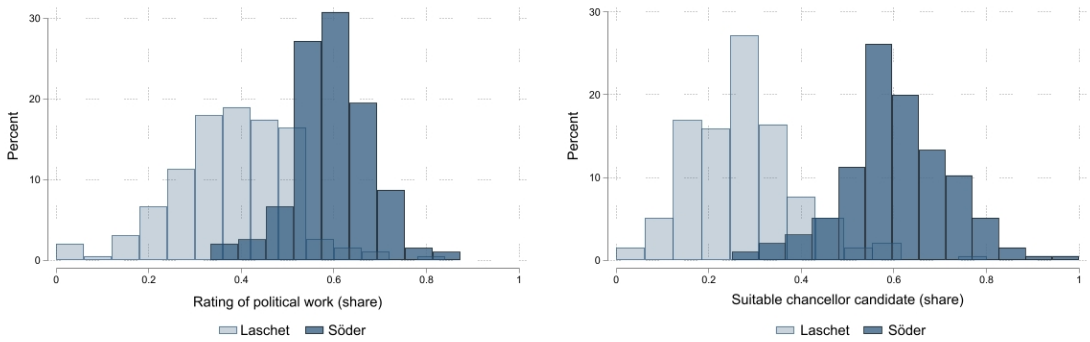
Figure 2.B.7: HISTOGRAM OF PARAMETER ESTIMATES USING JACK-KNIFE REGRESSIONS



Notes: The figure shows a histogram of parameter estimates on the *Polls* variable using jack-knife regressions, where we exclude one MP at a time ('leave-one-out'). The dotted vertical line represents our baseline estimate in Table 2.1, Column (V). All parameter estimates are well within the 90% confidence interval of the baseline estimate.

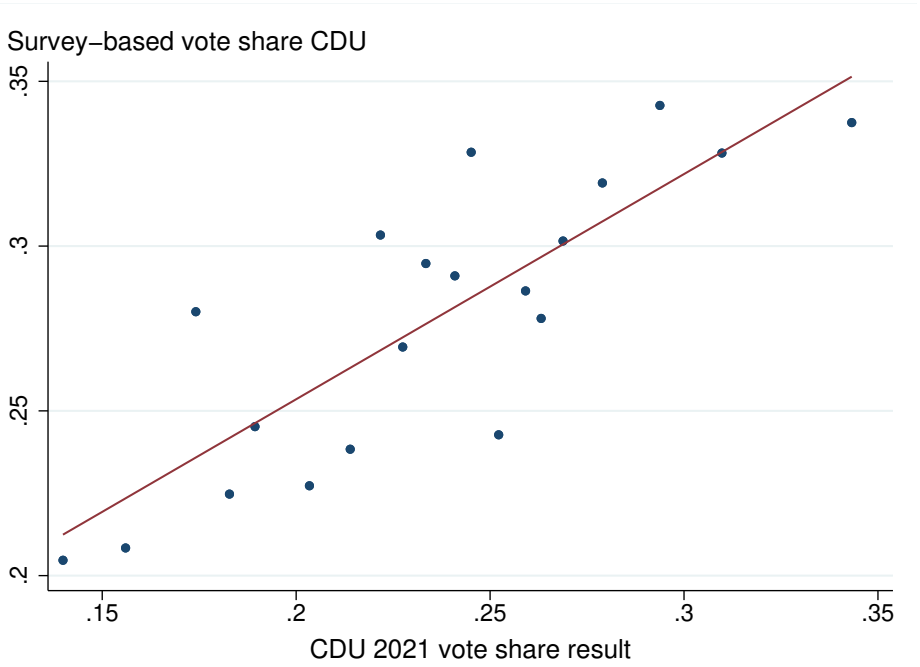
Figure 2.B.8: DISTRIBUTION OF VOTERS' PREFERENCES ACROSS CONSTITUENCIES

(a) Quality of political work **(b) Suitability as chancellor**



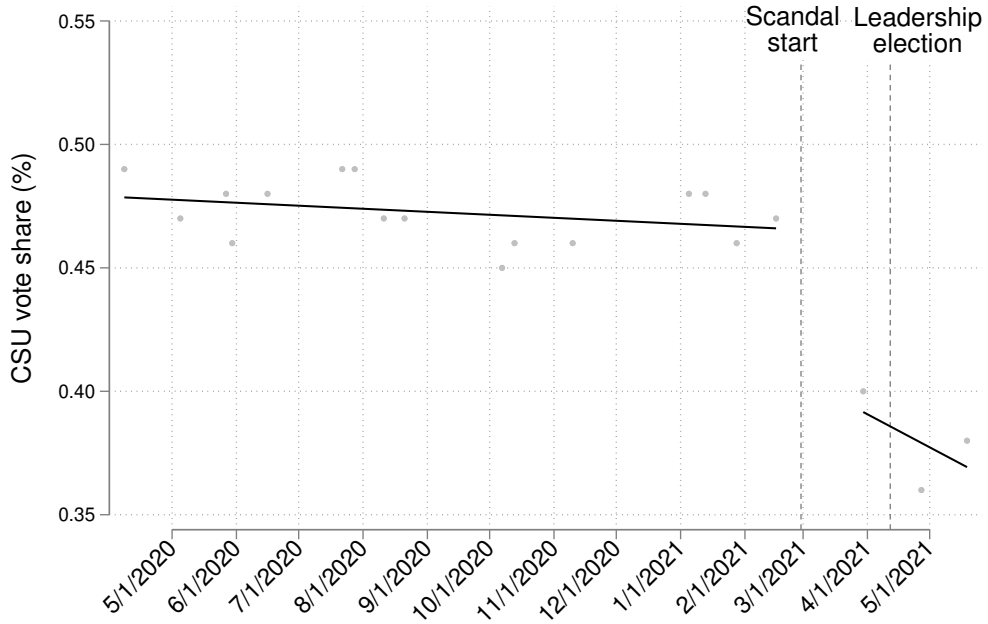
Notes: The figure shows distributions of our two measurements of voters' preferences. Subfigure (a) displays the distribution of the share of survey participants within a constituency who are satisfied with the candidates' political work. Subfigure (b) displays the distribution of the share of survey participants within a constituency who view Markus Söder and Armin Laschet as a suitable chancellor candidate.

Figure 2.B.9: SURVEY-BASED PARTY SHARES AND 2021 REALISED VOTE SHARES



Notes: The figure plots the survey-based vote shares for the CDU against the realised vote shares in the 2021 national elections.

Figure 2.B.10: SCANDAL AND CSU POLLING VOTE SHARES



Notes: The figure plots CSU polling data in Bavaria in the year before the leadership election.

2.C Supplementary Tables

Table 2.C.1: DESCRIPTIVE STATISTICS

Variable	Observations	Mean	Std. dev.	Min	Max
Signatory	195	0.359	0.481	0	1
Poll	195	0.740	0.306	0	1
Ideology	195	0	1	-2.276	4.102
Not-run	195	0.215	0.412	0	1
Shock	195	0.203	0.217	0	0.86
Minimal distance (in 100km)	195	1.360	0.880	0.057	3.740
	MP-level controls:				
Tenure	195	11.581	7.246	1.4	48.4
Education_{low}	195	0.154	0.361	0	1
Education_{high}	195	0.661	0.475	0	1
Education_{PhD}	195	0.185	0.389	0	1
Female	195	0.221	0.416	0	1
Party elite	195	0.164	0.371	0	1
Party assistance	195	0.195	0.397	0	1
PKM faction member	195	0.662	0.474	0	1
Religious affiliation (1=protestant)	195	0.452	0.499	0	1
Roll-call vote share against party line (in %)	195	0.843	1.243	0	7.477
	Constituency-level controls:				
AfD sec. vote share in 2017	195	0.128	0.058	0.049	0.329
Unemployment rate (in %)	195	6.3	2.0	2.9	15.8
Population density	195	747	1,092	36.8	6,476
Avg. private HHs' income	195	22,654	2,296	16,450	32,099
Rating_{AL}: Political work	195	0.381	0.126	0	0.80
Rating_{MS}: Political work	195	0.597	0.083	0.33	0.87
Rating_{AL}: Chancellor suitability	195	0.273	0.114	0	0.80
Rating_{MS}: Chancellor suitability	195	0.607	0.115	0.25	1

Notes: The table shows descriptive statistics of the variables used in our empirical analysis. The dummy for the party elite takes the value of one for members of the government (cabinet and parliamentary state secretaries) and for members of the CDU board.

Table 2.C.2: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS: PROBIT MODEL

Dependent variable: Support for Söder					
	(I) Parsi.	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
	Panel A: Probit Regression Results				
Poll	-0.760**	-1.004**	-1.018*	-1.019*	-1.095**
	(0.336)	(0.450)	(0.547)	(0.551)	(0.536)
$Not\text{-}run \times Poll$	0.132	0.407	1.051	1.045	0.668
	(0.749)	(0.877)	(1.032)	(1.030)	(1.031)
Ideology				0.027	-0.159
				(0.134)	(0.151)
$Not\text{-}run \times Ideology$					0.802*
					(0.413)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
Pseudo R-squared	0.026	0.228	0.291	0.291	0.313
	Panel B: Marginal Effects				
$Poll_{Running}$	-0.273**	-0.283**	-0.264*	-0.264*	-0.280**
	(0.115)	(0.123)	(0.139)	(0.140)	(0.135)
$Poll_{Not\ running}$	-0.237	-0.179	0.009	0.007	-0.106
	(0.242)	(0.255)	(0.277)	(0.277)	(0.262)
Ideology				0.007	
				(0.035)	
$Ideology_{Running}$					-0.041
					(0.038)
$Ideology_{Not\ running}$					0.160*
					(0.087)
Equal. (p-val)	0.75	0.39	0.01	0.01	0.19

Notes: The table shows the results from estimating Equation (2.3) with a Probit model. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of $Not\text{-}run$. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD). 'Equal. (p-val)' reports p-values on a Wald test for equality of the estimated marginal effect of $Poll_{Running}$ and the marginal effect of $Poll_{Not\ running}$. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 2.C.3: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS EAST-WEST DUMMY

Dependent variable: Support for Söder					
	(I) Parsi.	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.285**	-0.261*	-0.266*	-0.268*	-0.273*
	(0.128)	(0.139)	(0.139)	(0.141)	(0.140)
	(0.229)	(0.210)	(0.215)	(0.214)	(0.213)
Not-run × Poll	0.042	0.035	0.064	0.063	0.013
	(0.290)	(0.273)	(0.280)	(0.277)	(0.270)
Ideology				0.018	-0.010
				(0.031)	(0.031)
Not-run × Ideology					0.160
					(0.098)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
East-Dummy	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.034	0.244	0.247	0.249	0.262

Notes: The table shows the results from estimating Equation (2.3) when, instead of using state fixed effects, we control for geographical and cultural differences via a dummy whether the MPs run in East- or West-Germany. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.4: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS MEASUREMENT

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.280**	-0.256*	-0.286*	-0.286*	-0.291*
	(0.129)	(0.140)	(0.169)	(0.169)	(0.166)
Not-run × Poll	0.039	0.041	0.162	0.163	0.094
	(0.286)	(0.272)	(0.309)	(0.310)	(0.309)
Ideology				-0.002	-0.039
				(0.038)	(0.039)
Not-run × Ideology					0.185*
					(0.098)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.039	0.247	0.310	0.310	0.327

Notes: The table shows the results from estimating Equation (2.3) when we additionally include the standard deviation of our ideology measurement. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.5: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS COVID-19

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.275**	-0.281**	-0.293*	-0.294*	-0.299*
	(0.131)	(0.138)	(0.169)	(0.171)	(0.168)
Not-run × Poll	0.039	0.071	0.169	0.165	0.093
	(0.290)	(0.275)	(0.307)	(0.306)	(0.305)
Ideology				0.008	-0.024
				(0.034)	(0.036)
Not-run × Ideology					0.174*
					(0.099)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.037	0.252	0.308	0.308	0.323

Notes: The table shows the results from estimating Equation (2.3) when we additionally include measurements for the levels of Covid-19 cases in the constituencies. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.6: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS STATE LISTS

Dependent variable: Support for Söder					
	(I) Parsi.	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
	Panel A: Controlling for 2017 state lists placement				
Poll	-0.285**	-0.260*	-0.301*	-0.302*	-0.306*
	(0.128)	(0.139)	(0.166)	(0.168)	(0.164)
Not-run × Poll	0.042	0.031	0.138	0.135	0.068
	(0.290)	(0.272)	(0.307)	(0.306)	(0.305)
Ideology				0.006	-0.025
				(0.035)	(0.037)
Not-run × Ideology					0.171*
					(0.099)
	Panel B: Controlling for 2021 state lists placement				
Poll	-0.285**	-0.264*	-0.285*	-0.287*	-0.291*
	(0.128)	(0.140)	(0.170)	(0.172)	(0.168)
Not-run × Poll	0.042	0.031	0.149	0.146	0.073
	(0.290)	(0.273)	(0.309)	(0.308)	(0.308)
Ideology				0.009	-0.024
				(0.034)	(0.036)
Not-run × Ideology					0.176*
					(0.098)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
List dummy	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195

Notes: The table shows the results from estimating Equation (2.3) when additionally controlling for whether MPs were placed on state lists in 2017 (Panel A) or in 2021 (Panel B). The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.7: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ACCOUNTING FOR VOTERS' PREFERENCES

Dependent variable: Support for Söder				
	Baseline	*+ Cand. pref.*	*Dif. in Pref. (AL-MS)*	*+ Cand. and party pref.*
	(1)	(2)	(3)	(4)
Poll	-0.294*	-0.317*	-0.287*	-0.277
	(0.167)	(0.173)	(0.167)	(0.181)
Not-run × Poll	0.077	0.157	0.106	0.153
	(0.307)	(0.322)	(0.309)	(0.318)
Ideology	-0.023	-0.022	-0.021	-0.027
	(0.036)	(0.036)	(0.036)	(0.037)
Not-run × Ideology	0.175*	0.173*	0.161	0.175*
	(0.098)	(0.103)	(0.102)	(0.103)
Rating Pol. Work$_{AL}$		-0.421*		-0.438*
		(0.252)		(0.254)
Suit. Chancellor$_{AL}$		0.424		0.433
		(0.325)		(0.326)
Rating Pol. Work$_{MS}$		0.241		0.326
		(0.293)		(0.311)
Suit. Chancellor$_{MS}$		0.161		0.182
		(0.293)		(0.298)
Rating Pol. Work$_{Dif.}$			-0.342	
			(0.213)	
Suit. Chancellor$_{Dif.}$			0.113	
			(0.212)	
CDU sec. vote share				-0.787
				(0.578)
Constituency controls	Yes	Yes	Yes	Yes
MP controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Leadership pref.	Yes	Yes	Yes	Yes
Party pref.				Yes
Observations	195	195	195	195
R-squared	0.322	0.338	0.332	0.346

Notes: The table shows the results from estimating Equation (2.3) when accounting for voters' preferences by including (1) the share of voters who are satisfied with the quality of Markus Söder/Armin Laschet's political work, (2) the share of voters who consider Markus Söder/Armin Laschet to be a suitable chancellor candidate, and (3) the party preferences of constituents. All columns show results when estimating the full specification of Equation (2.3). MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.8: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — TESTING FOR EXPRESSIVE VOTING

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.221*	-0.277**	-0.250	-0.253	-0.270
	(0.120)	(0.138)	(0.175)	(0.177)	(0.175)
Run-right × Poll	-0.395	0.142	0.070	0.075	0.064
	(0.344)	(0.356)	(0.392)	(0.395)	(0.388)
Ideology				0.010	-0.024
				(0.035)	(0.037)
Not-run × Ideology					0.182*
					(0.098)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.057	0.230	0.296	0.296	0.313

Notes: The table shows the results when interacting the Poll measure with a dummy whether the runner-up of the CDU MP is a right-wing candidate (from the AfD). We show results from five specifications. We start with a parsimonious model that examines how MPs' electability (*Poll*), and the interaction of electability with the runner-up dummy, relate to MPs' leadership choices. We gradually augment this model by introducing MP- and constituency- controls (Column II), state fixed effects (Column III), ideology (Column IV) and the interaction term of ideology with the re-election motive (Column V). All specifications also include the base effect of *Run-right*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate.

Table 2.C.9: PREDICTED WINNING LIKELIHOOD AND CANDIDATE CHOICE — ROBUSTNESS PARTY ASSISTANCE

Dependent variable: Support for Söder					
	(I) Parsimonious	(II) + Controls	(III) + State f.e.	(IV) + Ideology	(V) + Ideology Int.
Poll	-0.283**	-0.273*	-0.279*	-0.281*	-0.287*
	(0.126)	(0.140)	(0.168)	(0.170)	(0.166)
Not-run × Poll	-0.005	0.011	0.138	0.133	0.058
	(0.300)	(0.281)	(0.313)	(0.311)	(0.307)
Ideology				0.011	-0.023
				(0.034)	(0.036)
Not-run × Ideology					0.184*
					(0.098)
Party assistance	-0.219***	-0.122*	-0.116	-0.117	-0.130
	(0.075)	(0.073)	(0.082)	(0.082)	(0.080)
Constituency controls	No	Yes	Yes	Yes	Yes
MP controls	No	Yes	Yes	Yes	Yes
State fixed effects	No	No	Yes	Yes	Yes
Observations	195	195	195	195	195
R-squared	0.066	0.253	0.314	0.314	0.331

Notes: The table shows the results from estimating Equation (2.3) when we additionally control for dummies whether MPs received party assistance in the 2021 elections campaign. The specifications in Columns (I)-(V) follow the structure in Table 2.1, and control for the base effect of *Not-run*. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

Table 2.C.10: RE-ELECTION SHOCK AND CANDIDATE CHOICE — INSTRUMENTAL VARIABLE REGRESSION RESULTS ROBUSTNESS

Dependent variable: Support for Söder				
	(I) Baseline	(II) Party pref.	(III) Leader pref.	(IV) Party & leader pref.
	Panel A: First stage			
Distance (in 100km)	-0.120***	-0.117***	-0.111***	-0.108***
	(0.034)	(0.033)	(0.034)	(0.034)
	Panel B: Second stage			
Shock	1.361**	1.325*	1.447*	1.385*
	(0.685)	(0.698)	(0.773)	(0.787)
Ideology	-0.024	-0.027	-0.024	-0.027
	(0.039)	(0.039)	(0.038)	(0.039)
Not-run × Ideology	0.212**	0.214**	0.229**	0.230**
	(0.094)	(0.094)	(0.105)	(0.104)
Constituency controls	Yes	Yes	Yes	Yes
MP controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Observations	195	195	195	195
Kleibergen-Paap F-stat.	12.65	11.96	10.97	10.15
Anderson-Rubin p-val	0.038	0.048	0.044	0.060

Notes: The table shows robustness results for our instrumental variable design when additionally controlling for voters' party support for the CDU (Column II), controlling for voters' preferences over the leadership candidates (Column III), or both measures (Column IV). Both measures are constructed using the infratest dimap survey data. MP controls include MPs' tenure, education levels, religious affiliation, general rebel tendencies, gender, PKM faction membership, and a dummy whether an MP has a leading party position (value of one for members of the government and members of the CDU board). Constituency controls include the unemployment rate, households' average income, and the urbanisation rate, and the share of second votes in the previous 2017 national election for the populist right-wing party 'Alternative für Deutschland' (AfD).

2.D Extension: Party List Elections

An institutional feature of the German electoral system is that voters have two votes. The first vote is to elect an MP in their constituency on a First-Past-The-Post basis. The second vote is for a political party at the national level. These second votes are then used to elect MPs from party lists. In Germany, these lists are created at the state level (so we call them 'state lists' in the main text). MPs are chosen so that the proportion of MPs a political party has in parliament is the same as the proportion of second votes it received nationally. With party lists, voters do not choose a specific person: if party X wins n seats through the second vote, then the top n people on its party list are elected as MPs.

We abstract away from state-level party lists and consider a single party list at the national level. We also assume that an MP stands for election *either* in a constituency *or* through the party list. Both are merely simplifications that help to keep the model clean. Allowing both routes simultaneously would make the model more complex without adding insight.[28] This extended model incorporates two new features. Voters now have two votes – the first for the constituency's MP and the second on national party lists. In addition, MPs can either run in a constituency or through a party list. We contend that these features are a useful first-order approximation of the German electoral system.

Agents. There are four types of agents: a unit mass of voters, indexed j, finitely many *First-Past-The-Post* (FPTP) members of parliament (FPTP MPs), $m \in M = \{1, ..., \mathcal{M}\}$, finitely many *party list* members of parliament (list MPs), $n \in N = \{1, ..., \mathcal{N}\}$ and two leadership candidates, $\ell \in \{1, 2\}$. There is one FPTP MP per constituency (also indexed m), and each voter j is assigned to exactly one constituency. For clarity, we will use $i \in M \cup N$ to refer to an MP where separating FPTP and list MPs is not necessary.

Elections and strategies. Two elections happen sequentially. In the first, all MPs (both FPTP and list) from party X vote in the *leadership election* for leadership candidate 1 or 2. In the second, each FPTP MP $m \in M$ stands for election in a single constituency, each list MP $n \in N$ stands for election on the party list, and each voter j casts two votes in her constituency. One is for a FPTP MP (which we call a *first*

[28] We will see later in this section that MPs' behaviour does not depend on whether they stand for election in a constituency or through the party list. This is at least suggestive that their behaviour would not change much if they were to stand through both routes simultaneously.

vote) and one for a national party (which we call a *second vote*). Combined, they form *national elections*. Abstentions are not allowed, and elections for FPTP MPs are by majority rule.

Each national party receives a number of list MPs proportional to the share of second votes it received. List MPs are chosen according to the party list, from the top downwards. For simplicity, assume the national elections involve only two parties, the MPs' own party (X) and some other party (Y). Each MP from party X can vote for leadership candidate 1 or leadership candidate 2. So strategies for MPs are $s_m = \{1, 2\}$ for all m. Similarly, each voter can vote for party X, or for party Y in both the first vote and in the second vote. So strategies for voters are $s_j = \{X, Y\} \times \{X, Y\}$. We assume that agents naively vote for their most preferred choice in both leadership and national elections.[29]

Endowments and information. Exactly as in Section 2.2.1 (with all parameters endowed to MPs in Section 2.2.1 being endowed to both FPTP and list MPs), with the following addition: each list MP is endowed with a position on the party list equal to her index. This party list position then induces a threshold $T_n \in [0, 1)$, such that a list MP n is elected if and only if the vote share of party X is strictly larger than T_n.

Preferences. The utility of voter j in constituency m when the MP from party X wins is:

$$u_j = Q_m + Q_\ell + \nu_\ell + \epsilon_j, \tag{2.6}$$

and we normalise her utility from the MP from party Y winning to be zero. The rationale for the appearance of Q_m is as in Section 2.2.1. Voter j also receives the following utility if party X wins the overall election:

$$u_j^p = Q_\ell + \nu_\ell + \epsilon_j. \tag{2.7}$$

MPs care about their own re-election and the ideology of their leader. Both of these are specific to individual MPs.[30] But for convenience, we assume the payoff from re-election does not depend on the identity of the leader. This gives preferences:

$$u_i(\ell) = I_{\ell,i} + R_i \cdot Pr_i(\text{win}|\ell), \quad \text{for } \ell \in \{1, 2\}, \tag{2.8}$$

[29] As is standard, this assumes that they never play a weakly dominated strategy.

[30] Note that the electability of individual list MPs does not appear in u_j^p because the voter does not know which MP their vote will help elect.

where R_i is the MP's re-election motivation, $I_{\ell,i}$ the MP's ideological preference for leadership candidate ℓ, and $Pr_i(\text{win})$ is the probability that she is elected.

2.D.1 Results

As in the main text, voters' decisions are deterministic from their own point of view. Voter j casts her first vote for party X (i.e. for MP m) if and only if $u_j > 0$. And she cases her second vote for party X if and only if $u_j^p > 0$. An obvious implication is that a voter may cast her two votes for different parties.

Whether or not a FPTP MP m wins or loses in the national elections depends on the *median* voter in her constituency – who has $\epsilon_j = 0$ by construction. For a list MP n the problem turns out to be similar. Whether or not a list MP $n \in N$ wins re-election depends on whether the *fraction* of voters who cast their second vote for party X is greater than T_n. Therefore it is the voter at the T_n-th percentile (rather than at the median) who is critical for the list MP. And recall that the CDF of ϵ, $G(\cdot)$ is common knowledge. So the critical voter for list MP $n \in N$ has $\epsilon_j = G^{-1}(T_n)$.

Therefore if ν_ℓ is known, then re-election is deterministic for both types of MPs (FPTP and list). But remember that an MP does not observe ν_ℓ at the point she chooses her leader. So the *perceived* probability of winning for a FPTP MP is $F_\ell(Q_m + Q_\ell)$. And for a list MP it is $F_\ell(-G^{-1}(T_n) + Q_\ell)$. This is the only difference between the types of MPs. Consequently, they make qualitatively identical decisions – the only difference being that a FPTP MP cares about her own electability, Q_m, while a list MP cares about her election threshold T_n. They play identical roles. The following result formalises this discussion.

Proposition 5. *There exists a unique equilibrium where:*
(1) $s_j^* = \{\mathcal{X}(u_j), \mathcal{X}(u_j^p)\}$ *where* $\mathcal{X}(u) = X$ *if* $u > 0$ *and* $\mathcal{X}(u) = Y$ *if* $u \leq 0$,
(2) $s_m^* = 2$ *if and only if* $F_2(Q_m + Q_2) - F_1(Q_m + Q_1) > \frac{1}{R_m}(I_{1,m} - I_{2,m})$,
(3) $s_n^* = 2$ *if and only if* $F_2(-G^{-1}(T_n) + Q_2) - F_1(-G^{-1}(T_n) + Q_1) > \frac{1}{R_n}(I_{1,n} - I_{2,n})$.

Proof. **Part (1)** follows trivially from the assumptions of the model. **Part (2)** is identical to the discussion preceding the proof of Remark 1. **Part (3)** Party X's vote share is equal to the probability that a randomly chosen voter j casts her second vote for party X: Vote share$(X) = Pr(Q_\ell + \nu_\ell + \epsilon_j > 0)$. Straightforward algebra yields Vote share$(X) = G(Q_\ell + \nu_\ell)$.[31] This means that list MP n is elected if and

[31] To see this: $Pr(Q_\ell + \nu_\ell + \epsilon_j > 0) = 1 - Pr(\epsilon_j < -Q_\ell - \nu_\ell) = 1 - G(-Q_\ell - \nu_\ell) = G(Q_\ell + \nu_\ell)$, using the fact that $G(-x) = 1 - G(x)$ by the symmetry of the distribution.

only if $G(Q_\ell + \nu_\ell) > T_n$. So $Pr_n(\text{win}|\ell) = Pr(T_n < G(Q_\ell + \nu_\ell))$, which rearranges to $Pr_n(\text{win}|\ell) = F_\ell(Q_\ell - G^{-1}(T_n))$. Therefore $u_n(\ell) = I_{n,\ell} + R_n \cdot F_\ell(Q_\ell - G^{-1}(T_n))$. By assumption $s_n^* = 2$ if and only if $u_n(2) > u_n(1)$. Straightforward rearranging then yields the result. □

The similarity between parts (2) and (3) is immediate. The behaviour of list MPs is identical to that of FPTP MPs except that $-G^{-1}(T_n)$ replaces Q_m. Therefore all subsequent results from Section 2.2.3 will apply unchanged. The only thing to note is that $G^{-1}(\cdot)$ is an increasing function (so $-G^{-1}(\cdot)$ is a decreasing function), so comparative statics found for Q_m will be flipped when considering T_n. This is intuitive. A higher threshold for election (due to a lower position on the party list) makes an MP harder to elect: in other words, less electable.

2.E Extension: Spillovers across Constituencies

The model we present in the main text assumes that there are no spillovers across constituencies. In practice, we might expect two kinds of spillovers between MPs in different constituencies: payoff spillovers and strategic spillovers. The former is where an MP cares about the payoff of other MPs. This could capture altruism, or that an MP might herself benefit if others also win re-election (for example, in helping her secure certain committee positions when in parliament). The latter reflects that an MP's behaviour is directly influenced by the behaviour of others. This could capture coattail effects in a reduced form fashion, or some other type of peer effects.

A priori, we have no reason to expect any particular pattern for either type of these spillovers. So we allow for an extremely rich pattern of possible spillovers by modelling them with a weighted and directed network.[32] In the interest of generality, we also allow the patterns of spillovers to be different for the payoff spillovers and the strategic spillovers. Formally, this takes the form of allowing a different network for each.

Enriching the model. The model is identical to Section 2.2.1 in the main text, apart from the addition of both payoff spillovers and strategic spillovers into MPs' utility functions. Formally, Equation (2.2) now becomes,

$$u_m(\ell) = I_{\ell,m} + R_m \cdot P_m(\text{win}|\ell) + \sum_{n \neq m} \mathcal{F}_{mn} Z_{\ell,n} + \sum_{n \neq m} \mathcal{G}_{mn} \cdot \mathbf{1}\{s_n = \ell\}, \quad \text{for } \ell \in \{1,2\}, \tag{2.9}$$

where $\mathcal{F}$ and $\mathcal{G}$ are $\mathcal{M} \times \mathcal{M}$ non-negative matrices representing networks, and $Z_{\ell,n} = I_{\ell,n} + R_n \cdot P_n(\text{win}|\ell)$. In line with our assumption in Section 2.2.1 that MPs vote naively, also assume that they vote as if they are pivotal. This means that they vote as if they can choose between $Z_{1,n}$ and $Z_{2,n}$ for all $n \in M$.

2.E.1 Analysing the richer model

A preliminary step. First, it is easier to work with $U_m = u_m(2) - u_m(1)$. This is exactly how we proceed in the proofs to the main text results. It is also helpful here

[32] A network is a collection of agents – here MPs – and a collection of links between them. Here the links capture the strength of the spillover. These links can take on any non-negative weight, and need not be symmetric (i.e. the spillover from MP m to m' can be very different to the spillover in the other direction, from m' to m.

to write $x_n = \mathbf{1}\{s_n = 2\}$. So $x_n = 1$ if $s_n = 2$ and $x_n = 0$ if $s_n = 1$. So we have:

$$U_m = (I_{2,m} - I_{1,m}) + R_m \cdot (P_m(\text{win}|2) - P_m(\text{win}|1)) + \sum_{n \neq m} \mathcal{F}_{mn}(Z_{2,n} - Z_{1,n}) \quad (2.10)$$
$$+ \sum_{n \neq m} \mathcal{G}_{mn}(2x_n - 1)$$

It is immediate that an MP will vote for candidate $\ell = 2$ if and only if $U_m \geq 0$. So we have $x_m^* = 1 \iff U_m \geq 0$. Simple rearranging shows that we have $U_m \geq 0$ if and only if:

$$\sum_{n \neq m} \mathcal{G}_{mn} x_n \geq \frac{1}{2} \left(Z_{1,m} - Z_{2,m} + \sum_{n \neq m} \mathcal{F}_{mn}(Z_{1,n} - Z_{2,n}) + \mathcal{D}_m \right), \quad (2.11)$$

where $\mathcal{D}_m = \sum_{n \neq m} \mathcal{G}_{mn}$ – this is called an agent's *degree* in the network $\mathcal{G}$.[33]

A threshold game. We can now see that an agent takes the action $x_m = 1$ if the (weighted) number of her neighbours – the term on the Left-Hand Side of eq. (2.11) – exceeds some threshold – the term on the Right-Hand Side of eq. (2.11).[34] For convenience, denote the threshold (the Right-Hand Side of eq. (2.11)) k_m for all $m \in M$. In games of this type it is well-known that there is a complete lattice equilibria: including a minimal and a maximal.

Remark 2. *There always exists a minimal and a maximal Nash equilibrium. The set of Nash Equilibria of the game form a complete lattice under set inclusion.*

Proof. This is a restatement of the proof to Remark 1 in Langtry et al. (2024b) (with appropriate adjustments to the notation). Let $X = \{x_m : m \in M\} = \{0, 1\}^{\mathcal{M}}$, and $\geq$ be the element-wise greater than relationship. It is well-known that these form a complete lattice. Notice that agent m's best response function is $BR_m(x_{-m}) = 1 \iff \sum_{n \neq m} \mathcal{G}_{mn} x_n \geq k_m$. Now define the function $f : X \to \mathbb{R}$ as $f(\{i : x_m = 1\}) = \{i : BR_m(\mathbf{x}_{-m}) = 1\}$. The function is isotone because it is clear that for all m, $BR_m(\mathbf{x}_{-m})$ is increasing in x_n for all n. The result then follows directly from Tarski's Fixed Point Theorem. □

[33] Formally, it is a weight out-degree. See Jackson (2008) for an excellent textbook treatment of network theory and more detail on terminology.

[34] We say that two MPs, m and n are *neighbours* in the network $\mathcal{G}$ if $\mathcal{G}_{mn} > 0$. It simply means they are linked.

Of greater interest to us are the comparative statics of the model. We proceed in two steps. First, we show the comparative statics directly in terms of the k_m's. This result is an application of Milgrom and Shannon (1994, Theorem 13). After that, we will link the parameters of our model to the k_m's. In both instances, we focus on the minimal and the maximal equilibria of the game.

Remark 3. *Let x^{MIN} and x^{MAX} denote the action profiles in the minimal and maximal equilibria respectively. x_m^{MIN} and x_m^{MAX} are weakly decreasing in k_n, for all $m, n \in M$.*

Proof. First, notice that we can write $U_m = -k_m + \frac{1}{2}\sum_{n \neq m} \mathcal{G}_{mn} x_n$. It is clear that an increase in k_m decreases U_m, which can only decrease x_m. Then the strategic complementarities – that each agent's action is increasing in the actions of all others – mean that a decrease in x_m can only decrease the actions of all other others $n \in M$ (see Langtry et al. (2024b, Remark 2 and its proof) for further discussion). □

The effects of parameters. Given the machinery and results above, all that remains is to understand how our various parameters of interest impact the thresholds k_m. As before, we begin with the effect of a candidate's 'riskiness'. Here, it is convenient to state the result in a somewhat more constrained case – where *all* MPs have their electability either above or below a common threshold.

Remark 4. *Suppose that $Pr(|\nu_2| \geq k)$ weakly increases for all $k > 0$. If $Q_m \leq \bar{Q}$ for all m, k_m increases for all m. And if $Q_m > \bar{Q}$ for all m, k_m decreases for all m.*

Proof. That an increase in $Pr(|\nu_2| \geq k)$ increases $F_2(Q_m + Q_2)$ if and only if $Q_m \leq \bar{Q}$ is shown in the proof to Proposition 1. This then increases $Z_{2,m}$, which reduces k_m. The requirement that $Q_n \leq \bar{Q}$ for all n means that the same thing is true for all n. So $Z_{2,n}$ also increases for all n. This also reduces k_m (recall that the network structure means that an MP cares about her own re-election probability, and the re-election probabilities of others). So k_m unambiguously falls following this increase in risk. This argument holds for all m. □

So when MPs all have 'low electability', more of them will vote for candidate 2 if candidate 2 becomes riskier. And conversely, when MPs all have 'high electability'. This extends the headline intuition from the main text – that MPs prefer risk when they have low electability, and prefer safety when they have high electability – to a setting with complex patterns of spillovers across constituencies.

Next, we turn to the impact of ideological alignment, leadership candidate electability, and re-election motives.

Remark 5. *For all m, n, the threshold k_m is:*
(i) strictly increasing in $I_{1,n}$, and strictly decreasing in $I_{2,n}$,
(ii) strictly increasing in Q_1, and strictly decreasing in Q_2,
(iii) strictly decreasing in R_n if and only if $F_1(Q_n + Q_1) - F_2(Q_n + Q_2) < 0$.

Proof These follow immediately from the definition of the threshold k_m as the Right-Hand Side of eq. (2.11). □

Combining Remarks 3 and 5 shows us that, in both the minimal and the maximal equilibrium, more agents vote for candidate 2 when: (i) some MPs become more ideologically aligned to candidate 2, (ii) candidate 2 becomes more electable, or (iii) some MPs who have better re-election prospects under candidate 2 care more about re-election. Notice that this third feature is now phrased differently to the analogous result in the main text – Proposition 3. Adding spillovers to the model means that it is possible that an MP m is more ideologically inclined towards candidate 2 *and* has better re-election prospects under candidate 2, but still prefers to vote for candidate 1.

The final component of the model is an MP's own electability, Q_m. As before we work here under an additional assumption that the risky candidate has a similar risk profile to the safe candidate, *except* that extreme outcomes are more likely. To recap, let F_E be the CDF of a distribution with point mass of 0.5 at τ and at $-\tau$, where $\tau = \inf\{t : F(t) = 1\}$. And assume that $F_2(x) = \alpha F_1(x) + (1 - \alpha)F_E(x)$ for some $\alpha \in (0, 1]$. In a world with strategic interactions, the result from Proposition 4 does not change: an increase in electability pushes MPs towards the safer candidate.

Remark 6. *Suppose $F_2(x) = \alpha F_1(x) + (1-\alpha)F_E(x)$ for some $\alpha \in [0, 1)$, and $Q_1 = Q_2$. If an MP m becomes more electable (i.e. Q_m rises), then generically all MPs become* less *inclined to vote for the risky candidate ($\ell = 2$).*

Proof. That $Z_{2,n} - Z_{1,n}$ is decreasing in Q_n follows directly from the proof to Proposition 4. Therefore U_m is weakly decreasing in Q_m for all m, n (and strictly so whenever $\mathcal{F}_{mn} > 0$). This makes MPs less inclined to vote for candidates $\ell = 2$. Some MPs may then change their vote (from 2 to 1). This may induce more MPs to change in the same way too. But due to the strategic complementarities, an MP changing from 2 towards 1 can never induce a change in some other MP's behaviour in the opposite direction. □

2.F Ideology

2.F.1 Validation of the ideology scores

We conduct two exercises to validate the ideology scores derived from our text scaling model. First, we estimate the model for all MPs across all parties. The model accurately captures ideology across parties: The score distributions of MPs from left-wing parties (Left party, Greens, and the SPD) are to the left of the spectrum, while the score distributions of MPs from more right-wing parties (CDU/CSU, FDP, and the AfD) are to the right of the spectrum (see Figure 2.B.4). Second, we perform pairwise comparisons by including label and reference texts only from the CDU/CSU in combination with the Left party or the AfD (both extremes of the ideological spectrum). Even though we reduce the available information, the model continues to meaningfully differentiate between MPs from the two respective parties (see Figure 2.B.5). Further, the correlation of ideology scores for CDU MPs in our baseline model and the pairwise comparisons is strong (0.74 and 0.43). This shows that our model consistently predicts individual ideology scores.

2.F.2 Ideological alignment: Taking the model to data

The model in Section 2.2 works with ideological *alignment* between an MP m and a leadership candidate ℓ, $I_{\ell,m}$. It then finds that the *difference* in ideological alignment, $I_{1,m} - I_{2,m}$, is what matters for MPs' voting behaviour. Working directly with ideological alignment, rather than raw ideology of MPs and leadership candidates separately, is more parsimonious and helps us state the theoretical predictions more cleanly. But only raw ideology, *not* ideological alignment, is available in the data.

Here, we show the one-to-one mapping between raw ideology and the difference in ideological alignment. This demonstrates that using a measure of raw ideology (as we do in Section 2.4) is in fact appropriate given our model.

Let an MP m have a raw ideology $\tilde{I}_m$, and a candidate ℓ have a raw ideology $\hat{I}_\ell$. Then let ideological alignment be defined as $I_{\ell,m} = -(\tilde{I}_m - \hat{I}_\ell)^2$. This gives the difference in ideological alignment as.

$$I_{1,m} - I_{2,m} = -(\tilde{I}_m - \hat{I}_1)^2 + (\tilde{I}_m - \hat{I}_2)^2. \tag{2.12}$$

This shows that the difference in ideological alignment – the object of interest in the theoretical model – is linear in an MP's raw ideology. To see this, simply notice that

$$\frac{d(I_{1,m} - I_{2,m})}{d\tilde{I}_m} = 2(\hat{I}_1 - \hat{I}_2). \tag{2.13}$$

It is clear that this argument extends to any function $I_{\ell,m} = -h(\tilde{I}_m - \hat{I}_\ell)$ that is strictly convex and symmetric about zero. However, in this more general case the difference in ideological alignment will be strictly increasing in $\tilde{I}_m$, but not necessarily linear.

Chapter 3

Experts as Intermediaries

This chapter is based on co-authored work with Klaus Gründler, Michael Lamla, and Niklas Potrafke (see Gründler et al., 2025). ChatGPT (model GPT-4o) was used as a supporting tool to improve the linguistic quality of this chapter.

3.1 Introduction

Households often struggle to understand economic policies, leading to expectations and macroeconomic beliefs that diverge from policymakers' intentions. Such misguided beliefs can distort key economic decisions (Bailey et al., 2021; Dräger and Nghiem, 2021; Coibion et al., 2023a) and ultimately weaken the transmission and effectiveness of policy interventions. Monetary policy, while crucial in shaping inflation and economic activity, is often particularly challenging for households to comprehend (Coibion et al., 2022). As a result, central banks have increasingly redirected their communication efforts from financial markets to the broader public (Ehrmann and Wabitsch, 2022; Dräger, 2023; Blinder et al., 2024). Yet, conventional communication channels often fail to pierce through the 'veil of inattention' (Lamla and Vinogradov, 2019; Fiore et al., 2021). This limitation underscores the need for alternative intermediaries to bridge the gap between policies and the public. By translating policy signals into accessible and understandable insights, professional economists ('economic experts'), who understand complex economic mechanisms and frequently appear in the media, may fill this role. Despite their potential importance, however, evidence on the impact of economic experts remains scarce.

In this paper, we examine the role of economic experts as intermediaries in explaining economic policies to the public, addressing two key questions: (1) How do experts update their beliefs in response to new policy signals, and how does their reaction compare to that of households and firms? (2) Do expert explanations (often referred to as 'narratives') causally shape household expectations and spending decisions? We conduct a series of large-scale cross-national expert surveys as well as experiments among representative household samples to study experts as mediators of economic policies in the high-stakes macroeconomic context created by the 2022–2023 cross-national inflation surge. The sharp rise in inflation, reaching record levels in the United States and Europe, resulted in real income losses for many households and sparked widespread public concern. Competing explanations for the inflation surge dominated media discussions, and, amid this heightened uncertainty, central banks across several countries responded with unprecedented interest rate hikes, exacerbating households' difficulties in grasping the consequences of monetary policy.

We document three main results. The first main result is that economic experts strongly update inflation expectations in response to new monetary policy signals. This reaction is significantly different from that of households and firms. We design an experiment of natural updating to study the causal response to a major monetary policy shock amid the 2022 global inflation surge. On December 14 and 15, the Fed,

the ECB, the Bank of England, the Swiss National Bank, and the Norwegian Central Bank all announced a major increase in main interest rates. Identifying its impact on overnight index swap rates, bond rates, and the STOXX50 index, we document that this shock was significantly stronger than previous monetary policy changes. The ECB's announcement was particularly exceptional, marked by a notably hawkish tone from President Christine Lagarde, along with unexpected forward guidance signaling further interest rate increases in the near future. Our main experiment was conducted simultaneously in the United States, the United Kingdom, Switzerland, Norway, and all Euro area member states. Implemented via the Economic Expert Survey (EES)—the world's largest expert panel—we elicit inflation expectations from leading economic experts without referencing monetary policy, using survey questions identical to those in previous EES quarterly waves. With an average of 6,690 Google Scholar citations and 1,846 Twitter followers, our experts hold substantial influence in both academic circles and the wider public domain. Participants were randomly allocated to a first wave, surveyed shortly before the policy shock, and a second wave, polled immediately afterward (see Boumans et al. (2024) for a similar two-wave setup).

Our experiment reveals that the policy shock reduced inflation expectations of attentive experts by an average of 0.62 percentage points and is similarly effective when we condition on country-level fixed effects and a series of biographic and socio-demographic characteristics of experts. We further show that the effects are not driven by differentials in prior expectations, confounding events, expectations about other macroeconomic fundamental, selection effects, or experts from specific countries or central bank regions. By exploring the anatomy of the treatment effects through open-ended text responses, we find that treated experts are more positive and refer to monetary policy more frequently in their explanations, with no significant impact on any other factor. Comparing the response of experts with those of representative samples of households and firms in an identical natural updating setting, we reveal substantial differences in expectation updating. While both households and firms revise their interest rate expectations upward following the shock, neither group effectively translates these higher rates into lower inflation expectations. Assessing forecast accuracy, we find that experts' updates are objectively more precise.

In a follow-up conducted three months after the main experiment, we assess the effectiveness of the subjective components included in the December 2022 monetary policy shock. Our second main result shows that both the tone of announcement and its forward guidance matter to an approximately equal extent. We also find that previously inattentive experts update their expectations similarly when being informed about these components. This result suggests that differences in updating across atten-

tion levels reflect variations in the available information set, rather than fundamental differences in subjective macroeconomic models used to interpret the economy.

Our cross-national experimental results consistently demonstrate that economic experts respond quickly and naturally to monetary policy shocks, a fundamental prerequisite for their role as intermediaries for households. A second requirement is that they also supply their explanations to the broader public. To quantify this supply and to understand experts' media presence during the 2022-2023 inflation surge, we collaborated with MediaTenor, a global media analysis firm, to collect all media mentions of experts working in one of Germany's largest economic research institutes between 2019 and 2024. We find that expert narratives are referenced an average of 583 times per month, equivalent to roughly 20 media mentions each day. This number is significantly higher compared to the media presence of CEOs working in companies listed in Germany's leading stock market index DAX (cited 232 times per month). Our analysis also uncovers that the frequency of expert explanations related to inflation closely match inflation trends.

Having established that experts routinely provide explanations that incorporate the latest available information and current policy shocks, an important question is how households *respond* to these explanations. Our third main result is that expert narratives significantly shift inflation expectations, increase the accuracy of households' inflation forecasts, and substantially impact households' spending intentions. We obtain these results from information provision experiments, where background information about the ECB's monetary policy change is varied across different treatment conditions. The experiment is conducted with a representative sample of households in Germany, the largest economy within the EMU, where residents exhibit a particularly strong aversion to inflation.

We find no effect on inflation expectations when individuals are informed of an increase in the ECB's main interest rate, mirroring the results for households in our natural updating setting. When we instead present households with expert explanations about the mechanisms of an interest rate increase, we find a significant decrease in expected inflation rates of 0.3 percentage points, relative to an active control group. Providing a quantitative guidance along with the inflation narrative increases the treatment effect on inflation posteriors to 0.67 percentage points. While finding similar results on qualitative posteriors, our treatment did not reduce uncertainty in inflation expectations, indicating that the design generates clean exogenous variation with limited side effects from information provision.

Exploring the consequentiality of expert narratives, we find that the accuracy of inflation forecasts is significantly higher for participants who were provided with an explanation of the mechanisms behind the monetary policy shock. We then use the exogenous shift in inflation expectations created by expert explanations as a first stage to causally estimate their consequences on spending intentions. Consistent with the consumption Euler equation, we find that reduced inflation expectations significantly decrease spending intentions on luxury items, which can be more easily substituted with savings than spending on subsistence goods.

Our results consistently demonstrate that experts can serve as effective intermediaries in communicating economic policies to the broader public. This finding carries several important policy implications. First, relying on experts to explain economic mechanisms could help bridge the knowledge gap between policymakers and households, thereby enhancing the effectiveness of policy. Second, central banks may benefit from incorporating expert intermediaries into their communication strategies, particularly when addressing the broader public. Finally, when experts disseminate information and impact the macroeconomic beliefs of households, they might *themselves* be important agents, a role that has widely been overlooked in the literature. Investigating this role, along with the underlying motives of experts when providing advice, offers a promising avenue for future research.

Contribution to the literature. The main contribution of this paper is to provide comprehensive experimental evidence on the role of professional economists as mediators of economic policies. Unlike professional forecasters, whose main task is to predict inflation, economists—who primarily work in academia—have profound impact on policymaking and public discourse, yet they rarely engage in direct macroeconomic forecasting. Their remuneration is also not dependent on forecasting and therefore they are not prone to any potential biases arising from this incentive structure (e.g., Félix et al., 2021; Gemmi and Valchev, 2023). Understanding how these agents form expectations is hence of first order importance to assess their role as advisors. Our paper connects to the scarce literature that explores the formation of macroeconomic expectations and beliefs of professional economists. This literature has shown that economic experts tend to quickly respond to exogenous shocks such as the Russian invasion of Ukraine (Dräger et al., 2025) or the outcome of the US presidential election (Boumans et al., 2024). Almost nothing is known, however, about the causal response of experts to changes in economic policies. Specifically, how macroeconomic expectations and beliefs are updated in response to monetary policy shocks remains unexplored. We contribute to this literature by showing that experts update inflation expectations sig-

nificantly after a monetary policy shock. Notably, this adjustment occurs in a natural setting, indicating a high degree of responsiveness to new policy signals among experts with a professional background in macroeconomics.

A related literature specifically focuses on inflation updating of households and firms relative to experts as benchmarks (e.g., Link et al., 2023) or in response to receiving expert forecasts. These papers mostly use macroeconomic expectations of economists as a numerical anchor to exogenously shift expectations of households, which, in turn, allows to study the causal impact of expectations on behavior (see e.g., Roth and Wohlfart, 2020; Chopra et al., 2023). Very few papers explore how experts' *explanations* affect the behavior of other agents. For instance, Lee (2022) shows that policymakers update their beliefs about a topic when being informed about scientific results on this topic. Another notable exception is Andre et al. (2023), who show that economic narratives of experts about the causes of inflation impact the narratives of households and can affect their beliefs about inflation. We contribute to this literature in several ways, exploring the effect of expert explanations versus objective (undecoded) information, studying the effect of explanations on individuals' spending decisions, and examining whether expert explanations enhance the accuracy of people's inflation forecasts. Most importantly, we focus on experts' role in explaining a specific economic policy rather than disseminating information about the general macroeconomic drivers of inflation.

Our work also contributes to the broader literature on the determinants of inflation expectations among households and firms (e.g., Dräger et al., 2016; Binder and Rodrigue, 2018; Coibion et al., 2018; Lamla et al., 2019; Binder, 2020; Coibion et al., 2020; Gorodnichenko and Sergeyev, 2021; Weber, 2022; Weber et al., 2022). Those studies have shown that households and firms typically overestimate inflation and tend to react quite sluggishly (if at all) to new information about the state of the world. These regularities are consistent with the reaction found in our natural updating setting for households and firms, but are at stark contrast of the immediate responses of economic experts. We also show that expert explanations are effective in explaining economic policies to households and trigger strong updating effects compared to undecoded information.

Our macroeconomic framework also speaks to the literature that explores the impact of monetary policy on inflation expectations. Recent studies particularly focus on the effects of monetary policy communication (Enders et al., 2019; Lamla and Vinogradov, 2019; Binder et al., 2022; Brouwer and Haan, 2022; Dräger et al., 2024; Di Pace et al., 2025). While these studies have shown that the effects are sensitive to

individuals' attention to macroeconomic signals—which varies depending on the state of the macroeconomy (Korenok et al., 2023; Weber et al., 2025)—the effects of central bank communication are typically small and decay rapidly. This is the first paper that focuses on the response of professional economic experts. Even though these experts usually do not engage in forecasting inflation rates, they update significantly and immediately after the monetary policy announcement, even in the setting of natural updating. Recent research also emphasizes the importance of the language and embedded tone of central bank announcements (Shapiro and Wilson, 2022a; Binder et al., 2023; Gorodnichenko et al., 2023) and, more broadly, the implications of forward guidance (see e.g., Coibion et al., 2023b; Sutherland, 2023). We show that experts strongly react to these subjective factors and provide experimental evidence on their relative importance.

Finally, we contribute to the literature that examines how inflation expectations map into households' spending decisions. There is some correlational evidence that inflation expectations are positively associated with spending, as predicted by the Euler equation framework (see Dräger and Nghiem, 2021; Crump et al., 2022; D'Acunto et al., 2023). This is in contrast to work by Bachmann et al. (2015) and Burke and Ozdagli (2023) who find no conclusive evidence for a robust relation between spending and expected inflation. Coibion et al. (2022) and Coibion et al. (2023a) provide causal evidence, reporting that an exogenous fall in expected inflation causes households to decrease their spending on non-durable goods, but increase their spending on durable goods. Dräger et al. (2024) similarly find that an exogenous increase in inflation expectations lead to higher spending intentions. Our study offers complementary evidence going into a similar direction, revealing that households' spending intensions are causally decreased when inflation rates drop, particularly regarding spending plans on luxury items.

Organization. The remainder of this paper is organized as follows. Section (3.2) presents the conceptual framework of our study by providing an overview of our setting, data, and research design. Section (3.3) describes our empirical strategy and presents the results for our main expert experiment. It also explores the anatomy of the results and presents the results of our follow-up experiment to tease out the mechanisms underlying the treatment effects. Section (3.4) presents the main results for the information provision household survey experiment. Finally, Section (3.5) concludes.

3.2 Setting, Data, and Research Design

3.2.1 Setting

Our primary goal is to explore the extent to which economic experts can serve as intermediaries in explaining economic policies to households. We study this question in the context of the 2022-23 inflation surge, which offers a unique laboratory to examine the potential of experts to guide expectations and economic decisions of households. In this section, we describe the setting of our study, explain the policy shock that we exploit, and document that economic experts have offered vast supply of explanations during the inflation surge.

3.2.1.1 The 2022-23 global inflation surge

Several features make the 2022-23 cross-national surge in inflation an ideal setting to study the scope of experts to explain economic policies to households. First, the rising inflation rates created a high-stake environment, with many households facing real income losses. For instance, the Eurozone reached a record high of 10.6% in October 2022, with similar numbers in the United States and other advanced economies (see Figure 3.A.1 in the appendix). Second, the unprecedented inflation spike prompted widespread concern and media coverage, placing inflation high on the public agenda. Third, a variety of possible and competing explanations for the high inflation rates circulated in the media and public discourse, creating elevated uncertainty about economic policies in 2022.[1] Finally, central banks in many countries reacted with substantial increases in interest rates to the growing inflation concerns. The sharp interest rate hikes by central banks in response to high inflation have intensified the already existing challenges households face in understanding monetary policy (e.g., Lamla and Vinogradov, 2019), as such rapid increases represent an unprecedented experience for most agents.

3.2.1.2 Supply of expert narratives during the 2022-23 inflation surge

The potential of experts to act as intermediaries depends on the *supply* professional economists offer to explain economic phenomena to the public. During the 2022-23 inflation surge, experts have been extraordinarily present in the media and the public

[1] For instance, the Economic Policy Uncertainty Index (Baker et al., 2016) reached a level in 2022 that was historically only exceeded by the spike during the COVID-19 pandemic in 2020.

discourse to supply economic narratives and explanations about the causes and consequences of inflation. To quantify this role more precisely, we collaborate with the global media analyst MediaTenor to investigate the role of economic experts during the 2022-2023 inflation surge. Founded in 1993 in Zurich (Switzerland), the company specializes in monitoring and analyzing media content across various platforms, including print, television, and online outlets. In collaboration with MediaTenor, we gathered all media mentions of economic experts associated with the ten largest economic research institutes in Germany between 2019 and 2024, including television, print, and online media.[2] We also observe the dominant topic of all mentions.

Figure (3.1) shows the media mentions of economic experts in Germany (blue line, left axis), along with the share of mentions referring predominantly to inflation (red bars, right axis). The figure shows that expert assessments are mentioned an average of 583 times per month, equating to roughly 20 mentions of economic experts in the media each day. To put these numbers into perspective, we also collect all media mentions of CEOs working in companies listed in the DAX (Germany's leading stock market index) as a benchmark. In 2024, experts were mentioned 7,741 times, while all DAX CEOs combined received only about one-third of that number (2,793 mentions).

Over the full period 2019–2024, articles on monetary policy and inflation ranked fifth among the most important topics. However, there is a noticeable increase in media mentions related to inflation starting in late 2021, closely correlating with inflation trends.

3.2.1.3 The December 2022 cross-national monetary policy shock

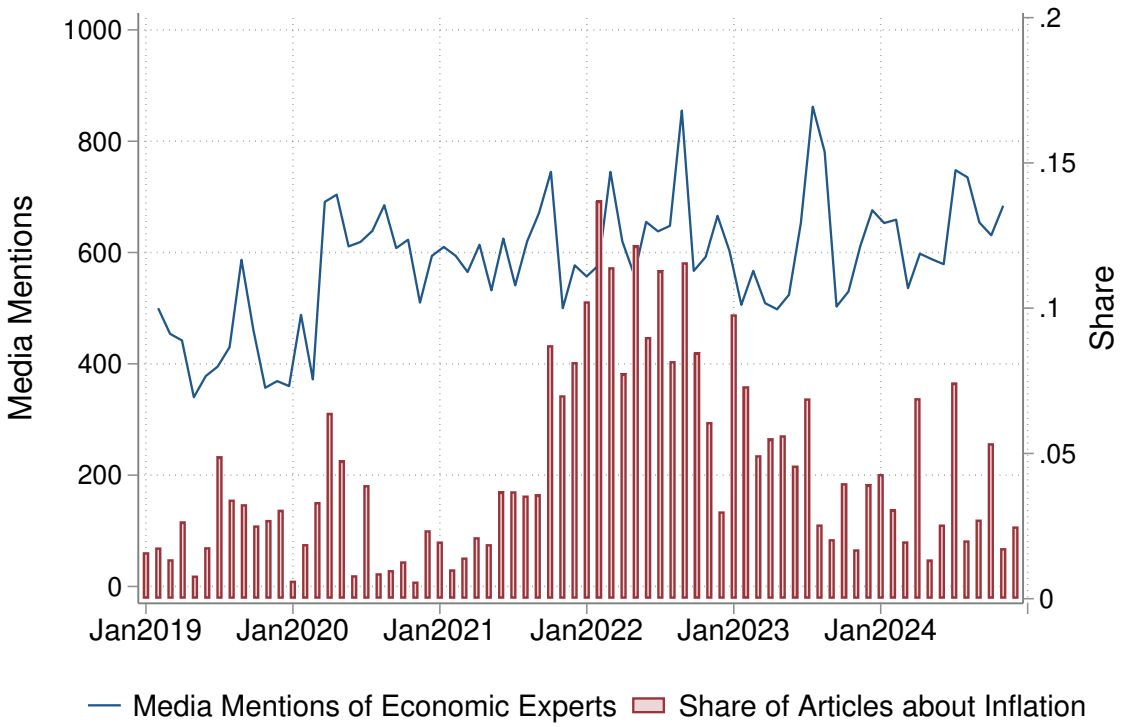
Our setting focuses on the monetary policy decisions by the world's largest central banks at the spike of the 2022-23 inflation surge. Amid an increase in price levels unparalleled in the younger history of Europe and Northern America, the ECB, the Bank of England, the Swiss National Bank, and the Norwegian Central Bank all scheduled their interest rate decisions for December 15, 2022.[3] Similarly, the Fed scheduled its interest rate decision for December 14, 2022.

The ECB, the Fed, the Bank of England, and the Swiss National Bank announced a 50 basis point increase in their main policy rates, while the Norwegian Central Bank raised its main policy rate by 25 basis points. To quantify the impact of this monetary

[2]MediaTenor codes all mentions that covered at least five lines or seconds in an article or post. The collection does not include social media posts.

[3]Inflation in the Euro Area and the United States reached a level as high as in 2022 (9.1% in June 2022) for the last time in 1981.

Figure 3.1: SUPPLY OF ECONOMIC EXPLANATIONS



Notes: The figure shows the number of media mentions of German economic experts employed at the ten largest research institutes in German media. Data comes from MediaTenor and includes experts from the following institutes: Deutsches Institut für Wirtschaftsforschung (DIW), Hamburger Welt-Wirtschafts-Institut (HWWI), IAB Institut für Arbeitsmarkt- und Berufsforschung, Institut der deutschen Wirtschaft (IW), Institut für Makroökonomie und Konjunkturforschung (IMK), Institut für Weltwirtschaft an der Universität Kiel (IfW), Institut für Wirtschaftsforschung (ifo Institut), Leibniz-Institut für Wirtschaftsforschung Halle (IWH), Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI), Zentrum für Europäische Wirtschaftsforschung (ZEW).

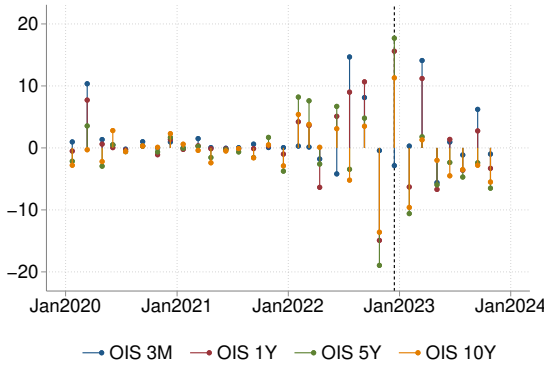
policy shock on financial markets, we use data from the Euro Area Monetary Policy Event Study Database (EA-MPD, see Altavilla et al. (2019)). The impact of monetary policy decisions is measured based on high-frequency data around a very narrow event window of ECB decisions (10 minutes pre- and post the policy announcement).

Figure (3.2) visualizes the impact of all monetary policy changes announced by the ECB between 2020 until 2024. Panel (a) shows changes in Overnight Index Swaps (OIS), Panel (b) depicts changes in German bond rates, and Panel (c) shows changes in the STOXX Europe 50 Index. The results consistently demonstrate that the monetary policy shock triggered by the ECB's announcement on December 15, 2022 (highlighted by the vertical dotted lines), had a significant and pronounced impact on market outcomes. The effects are particularly strong for longer-term OIS and bond rates, indicating that the announcement was primarily a forward guidance shock rather than a conventional monetary policy shock.

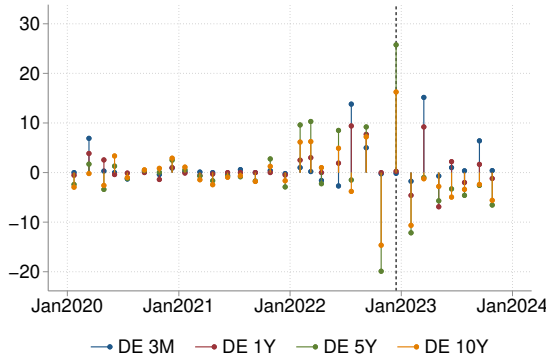
Why the announcement on December 15, 2022 was exceptional. The ECB first publishes a press release with the information on the policy decision without a discussion or a rationale for the decision. Around 45 minutes later, the ECB holds a press conference explaining the interest rate decision. The separate releases of the pure policy decision and the narrative information described in the press conference allows to separately measure the impact of the *objective* and *subjective* types of information conveyed in the monetary policy announcement. Panel (d) of Figure (3.2) separately shows the effects of the press release and the press conference on the financial market indicators of Panels (a)–(c). While the press release had sizable effects on all indicators, the impact of the press conference was even (much) larger.

Figure 3.2: IMPACTS OF THE MONETARY POLICY SHOCK ON DECEMBER 15, 2022

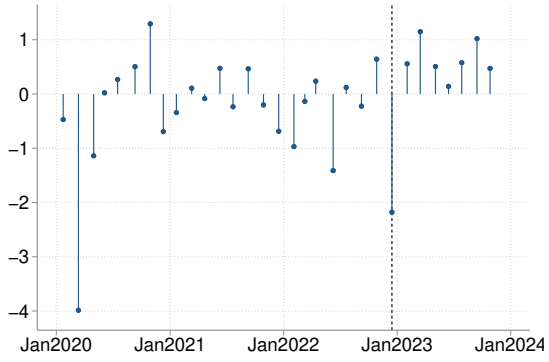
(a) Changes in Overnight Index Swaps rates



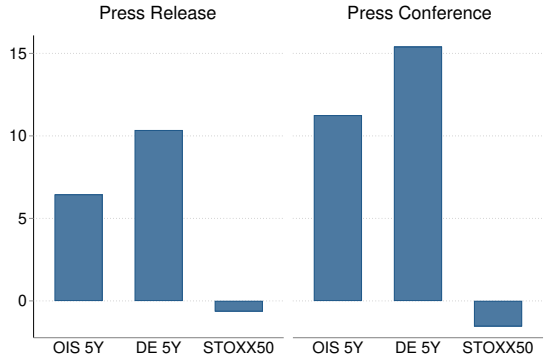
(b) Changes in German bond rates



(c) Changes in STOXX50 index



(d) Press release and Conference window



Notes: Panel (a) shows changes in Overnight Index Swaps (OIS) rates around monetary policy events. Panel (b) shows changes in German bond rates around monetary policy events. Panel (c) shows changes in the STOXX-50 stock index around monetary policy events. Panel (d) shows changes in the three indicators separated by Press release window and Press Conference window. Data comes from the Euro Area Monetary Policy event study Database (Altavilla et al., 2019).

The reason for the exceptional impact of the press conference on financial markets is that the December 2022 monetary policy shock conveyed important subjective elements on top of the objective information about the main interest rate change. ECB President Christine Lagarde's statements during the press conference conveyed an unexpectedly hawkish tone and a path surprise component in forward guidance that signaled more potential interest hikes in the future than previously anticipated. On December 15, 2022, the Financial Times wrote that *"Investors were rattled by the hawkish tone of the meetings, in particular by comments from the ECB that* 'inflation remains far too high' *and that rates would continue to rise by 0.5 percentage points* 'for a period of time'*"*. The Financial Times further wrote a day later: *"Her* [Christine Lagarde] *near promise of further half-percentage point rate rises coming in February and March surprised economists, many of whom had expected the central bank to quickly end its cycle of rate rises in the next few months"* (Financial Times, December 16, 2022).

3.2.2 Data and Samples

Our research project is designed to study whether economic experts can serve as intermediaries to explain the December 2022 monetary policy shock to households. To this end, we implement three large-scale cross-national expert surveys and a household survey that is broadly representative of the German population to implement our setting. Below, we describe how we recruit these samples.

3.2.2.1 Expert surveys

All expert surveys are collected implementing special modules of the Economic Expert Survey ('EES'), which is the largest regularly conducted survey of professional economists globally (see Gründler et al. (2023) for details). The survey, which builds on and largely extends the World Economic Survey (WES) initiated in 1981, is collected by the ifo Institute in Munich (Germany), the CESifo research network, and the IWP Lucerne (Switzerland). It asks renowned international economic experts about their evaluation of economic policies in their country of expertise and publishes these results in a quarterly report ('Evaluating Global Economic Policy').

The professional economists covered in the EES work at universities, research institutions, embassies, central banks, multinational firms, and international think tanks. These experts, which include Nobel Prize winners in economics and members of national councils of economic advisors, have a high academic impact, shape national debates, and are influential in designing economic policies. As can be seen in Table

(3.B.1) in the appendix, the participating academic experts have an average of 6,690 Google Scholar citations (median: 2,557) and an average of 1,846 Twitter followers (which in 2022 was the most popular social media platform among economists).

To bring our conceptual framework to the data, we record responses from experts in Northern America, EMU member countries, and three European countries with interest rate increases at the same day (United Kingdom, Norway, and Switzerland).[4] Overall, 442 experts participated in our main experiment, with 69% of the participants working in EMU countries, 18% are recruited from Northern America, and 13% work in Norway, UK, or Switzerland. The majority of experts work in universities (66%). Other participants are employed at research institutes (12%), central banks (5%), the private sector (8%), and the public sector (5%). Furthermore, about 90% of the experts are male, with an average age of 57 years (standard deviation 11.84 years).[5] A total of 177 experts (40% of the sample) have a primary focus on topics with JEL code *'E: Macroeconomics and Monetary Economics'*.

3.2.2.2 Household survey

We collect our household survey between September 18 and September 22, 2023 in cooperation with the survey company Respondi, which is commonly used in academic research. As shown in Table (3.B.2) in the appendix, our sample includes 1,260 respondents and is representative of the German population in terms of gender, age, education, region, and total household income. For example, 51% of the sample are males (50% in the German population), 15% of the sample live in East Germany (15% in the German population), and we achieve similar matches for age brackets, income brackets, education brackets and participants' employment status.

3.2.3 Conceptual Framework and Survey Structure

In what follows, we describe our conceptual framework and the main elements of our expert and household surveys. Details on the survey structure are described in Appendix 3.C, the full survey instructions and questions are included in Appendix 3.E.

[4]By the time of our survey experiment, a total of 19 countries were members of the EMU. These countries were Austria, Belgium, Cyprus, Estonia, Finland, France, Germany, Greece, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Portugal, Slovakia, Slovenia, and Spain.

[5]The gender composition is similar to other studies involving expert responses; see, e.g., Andre et al. (2023).

3.2.3.1 Overview

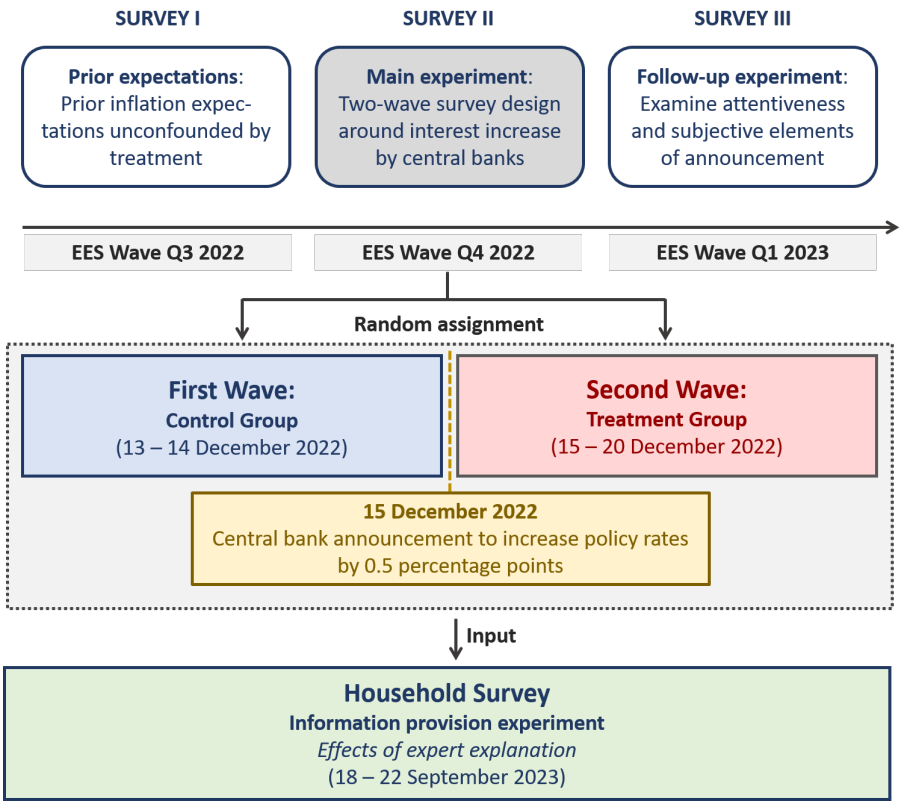
Our setting rests on two fundamental building blocks, designed to explore the scope of experts to serve as intermediaries: (1) How do experts update in response to new monetary policy signals? Is this process different from updating of households and firms? (2) Do expert narratives impact households' expectations and beliefs? Are narratives provided by experts consequential for households' spending decisions? We explore the first block of questions through three cross-national expert surveys and experiments (Part I), while the second block is examined using household survey experiments (Part II). Figure (3.3) provides an overview of our experimental design.

3.2.3.2 Updating of experts (Part I)

We start by exploring updating of economic experts in response to the December 2022 monetary policy shock (see Appendices 3.E.I and 3.E.II for instructions). When surveying experts with economic and methodological training, eliciting priors and posteriors within the same survey risks revealing the experimental nature of the study. The first survey, EES wave Q3 2022, therefore elicits prior inflation expectations of participants. The second survey, EES wave Q4 2022, includes our main experiment to study updating of experts after the monetary policy shock. The experiment is designed to study natural updating, meaning that all experts in our experiment are blind to the goal of our study, with no reference to monetary policy. Our experiment is structured in two waves. We randomly assign the universe of experts in our sample—which include experts from all EMU countries, Switzerland, Norway, the United Kingdom, and Northern America—to a control group (surveyed in the first wave closely before the monetary policy shock) and a treatment group (surveyed in the second wave closely after the shock). In both waves, participants are asked about their inflation expectations in the short-, medium-, and long-run. These questions are identical to the ones regularly asked in previous waves, providing no indication about our setting. As in previous waves, we also include open-ended text questions about the drivers of inflation that allow us to study the anatomy of potential treatment effects. Furthermore, we include questions about the general macroeconomic environment.

After the main experiment, we design a follow-up survey (EES wave Q1 2023) in which we examine the impact of the subtle subjective elements conveyed by the December 2022 monetary policy announcement, i.e. the hawkish tone and the path surprise components. Given that only *attentive* respondents should react to the monetary policy shock in our natural updating setting, we also use the follow-up experiment to explore

Figure 3.3: EXPERIMENTAL SETUP



Notes: The figure shows the experimental setup of our analysis. Our expert analysis consist of three cross national surveys, conducted over three quarters of the EES survey, between EES wave Q3 in 2022 and EES wave Q1 in 2023. The first survey (EES wave Q3 2022) elicits prior inflation expectations of participants before the main experiment. The second survey (EES wave Q4 2022) includes our main experiment. We randomly assign the universe of international experts regularly participating in the Economic Experts Survey (EES) to a control and a treatment group. The third survey (EES wave Q1 2023) is a follow-up in which we examine the relative importance of mechanisms, exploring the effects of the announcement tone versus path surprise components. We then use the information obtained in our expert experiment as inputs for our household-level experiment (information provision), in which we explore whether expert information can shape expectations, beliefs, and spending intensions.

whether inattentive experts update similarly to attentive experts when being provided with information about the monetary policy shocks. This exercise allows us to assess whether variations in natural updating among experts arise from information frictions (i.e. attentiveness to new monetary policy signals) or more fundamental differences in subjective macroeconomic models.

Comparison to households and firms. We implement a similar natural updating design using household-level data from the New York Fed's Survey of Consumer Expectations (SCE) and firm-level data from the German Bundesbank (BOP-F). These analyses allow us to compare our results for experts to natural updating behavior of other agents.

3.2.3.3 Reaction of households to expert narratives (Part II)

In the second step, we explore how households update their expectations when receiving different explanations from economic experts regarding the consequences and mechanisms of the monetary policy shock (see Appendix 3.E.III for instructions). We use the results of our expert survey as inputs in an information provision experiment based on a representative sample of German households. Our treatment design also allows us to compare households' updating behavior when receiving explanations from economic experts compared to receiving information without embedded encoding. We further explore the consequentiality of expert narratives, estimating the causal effect of explanations by experts on households' spending intensions.

3.3 Experimental Results for Experts

3.3.1 Experimental Strategy

Our main experiment for economic experts was pre-registered on December 10, 2022. The pre-analysis plan outlines the design of our trial and the questions included in our survey. In an extension of this plan, we complement our main experimental setup with a follow-up to investigate the effects of the subjective elements of the monetary policy announcement (see Section 3.2.3), which were unanticipated ex ante.

3.3.1.1 Econometric model

We study the response of experts to monetary policy signals in a framework of natural updating (see Boumans et al. (2024) for a similar setting). The central idea of our experiment is to compare inflation expectations of participants randomly allocated to the first wave (surveyed just before the policy shock) with those of respondents randomly assigned to the second wave (surveyed immediately after the shock), without referencing the monetary policy itself. This setting offers the advantage of capturing experts' natural responses to a monetary policy shock without influencing their expectations through direct information provision.

Our baseline model compares inflation expectations, π_i, of experts surveyed in the first wave ($T_e = 0$) with those of experts surveyed in the second wave ($T_e = 1$). We elicit inflation expectations of experts e for their country of expertise i, covering three time-horizons (short-, medium-, and long-run inflation expectations), denoted by τ. In

our setting of *natural* updating, participants' reactions depend on whether they have heard about the policy shock. To address this, our baseline model estimates how the effect of the monetary policy shock varies with attentiveness, Attentive_e, via

$$\pi_{ei}^{\tau} = \gamma^{\tau} T_e + \zeta \text{Attentive}_e + \lambda(T_e \times \text{Attentive}_e) + \eta_i + \mathbf{X}_e \boldsymbol{\mu} + \varepsilon_{ei}^{\tau}. \tag{3.1}$$

To eliminate systematic cross-national differences in macroeconomic fundamentals at the time of our experiment, we condition our estimates on fixed effects for experts' countries of expertise, η. In variants of our model, we also control for observed biographic and socio-demographic characteristics of the experts in our sample, as well as the time they took to complete the questionnaire. All models are estimated using OLS. Results are obtained by adjusting standard errors to arbitrary heteroskedasticity.

3.3.1.2 Key identifying assumption and balance tests

The key identifying assumption underlying our experiment requires that absent of the treatment, respondents in the control and the treatment groups are statistically identical, i.e.

$$\mathrm{E}[\varepsilon_{ei}^{\tau} | T_e = 1] = \mathrm{E}[\varepsilon_{ei}^{\tau} | T_e = 0] = 0. \tag{3.2}$$

Under this assumption, the parameter $\hat{\gamma}$ identifies the effect of the interest rate announcement on inflation expectations for inattentive experts, and the treatment effect of attentive respondents is identified by $\hat{\gamma} + \hat{\lambda}$. The identifying assumption should be fulfilled by construction in our two-wave design. To test for the integrity of randomization and potential selection effects, we compare sample means of observable biographic and socio-demographic characteristics of experts. Table (3.B.3) in the appendix shows that our sample is well balanced for all observable characteristics. We also do not find any difference in attrition between the two waves of our main experiment compared to previous rounds of the EES.

We also perform a joint F-test, in which we test for the joint significance of all observable characteristics. Regressing treatment status on the full set of characteristics, we obtain a F-statistic of 0.54 (with a corresponding p-value of 0.85), which shows that observable characteristics are strongly balanced between treatment and control group. To nevertheless rule out any concern about imbalance, we report variants of our baseline model in which we account for individual-level characteristics.

3.3.1.3 Attentiveness of experts

Our natural updating setting prevents us from directly asking respondents about whether they heard news about monetary policy changes, as such questions would prime participants and prompt them to consider monetary policy issues even if they would not have naturally thought about them. To classify experts as attentive, we instead leverage their professional background as an indicator of their likelihood to consume macroeconomic news and stay informed about monetary policy. We classify respondents as macroeconomists if they have a primary focus on topics with JEL code *'E: Macroeconomics and Monetary Economics'*.

We take two steps to validate this strategy. First, we ask respondents about their perceived primary drivers of inflation in an open-ended text question and apply a tailored manual coding scheme to identify reference to monetary policy topics. Comparing reported drivers of inflation between macroeconomists and non-macroeconomists, we find striking differences in how often they reference monetary policy authorities (see Figure 3.A.2 in the appendix). For a second validation, we follow the approach of Binder (2017), quantifying uncertainty revealed in responses to survey I on prior (pre-experimental) expectations about inflation. The method exploits the fact that round numbers in survey responses on inflation expectations convey uncertainty of participants. Consistent with our results for free-text answers, we find that reporting integers is much more widespread across non-macroeconomists.

3.3.1.4 Discussion of the experimental design

Given the sharp increase in inflation rates in Europe and Northern America, there was great interest in how central banks would adjust their refinancing rates. While the objective information of the announcements (the decision to increase the refinancing rate by 50 basis points) may have been anticipated by some experts, an analysis of Google searches reveals that the salience of this event was mostly restricted to the day of the announcement (see Figure 3.A.3 in the appendix). The subjective information, i.e. the hawkish tone of the December 15 announcement and the path surprise component it entailed, caught agents by surprise.

Anticipation effects in the control group of our main survey experiment would downward bias the effect size estimated in our experiment. If experts in the control group had anticipated the extent of the policy shock, they would have already reported lower expected inflation rates than the treatment group, reducing the mean difference between the treatment and the control group. Imprecision of our measure of attentive-

ness has a similar effect, biasing the estimated coefficients towards zero (attenuation bias). When some attentive individuals are misclassified as inattentive, the estimated treatment effect on attentive experts will be downward biased. Our results, therefore, reflect lower bounds of the 'true' updating effect.

3.3.2 Baseline Results

The estimates of our natural updating experiment reveal that the monetary policy shock substantially decreased inflation expectations of attentive experts (see Table 3.1). The treatment reduced inflation expectations by an average of 0.62 percentage points ($p < 0.01$) and is similarly effective when we condition on the time respondents spend to answer our survey, biographic and socio-demographic characteristics of experts, and fixed effects for affiliations (see the row 'cumulative effect'). Using Wald tests, we find that the parameter estimates of these augmented specifications, shown in Columns (II)–(IV), are statistically indistinguishable from the benchmark estimate reported in Column (I).

To assess whether the treatment effects are driven by either the European or the Northern American sample, we estimate equation (3.1) separately for the two samples. We find that treatment effects are close to identical in both sub-samples and remain highly statistically significant (see Table 3.B.4). The similarity of treatment effects across continents rules out that the results are driven by specific confounding events in Europe or the United States.

3.3.3 Extensions and Complementary Findings

We conduct a series of additional analyses to substantiate our main experimental results. This section summarizes some of the most important extensions, with further details on the analyses documented in Appendix 3.D.

Confounding events and within day results. We conduct two exercises to explore a potential impact of unobserved confounding events in our context of natural updating. The results remain almost identical when we gradually restrict the post-treatment period (see Figure 3.A.4) or when we leverage within treatment-day variation, estimating treatment effects only based on respondents that participated before and after the announcement at precisely the day of the monetary policy announcement (Figure 3.A.5). Consistent with attention being larger in a natural updating setting when the

Table 3.1: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—BASELINE-RESULTS

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.173	0.171	0.202	0.247
	(0.241)	(0.241)	(0.240)	(0.238)
Macro	0.001	0.002	0.001	0.068
	(0.235)	(0.236)	(0.232)	(0.235)
Treatment × Macro	-0.794**	-0.788**	-0.770**	-0.810**
	(0.331)	(0.332)	(0.327)	(0.327)
Cumulative effect	-0.620***	-0.617***	-0.568**	-0.563**
	(0.228)	(0.228)	(0.225)	(0.226)
Observations (# experts)	442	442	442	442
Countries	24	24	24	24
R-Squared	0.507	0.507	0.517	0.527
Country Fixed Effects	Yes	Yes	Yes	Yes
Survey Time	No	Yes	Yes	Yes
Biographic Characteristics	No	No	Yes	Yes
Affiliation Fixed Effects	No	No	No	Yes
Equal. (p-val)	–	0.99	0.82	0.80

Notes: The table shows the baseline results of our main expert experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. 'Equal. (p-val)' reports p-values on a Wald test that compares the estimated parameters of Columns (II)-(IV) with the treatment effect identified in the parsimonious model shown in Column (I). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

timing between survey response and the treatment event is smaller, we find a 1.2 percentage point reduction in mean expectations in our least conservative specification. The results can also not be explained by updates in officially reported inflation forecasts. In fact, the ECB's inflation projections for 2023 have been revised *upwards* in early December 2022.[6] This revision is consistent with the projections included in the survey of professional forecasters published prior to our experiment. Taken together, there is very limited scope for confounding events biasing our results.

[6]Eurosystem staff projections foresaw inflation at 8.4% in 2022, 6.3% in 2023, and 3.4% in 2024 (June projections: 8.4% in 2022, 5.5% in 2023, and 2.3% in 2024).

Prior expectations and updating. We next use prior expectations of individuals to more closely examine *updating* of expectations in response to monetary policy signals. To quantify updating of inflation expectations, we relate the difference between posterior and prior (pre-experimental) inflation expectations, $\text{update}_{ei} = \pi_{ei}^{\tau,\text{posterior}} - \pi_{ei}^{\tau,\text{prior}}$, to the treatment status via

$$\text{update}_{ei} = \gamma^{\tau} T_e + \zeta \text{Attentive}_e + \lambda(T_e \times \text{Attentive}_e) + \alpha \pi_{ei}^{\tau,\text{prior}} + \eta_i + \mathbf{X}_e \boldsymbol{\mu} + \varepsilon_{ei}^{\tau}. \quad (3.3)$$

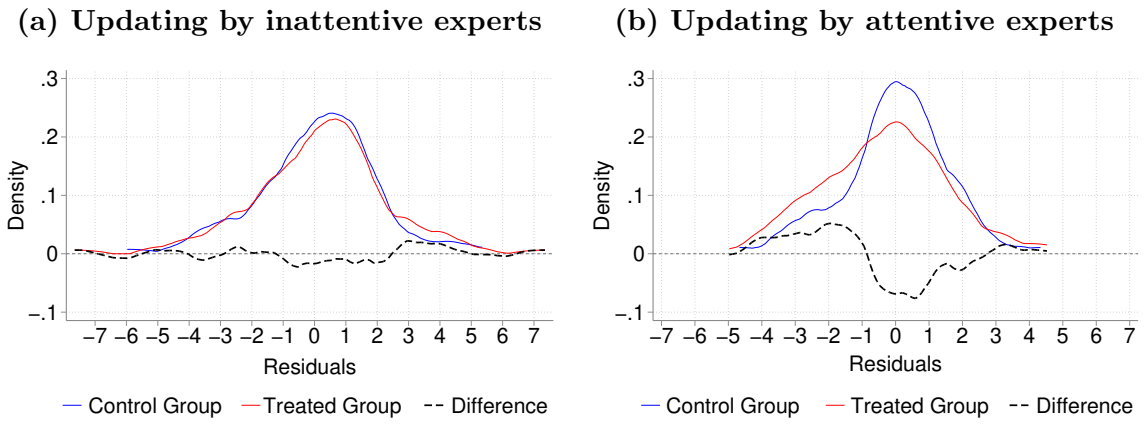
Individuals with higher prior inflation expectations should mechanically display larger changes in expectations, given that the scope for updating is smaller for lower prior expectations. To account for level differentials in individuals' priors, we control linearly for experts' pre-experimental expectations $\pi_{ei}^{\tau,\text{prior}}$ (see e.g., Roth and Wohlfart, 2020). Accounting for pre-experimental expectations about inflation also allows us to address differentials in expectations that may correlate with individual-level unobservables. Some of the differences in inflation expectations across participants may be systematic, e.g. when initiated by past inflation experiences (see Malmendier et al., 2021). The rest of our empirical model for updating replicates the specifications of our baseline experimental models.

Figure (3.A.6) in the appendix plots posterior inflation expectations against prior expectations. Inflation expectations in our main experiment are strongly correlated with expectations elicited in the pre-experiment survey, pointing to a large degree of internal and cross-survey validity. It also underlines consistency in inflation expectations of participants. Importantly, we also observe significant updating in expectations, visualized by deviations from the 45 degree line. Figure (3.4) shows the distribution of priors and posteriors and the updating effect, revealing a substantial shift of the distribution for attentive experts to the left. Consistent with these shifts, Table (3.B.5) in the appendix, presenting our main experimental results for updating, documents treatment effects that are almost identical to our baseline estimates.[7]

Treatment heterogeneity. Our baseline model interacts treatment status with experts' professional background to proxy attentiveness. Appendix Table (3.B.6) reveals that there is no treatment heterogeneity regarding any other expert characteristic, including prior inflation expectations, gender, age, affiliation, the number of Google scholar citations, or the time invested to answer our survey. To further inspect whether macroeconomists have a different response behavior or differ in other perceptions be-

[7] In Table (3.B.7) in the appendix, we provide complementary results showing that this holds true even when we estimate the updating specification *within* treatment day.

Figure 3.4: DISTRIBUTION OF UPDATING ACROSS TREATMENT ARMS



Notes: The figure shows the distribution of respondents' update in inflation expectations (posterior - prior) by treatment status. Panel A shows the distribution of updating for non-macro (non-attentive) experts. Panel B shows the distribution of updating for attentive experts. The dashed black line shows the difference in the distributions (treatment - control). To account for systematic differentials in macroeconomic fundamentals across countries included in our sample, we condition priors and posteriors on country-level fixed effects.

yond macroeconomic expectations, we take experts' answers to the four EES-core questions related to evaluations of national economic policies as outcomes, finding no significant treatment effects (Table 3.B.8). These results limit the possibility that the main experimental results are driven by any expert-level characteristic other than attentiveness that systematically differs between macroeconomists and non-macroeconomists.

Expectations about the general macroeconomy. Our main survey also elicits expectations about the general macroeconomy, which allows us to account for real-economic components of participants' inflation expectations. These broader macroeconomic expectations allow us to run complementary analyses that deliver two additional pieces of evidence. First, we find that the monetary policy shock decreased economic growth expectations (see Table 3.B.9). This finding is consistent with text-book economic models of a negative demand shock caused by restrictive monetary policy. Second, the treatment effects of our baseline experiment persist when we condition on experts' broader macroeconomic expectations (see Table 3.B.10). These results also show that higher growth expectations are significantly associated with higher inflation expectations, underlining that real-economic components matter for the formation of expectations about inflation.

Forecast errors. Exploring the consequentiality of being attentive to monetary policy signals, we assess whether experts in the treatment group exhibit lower forecasting errors.[8] We find that attentive experts in the treatment group exhibit 7.5 percentage points lower forecast errors ($t = 2.04$). When looking at *changes* in the forecast errors (vis-à-vis forecast errors of prior expectations), forecast errors are even smaller (10.8 percentage points lower forecast errors; $t = 1.92$).

Selection into survey and falsification. Given that our experiment is based on the same questions as in prior waves of the EES and that there are no references to monetary policy in the invitation or at later stages in the survey, the scope for selection into survey should be limited. Similarly, we find no differential attrition regarding answers to the inflation question across the two survey waves, and no differences in experts' survey-tenure across the control and treatment group. Re-estimating our baseline model using prior inflation expectations recorded in our first survey as a placebo outcome and assigning experts into the treatment and the control group of our main experiment does not reveal any statistically significant difference in mean expectations (Table 3.B.11). These results leave little scope for biases initiated by sorting.

Further robustness analyses. We run a series of additional tests to assess the robustness of our main experimental results for experts. Our baseline results include the full sample of experts from countries with a monetary policy change on December 14 and 15, 2022. The results are robust to excluding participants from the UK, Norway, and Switzerland, and to excluding all countries outside the EMU (Table 3.B.12). The results are also stable when we impose requirements on the minimum number of participants per country (Table 3.B.13). The Fed's December 14 announcement, a day before the ECB's, could have influenced inflation expectations in the EU control group. If anything, spillovers would have lowered expectations of participants in the EU control group, making our treatment effects a lower bound. However, a direct test comparing mean expectations in the EU control group before and after the Fed announcement finds no significant difference ($t = 0.09$), suggesting spillovers were negligible. More generally, we find that the results are not driven by particular countries, which is shown by jack-knife regressions, where we exclude one country at a time ('leave-one-out', see Figure 3.A.7). We also find no differences in treatment effects when we truncate inflation expectations at 15% to eliminate potential biases from outliers (Table 3.B.14), use Huber robust regressions (Table 3.B.15), or cluster standard errors at the level of countries' central banks (Table 3.B.16). Previous research has demonstrated that

[8] Forecast errors are calculated as in Equation (3.7).

repeated surveys might induce learning-through-survey effects, which could influence stated macroeconomic expectations (Kim and Binder, 2023). While experts are likely to be less prone to those effects than households and balance tests reveal no difference in survey-tenure across the control and treatment group, we directly rule out that such effects drive our treatment effects by controlling for experts' survey-tenure. Treatment effects—if anything—slightly increase (Table 3.B.17). Another concern might be that conducting within-country comparisons is too restrictive. We document that alternative strategies to using country fixed effects, including modeling region fixed effects or controlling for officially-reported pre-experimental inflation rates, deliver similar results (Table 3.B.18). Finally, we find that the central bank announcements only affected short- and medium-term inflation expectations, but had no impact on the formation of expectations for longer periods (Table 3.B.19). This finding is plausible, given that the primary purpose of the policy change was to bring down inflation in the short run.

Summary of main expert experiment. Taken together, our first main result can be summarized as follows:

Result #1 *Attentive economic experts significantly update short-run inflation expectations in response to a change in monetary policy.*

3.3.4 Anatomy of the Experimental Results

3.3.4.1 Perceived causes of inflation and treatment status

The key hypothesis that we aim to test by designing our experiment is that experts surveyed in the treatment group update inflation expectations precisely because of the monetary policy change announced by central banks. We next use answers to open-ended questions on the perceived *causes* of inflation to examine the anatomy of our experimental results and to explore whether updating of inflation expectations can be traced back to monetary policy considerations.[9] Respondents are asked to answer this question by writing brief texts in free-text entry boxes. The main advantage compared to close-ended questions is that answers to open-ended questions allow us to elicit the causes of inflation without any priming of participants (see e.g. Stantcheva (2021), Ferrario and Stantcheva (2022), and Dräger et al. (2025) for related approaches). Pre-

[9]The question asks respondents: *'In your opinion, what are the current reasons for inflation in* `[country]` *'*?

venting priming is particularly important in our case, as our survey did not include any information related to the monetary policy announcement of central banks.

We visualize the words mentioned by participants in free-text answers in Figure (3.A.8) in the appendix. The word cloud presents the words respondents use in their written texts, with the relative size of the words reflecting the frequency of usage. After clearing the text from stop words, we identify a total of 1,067 distinct words.

To more rigorously investigate whether answers on the causes of inflation differ across treatment status, we manually code responses and classify answers across a series of inflation drivers, including (a) monetary policy, (b) supply-side effects, (c) demand-side effects, (d) energy prices, (e) market-related factors, and (f) fiscal policy. Our main category on monetary policy captures references to monetary policy, interest rate changes, and mentions of specific central banks.[10]

Relating causes of inflation to the treatment status in Figure (3.5), we find that participants who were surveyed after the central bank announcements are significantly more likely to refer to monetary policy issues when describing the causes of inflation ($t = 1.97$). For all other categories, we do not detect any differences between participants surveyed in the control and treatment group.

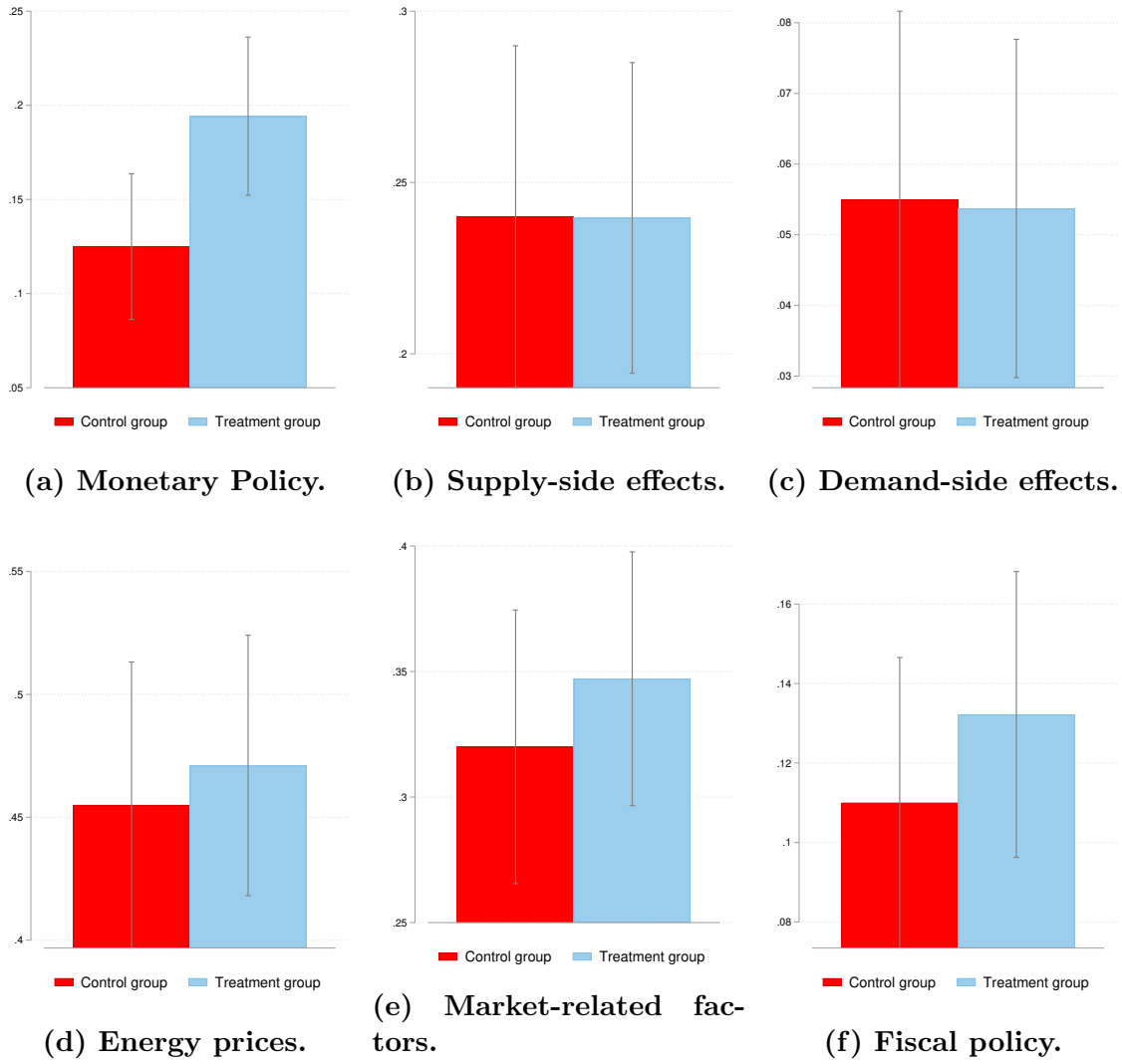
3.3.4.2 The tone of survey responses

Exploring differences in the perceived causes of inflation depending on treatment status is informative, but it does not reveal the consent or dissent to current policies. To examine whether the policy announcement also impacted the way individuals' think about monetary policy, we use a more subtle measure, relating to the *tone* embedded in the free-text answers.

To classify the tone of experts' answers, we use the VADER (Valence Aware Dictionary for sEntiment Reasoning) model, a natural language processing algorithm that is trained to extract the polarity (positive or negative) as well as the intensity (strength) of emotions hidden in a written text. The algorithm, initially developed by Hutto and Gilbert (2014), is particularly designed to classify emotions in brief microblogs and social media messages, which most closely resembles the format of the answers provided

[10]Supply-side effects include supply chains and general shortages of supply. Demand-side effects cover factors that impact aggregate demand. Energy prices include answers referring to increasing prices for energy and food. Market-related factors consider wage-price spirals, labor market characteristics, and frictions. Finally, fiscal policy includes expansionary spending and debt, in many cases related to stimulus packages implemented during the Covid-19 pandemic. Some experts also refer to the Covid-19 pandemic, Brexit, lockdowns in China and the war in Ukraine. Yet, the fraction of answers referring to these factors as a single cause of inflation is small.

Figure 3.5: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS—TREATMENT EFFECTS ON THE CAUSES OF INFLATION.



(a) Monetary Policy. **(b) Supply-side effects.** **(c) Demand-side effects.**

(d) Energy prices. **(e) Market-related factors.** **(f) Fiscal policy.**

Notes: The figure shows answers to the open-ended question in our main survey, asking participants about their assessment of the causes of inflation in their countries of expertise. Answers are manually coded via dummy variables that capture whether respondents have included specific topics in their response. The figure shows how the treatment status (being surveyed in the second wave of our experiment) influences answers given regarding the causes of inflation.

by the experts in our experiment. The algorithm has been shown to provide classifications that are indistinguishable to the classification of emotions humans would assign to a text (see Hutto and Gilbert (2014) for a comparison of the algorithm to human classifications). The algorithm allows us to construct metrics that reflect the positive and negative components in the text, as well as a summary score that reflects the overall sentiment. This 'compound sentiment score' of the VADER algorithm classifies texts on a scale running from -1 (most negative) to $+1$ (most positive).

Our approach to measure sentiments in text is motivated by the computer science literature on 'author profiling', which has shown that it is extremely difficult for writers to mask their emotions and attitudes in written text (e.g., Cheng et al., 2011; Rangel and Rosso, 2016). For cross validation of our approach, we relate the compound sentiment score to the expected inflation rate for 2023. We find that respondents with positive sentiment report significantly lower inflation rates than experts with negative sentiment ($t = 2.02$).

The main result regarding the tone of answers, shown in Appendix Figure (3.A.9), is that experts surveyed in the second wave of our main experiment (after the policy announcement) are significantly more positive than experts polled in the first wave (before the policy announcement). We find sizable and statistically significant differences in tones regarding the fraction of components with positive sentiment, negative sentiment, and also regarding the overall sentiment score. These results are consistent with higher confidence in monetary policy to bring down inflation.

Summary of the anatomy of treatment effects. We summarize our findings regarding the anatomy of the experimental results as follows:

Result #2 *The interest rate announcement makes monetary policy as a determinant of inflation expectations salient. Experts who are surveyed after the policy change are more positive towards inflation.*

3.3.5 Benchmarking against Households and Firms

We next explore whether the response of experts to the monetary policy shock differs from that of households and firms.

Benchmarking against updating of households and firms. To benchmark the results for experts against the updating behavior of households and firms, we apply the

same natural updating framework from our main experiment to household- and firm-level data. We use firm-level data collected by the German Bundesbank (Bundesbank BOP-F) and household-level data collected by the Federal Reserve Bank of New York (Survey of Consumer Expectations, SCE).

Table (3.2) shows the results. The main finding is that both households and firms *do* identify the interest rate changes, but both agents cannot link the increase in interest rates to inflation rates.

Households are asked to estimate the likelihood that the interest rate on their savings account will be higher in one year than it is today, using a scale from 0 to 100 percent. Those surveyed immediately after the central bank announcement perceive this likelihood to be about 8 percentage points higher than those surveyed just before the announcement (Columns I and II, Panel A). Similarly, firms in the Bundesbank Panel are asked about their expectations for the central bank's key interest rate 12 months ahead. Firms appear attentive to the rate change: those surveyed after the announcement expect the future interest rate to be 0.4 percentage points higher than firms surveyed before the announcement (Columns I and II, Panel B).

Although households and firms recognize the interest rate change, they fail to connect it to inflation. Mean inflation expectations of the treated firms and households do not differ from those included in the control group—independent of the included controls, estimation technique, or sample window (Columns III to VIII of Panel A and B).

Discussion. The benchmarking exercise highlights fundamental differences between experts' responses to the monetary policy shock and those of households and firms. Most notably, experts' responses were objectively more accurate. First, treated experts had much lower forecasting errors than untreated experts (see Section 3.3.3). Second, professional forecasters revised their inflation projections substantially *downwards* in Q1 2023 for 2023, but did not change their predictions for 2024. The adjustment of professional forecasters took place after our experiment and is numerically very close to the treatment effects identified in our main experiment. That households and firms adjusted their interest rate expectations but failed to link them to inflation underscores the significant potential of economic experts to serve as intermediaries, helping firms and households better understand economic policy and form more accurate expectations and beliefs.

Table 3.2: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF FIRMS AND HOUSEHOLDS—BENCHMARKING

	Interest rate exp.		Inflation exp. (12 months)					Forecast err.
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
	Panel A: US-Households based on SCE data							
Treatment	7.841***	8.595***	-0.080	0.014	0.036	-0.028	-0.033	-10.774
	(2.548)	(2.439)	(0.504)	(0.491)	(0.408)	(0.582)	(0.799)	(8.315)
Observations	472	468	468	464	464	305	183	464
R-Squared	0.020	0.128	0.000	0.025	0.032	0.022	0.054	0.088
	Panel B: German Firms based on Bundesbank data (BOP-F)							
Treatment	0.399***	0.423***	-0.172	-0.197	-0.156	-0.296	-0.250	-1.755
	(0.057)	(0.061)	(0.167)	(0.171)	(0.163)	(0.217)	(0.232)	(2.295)
Observations	981	932	980	931	931	658	464	931
R-Squared	0.048	0.095	0.001	0.068	0.065	0.078	0.124	0.092
Controls	–	Yes	–	Yes	Yes	Yes	Yes	Yes
Huber rob.	–	–	–	–	Yes	–	–	–
Same sample	–	–	–	–	–	Yes	–	–
2-Day window	–	–	–	–	–	–	Yes	–

Notes: The household results in Panel A are based on SCE data by the NY-Fed. Observations are included in a symmetrical five day window around the Fed announcement (from December 9 to December 19, 2022). Controls include fixed effects for brackets of households' education levels, their income, age, numerical skills, gender, employment status and region. The firm results in Panel B are based on Bundesbank BOP-F data. Observations are included in a symmetrical five day window around the ECB announcement (from December 10 to December 20, 2022). Controls include fixed effects for brackets of firms' number of employees, their annual turnover, their sector, and region. Column (VI) restricts the included days to be identical to our expert setting. In Column (VII), we include only a two-day window around the monetary policy event. Forecast errors in Column (VIII) are calculated as in Equation (3.7).

3.3.6 Follow-up Experiment

Our main experimental results show that the December 2022 monetary policy shock causally decreased inflation expectations of attentive experts. The anatomy of the experimental results verifies that the treatment effects materialize via monetary policy considerations. However, the results so far leave two important questions unanswered, which we address through the design of a follow-up experiment: (1) Are the differences in updating between macroeconomists and non-macroeconomists exclusively driven by attentiveness? (2) What is the impact of the unexpected subjective components of the December 2022 monetary policy shock?

3.3.6.1 Experimental design

Our main experiment proxies attentiveness through a primary professional focus on topics related to macroeconomics. This strategy assumes that heterogeneity in expectation updating across experts arises from differences in the information sets available to attentive and inattentive experts (e.g., Mankiw and Reis, 2002; Sims, 2003), who are otherwise similarly capable of processing new information about the macroeconomy. Another explanation for the results could be that experts differ in the subjective mental models they use to understand how the macroeconomy functions (e.g., Gabaix, 2019; Andre et al., 2022), with heterogeneity in updating reflecting systematically different models between macroeconomists and non-macroeconomists. To test this alternative explanation, we design an information provision experiment that informs experts about the information conveyed in the ECB's announcement of December 15, 2022. If our main experimental results were driven by attentiveness and information frictions, we would expect the updating behavior of non-macroeconomists in the follow-up to resemble that of macroeconomists in the main experiment. Otherwise, if non-macroeconomists fail to update their expectations after receiving information about the monetary policy shock, this would suggest that our main experimental results are driven by differences in agents' mental models.

The follow-up experiment aims to inform experts about the subjective components of the monetary policy announcement on December 15, 2022, specifically the hawkish tone and the forward guidance. For non-macroeconomists, this information should be *new* if they were inattentive to the original announcement. Responses of macroeconomic experts, for whom both components should already be *known*, serve as a natural falsification test in our setting. Importantly, we exclude the interest rate change itself from the treatment condition to avoid potential numerical anchoring effects. Testing subjective macroeconomic models requires nuanced qualitative information that enables us to examine how experts translate this input into inflation expectations. The design of our follow-up experiment also allows us to study, as a second question, the relative importance of the hawkish tone and forward guidance in the December 2022 monetary policy shock. Recent research on monetary policy communication has demonstrated that the tone of central bankers during press conferences significantly impacts expectations of agents (e.g., Gorodnichenko et al., 2023). The ECB's December 2022 announcement provides an ideal setup to study the effect of the tone vis-à-vis the information about potential future policy changes.

Our experimental design allows to abstract from the resolution of uncertainty and instead disentangles the relative importance of the subjective information conveyed in

the ECB's announcement. The logic of our approach is to construct treatment arms that are additive. The 'Tone Treatment' is a subset of the 'Tone and Forward Guidance' treatment, allowing us to first establish a benchmark estimate for the effect of the tone, and then explore the additional effect of the path surprise component in the forward guidance on top of the tone.

3.3.6.2 Subjective information and inflation expectations

Integrity of randomization. The sample of experts polled in the third survey is well-balanced for a set of key characteristics (see Table 3.B.20). We also test for differences in pre-experimental inflation expectations elicited in the first survey. The means and distributions of experts' prior inflation expectations do not differ significantly across treatment arms (see Figure 3.A.10).

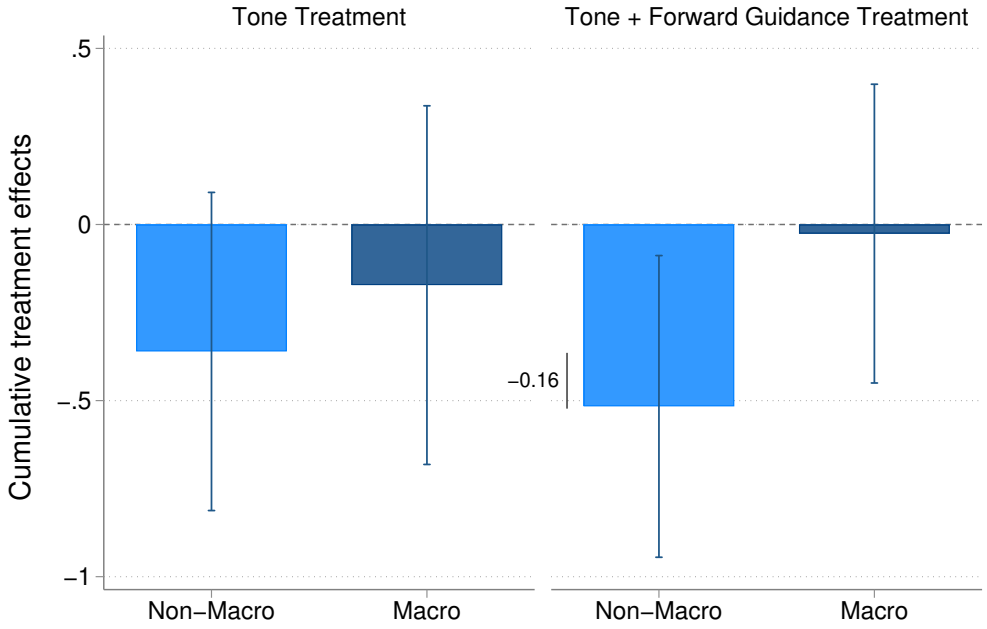
Of the 363 European experts that participated in the second survey, a total of 266 experts (73%) also answered the follow-up survey. Importantly, we observe no differential attrition across treatment arms, and response to the follow-up survey is not related to treatment status in the main experiment.

Econometric model and conceptualization. The econometric model underlying our follow-up experiment follows the full specification of our baseline experiment. We regress inflation expectations on the two treatment arms conveying the subjective information of the original press conferences on December 15, 2022. Following the specification in the main experiment, we interact the treatment indicators with whether the experts identify as macroeconomists. To rule out that the control group actively thinks about the ECB announcement, we specify a *passive* control group, i.e. we do not provide any additional information on the ECB for the control group. Nevertheless, we interpret our results as potentially downward biased, as inflation expectations of the control group should be biased towards expectations of the treatment groups if experts in the control group would think about the original monetary policy announcement.

Results. Figure (3.6) presents the main results of our follow-up experiment, plotting the treatment effects on inflation expectations relative to the control group conditional on country fixed effects, effort spent, affiliation fixed effects, and the full set of biographic and socio-demographic controls. Both the tone treatment and the integrated tone and forward guidance treatment decreased experts' inflation expectations. Non-macro experts in the first treatment arm (Tone Treatment) reduced their inflation

expectations by roughly 0.36 percentage points, compared to a zero effect for macroeconomists. For experts in the second treatment arm (Tone + Forward Guidance Treatment), we find sizable and statistically significant reductions in inflation expectations by 0.52 percentage points ($t = 1.98$) *only* for non-macroeconomist. The *additional* effect of the information about the path surprise component in forward guidance hence reduced inflation expectations by around 0.16 percentage points. For macro-experts, we again find no treatment effect.

Figure 3.6: RESULTS OF THE FOLLOW-UP EXPERIMENT—EFFECTS OF SUBJECTIVE COMPONENTS OF THE MONETARY POLICY ANNOUNCEMENT



Notes: The figure shows the treatment effects in our follow-up survey for both treatment arms (relative to the control group) when using the inflation expectations for 2023 as dependent variable.

Discussion. The results indicate that both types of subjective components are important, and play a roughly equal part in shifting experts' inflation expectations. In the scenario examined in our follow-up survey, the Tone and Forward Guidance treatment decreases inflation expectations of non-macro experts by about 0.5 percentage points. Projected onto the original experimental results from our main experiment, the cumulative effect size of our follow-up closely matches the reduction in inflation expectations for macroeconomists (0.6 percentage points). This result strongly supports our assumption that macroeconomists are more attentive to monetary policy changes than non-macroeconomists, and that differences in expectation updating are driven by

information frictions rather than variations in subjective macroeconomic models. Consistent with our results from the main experiment, we find that the treatment effect vanishes for longer-term inflation expectations (see Table 3.B.21).

Summary of the follow-up experiment. The main finding of our follow-up experiment is summarized as follows:

Result #3 *(a) Non-macroeconomists update expectations similarly than macroeconomic experts when provided with information. Differentials in settings of natural updating are therefore driven by attentiveness. (b) Both the hawkish tone and the path surprise entailed in the ECB's forward guidance matter for the formation of inflation expectations of experts.*

3.4 Expert Explanations and Households' Expectations

Having established that economic experts immediately adjust their inflation expectations in response to a monetary policy shock, we now investigate whether this capability enables them to act as intermediaries, effectively explaining the implications of such shocks to households. The presence of experts in the media, the inability of other agents to similarly update expectations in response to the shock, and the reduction in forecast errors after their expectation update offers great potential for experts to serve as intermediaries. In this section, we explore how households update expectations when receiving expert explanations and whether such explanations are consequential for households' spending intentions.

3.4.1 Survey Design

Setting. We pre-registered our experiment in September 2023, when inflation rates in Europe and Northern America slightly decreased, but most countries included in our main experiment were still locked in a high-inflation environment. Our experiment is conducted in Germany, the largest economy among EMU member states, where residents are more inflation-averse compared to those in other European countries (e.g., Kiss and Strasser, 2024). Our experiment was scheduled precisely one week after

another interest rate change on September 14, 2023, to refer to a recent update in monetary policy in our treatment interventions.

Our survey includes a sample of 1,260 representative German households. After asking for socioeconomic characteristics of households and using an attention check, we elicit households' (prior) beliefs on the inflation rate in German for 2023 using point forecasts. The mean prior belief on the inflation rate is 12.3% (Huber robust mean: 11.5%), which highlights that Germany underwent a high-inflation period during the time of the survey (September 2023).

Treatment conditions. Respondents in our experiment are allocated randomly into a control group and three treatment arms. We specify an active control group in order to prevent biases initiated by numerical anchoring. An active control group also helps isolating treatment effects from placebo responses, mitigates biases like the Hawthorne effect, and ensures comparability between groups. Our active control group ($N = 314$) receives information about recent population growth in Germany.

Our survey includes three treatment conditions. The first intervention ($N = 318$) informs individuals that the central bank has increased interest rates, serving as a benchmark to assess whether individuals are able to link the objective information about an increase in interest rates to inflation expectations. The second intervention ($N = 317$) provides an explanation about the underlying mechanisms by experts, explaining that interest rate increases, as implemented by the European Central Bank last week, lower the inflation rate as it reduces the demand for goods. The third intervention ($N = 311$) combines the explanation with a numerical guidance, additionally informing participants that after an interest rate hike, similar to the rate hike by the European Central Bank last week, leading economic experts expect inflation rates to be 1.2 percentage points lower.

Expectations and beliefs. After the treatments, we first ask whether households agree that the inflation rate in Germany for 2023 will be lower than the inflation rate in 2022. We then elicit households' numerical posterior beliefs on the 2023 inflation rate using a distributional question, where respondents assign probabilities to different bins of potential future inflation rates. Using the midpoints of the bins, we derive for each respondent the implied (posterior) mean and standard deviation of their inflation rate expectations. Lastly, we ask for households' spending intentions, media consumption, and trust in experts and journalists.

3.4.2 Empirical Strategy

Integrity of randomization. The sample of households is well-balanced for a set of key characteristics across the treatment arms (see Table 3.B.22), both individually and jointly. We also observe no differentials in attrition across treatment conditions.

Updating of inflation expectations. In the first step, we examine whether the provided expert explanations were successful in shifting households' inflation expectations. We estimate the following regression

$$\pi_{h,(t)}^{\text{post.},j} = \sum_{z=1}^{3} \gamma^{j,z} T_h^z + \alpha \pi_h^{\text{prior}} + \mathbf{X}_h \boldsymbol{\mu} + \zeta_t + \varepsilon_{h,(t)}^j, \tag{3.4}$$

where we regress our main posterior variables j (including posterior inflation expectations, posterior qualitative inflation, and posterior inflation uncertainty) for household h on our three treatment dummies z, households' prior inflation expectation (π_h^{prior}), fixed effects for the day respondents filled our survey (ζ_t), and a rich set of socio-economic and regional control variables ($\mathbf{X}_h$). The parameters $\gamma^{j,z}$ (with z = 1, 2, 3) identify how households causally update their inflation expectations in response to the treatments.

Inflation expectations and spending. In a second step, we examine the consequentiality of shifting household expectations via experts' explanations. We estimate the following regression,

$$\text{Spend}_{h,(t)}^j = \alpha^j \pi_h^{\text{post.}} + \beta \pi_h^{\text{prior}} + \mathbf{X}_h \boldsymbol{\mu} + \zeta_t + \varepsilon_{h,(t)}^j, \tag{3.5}$$

where we relate households' spending intentions on category j to households' posterior inflation expectations, conditional on fixed effects and controls. The OLS estimate of α^j does not allow for a causal interpretation. For instance, individuals with a generally more optimistic or pessimistic outlook may respond differently to both the question on their posterior beliefs and those regarding their personal economic situation and spending desires. Additionally, causality may run in the opposite direction, with personal financial situations influencing macroeconomic expectations (e.g., Kuchler and Zafar, 2019). To solve the endogeneity issue that expectations are not random, we instrument posterior beliefs $\pi_h^{\text{post.}}$ with the random assignment to the treatment

dummy for receiving the narrative along with the point estimate (treatment arm 3).[11] The parameters α^j hence capture the causal impact of a shift in inflation expectation, caused by expert explanations, on households' spending intentions.

3.4.3 Main Results for Households

3.4.3.1 Expert narratives and expectations updating

Table (3.3) presents the impact of expert explanations about the monetary policy shock on households' inflation expectations. Consistent with previous evidence showing that households struggle to understand the implications of monetary policy changes, we find no effect on inflation expectations when households are informed that the ECB has raised interest rates (without any embedded expert decoding). In contrast, the results from our main treatment intervention regarding expert narratives provide strong evidence that economic experts can act as intermediaries, effectively conveying monetary policy to a broader audience.

Explaining the mechanism of an increase of monetary policy rates to households reduces posterior inflation expectations by 0.3 percentage points (relative to a mean of prior inflation of 12.3%). This result suggests that expert explanations of monetary policy can meaningfully shape inflation expectations, even in the absence of explicit quantitative guidance on its effects. When providing quantitative information along with the narrative, the effects on expectation updating are stronger (0.67 percentage points).

Column (2) reports similar results when estimating the causal effect of expert explanations on a qualitative outcome, showing that the expert narratives substantially increased households' agreement with the statement that the inflation rate in 2023 will be lower than in 2022. The treatment effect for the interest hike treatment arm is again very close to zero and not statistically significant. At the same time, supplementing the narrative with a numerical point estimate reduces inflation expectations by approximately twice as much as the standalone narrative intervention. The effects on the

[11] We use responses from the active control group and the group receiving ineffective intervention that informs respondents about the recent monetary policy change along those included in the third treatment arm for estimation. Our setting employs the third treatment arm because it strongly shifts inflation expectations and delivers a particularly strong first stage. We obtain qualitative identical results when we, following Coibion et al. (2022) and Coibion et al. (2023a), instrument households' posterior inflation expectations with the treatment dummy and the interaction of the treatment with households' prior inflation expectations.

Table 3.3: EXPERTS' NARRATIVES AND HOUSEHOLDS' INFLATION EXPECTATIONS — MAIN RESULTS

	(I) Post. inflation exp.	(II) Qualitative post. (z-scored)	(III) Uncertainty
Interest hike	-0.124	0.021	-0.044
	(0.188)	(0.075)	(0.077)
Expert narrative	-0.298*	0.153**	0.051
	(0.181)	(0.074)	(0.079)
Expert narrative & point estimate	-0.675***	0.291***	0.032
	(0.187)	(0.078)	(0.081)
Observations	1,260	1,260	1,260
R-Squared	0.100	0.129	0.150
Day Fixed Effects	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The table shows treatment effects of our three treatment arms on future inflation expectations. The dependent variable in Column I is the mean of respondents' subjective probability distribution of the inflation rate in 2023. The dependent variable in Column II is the z-scored agreement with the following statement: 'The inflation rate in 2023 will be lower than in 2022'. The dependent variable in Column III is the standard deviation of respondents' subjective probability distribution of the inflation rate in 2023. All regressions include the following set of control variables: gender, age, household income, prior inflation expectations, household size, education levels, migration background, region, trust level in experts, survey time, and day fixed effects. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

qualitative posterior also underscore that participants have understood the mechanisms of monetary policy as explained by experts.

Reassuringly, we find that our treatment did not reduce the level of uncertainty in households' (posterior) inflation expectations (Column 3). This suggests that our active control group design generates clean exogenous variation in expected inflation rates, while potential side-effects of information provision appear to be limited.

Robustness. Our main results are robust to using Huber regression (see Panel A of Table 3.B.23) or truncating households' (prior) expectations (<=30%) to exclude outliers (see Panel B of Table 3.B.23).

Discussion. Our results show that expert explanations shift inflation expectations of households. While the explanation of how the policy works is effective on its own, the effects are strongest when such explanations are accompanied by numerical guidance regarding the potential impact of the policy. The effectiveness of the standalone narra-

tive also heavily depends on context. Figure (3.A.11) shows that the expert narrative treatment with a numerical anchor does not significantly depend on households' interest in inflation or trust in experts. For the effectiveness of the standalone narrative, however, trust in experts and interest in topics on inflation are important mediators. This pattern suggests that individuals who are more engaged with inflation-related topics and more trusting of experts are more receptive to the qualitative narrative offered by the experts. Taken together, our results provide compelling evidence that experts possess significant potential to serve as intermediaries, effectively disseminating information about monetary policy shocks to the broader public. The impact on expectation formation is most pronounced when experts provide a numerical assessment of the policy's likely effects in conjunction with an explanation of the policy mechanisms.

3.4.3.2 Are expert explanations consequential?

Having established that expert explanations shift posterior inflation expectations, we next explore whether narratives of experts also shape actual behavior of individuals.

Do expert explanations help form more accurate expectations? Comparing participants' inflation expectations across treatment interventions with realized inflation rates in 2023, we find that expert explanations significantly improve the accuracy of households' inflation expectations (a 3.28 percentage points reduction in errors for the standalone narrative and a 3.69 percentage points reduction for the combination of the narrative with the point estimate, relative to the control group). If households form economic decision based on their expectations, expert narratives should enable households to make more informed and substantiated economic choices.

Do shifts in expectations causally impact spending decisions? A reduction in inflation should lead to an increase in the real interest rate, which, according to the consumption Euler equation, makes saving more attractive. This effect is likely to be magnified by the ECB's increase in nominal interest rates. In accordance with rational expectations, individuals should respond to the expert narratives by increasing savings, reallocating consumption to future periods, and reducing current expenditure. In our IV estimates on the consequentiality of expert narratives, we estimate behavioral effects that are consistent with this prediction (Table 3.4). The exogenously generated reduction in inflation expectations lead respondents to causally *reduce* their intended spending on restaurants and leisure activities (Panel B, Columns 2 and 4). We find no treatment effects on spending intentions for supermarket purchases, suggesting that

the impact of changes in inflation expectations on spending decisions is contingent on the type of goods. Specifically, spending on luxury items—which can be more easily substituted with savings—appears more responsive to shifts in inflation expectations, while spending on subsistence goods, with limited room for adjustment, is less affected.

In line with Georgarakos et al. (2024), but in contrast to Coibion et al. (2022) and Coibion et al. (2023a), we find suggestive evidence that lower inflation expectations also decrease spending intentions on durable goods ($t = 1.21$). This result aligns with predictions from standard behavioral macroeconomic models, where consumption of durable goods is particularly susceptible to intertemporal shifts.

Table 3.4: INFLATION EXPECTATIONS AND SPENDING — INSTRUMENTAL VARIABLE RESULTS

	(1) Durable goods (z-scored)	(2) Restaurant (z-scored)	(3) Supermarket (z-scored)	(4) Leisure (z-scored)
	Panel A: First stage			
Expert narrative	-0.707***	-0.707***	-0.707***	-0.707***
	(0.162)	(0.162)	(0.162)	(0.162)
	Panel B: Second stage			
Inflation expectations	-0.124	-0.300***	-0.027	-0.226**
	(0.102)	(0.113)	(0.096)	(0.104)
F-value	19.04	19.04	19.04	19.04
Observations	943	943	943	943
Day Fixed Effects	Yes	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: The table shows the results of our household survey experiment, reporting the effect of the third treatment arm on inflation expectations (Panel A), and the subsequent effects on households' spending intentions (Panel B). Column 1 reports the effect of reduced inflation expectations on the z-scored agreement to the question, whether respondents think that 'now is a good or bad time to buy durable goods such as cars, furniture or household appliances?'. Columns 2 to 4 report the effect of reduced inflation expectations on z-scored answers to the question whether respondents 'intend to spend more, less or about the same on [item] in the next 4 weeks? Compare your intentions with what you have spent in the last 4 weeks.' All regressions include the following set of control variables: gender, age, household income, prior inflation expectations, household size, education levels, migration background, region, trust level in experts, survey time, and day fixed effects. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Robustness. Our results are stable across a series of robustness checks usually conducted when estimating the causal effects of inflation expectations on spending decisions. Our instrument creates variation only on the first-moment of inflation expectations. The negative effects of inflation expectations on spending decisions persist when we include the second-order moment to isolate the effect of the first-order moment (see also Georgarakos et al., 2024), and Table 3.B.24). The results are also robust when

we estimate the effect only against the active control group (see Table 3.B.25). The effects are also not sensitive to the exclusion or inclusion of covariates.

Summary of results for households. Our fourth main finding is summarized as follows:

Result #4 *Experts explanations ('narratives') are paramount for the effectiveness when communication information about monetary policy. Expert statements can guide households' inflation expectation – this, in turn, affects households' spending decisions.*

3.5 Conclusion

The limited capacity of households to grasp complex economic mechanisms often undermines the effectiveness of policy interventions. In this paper, we demonstrate that explanations provided by economic experts can effectively bridge the gap between economic policies and individuals' behavioral responses. Our experimental results demonstrate that economic experts respond rapidly to monetary policy shocks, exhibiting immediate and significant updates to their inflation expectations. This behavior sharply contrasts with the more sluggish and less precise reactions we observe among households and firms, highlighting the importance of experts as interpreters of complex economic signals. We further show that expert explanations, which translate monetary policy signals into accessible and understandable insights, have sizable impact on households' inflation expectations, accuracy of forecasts, and even spending intentions.

Our findings have significant implications for both policy and future research. From a policy perspective, central banks and economic authorities may benefit from collaborating more closely with economists to disseminate complex policy information in ways that are comprehensible to the broader public. The effectiveness of expert explanations in shaping public expectations underscores the importance of expert communication as an essential tool in economic governance. Finally, our study opens several avenues for future research. While we have focused on the role of experts as mediators of monetary policy, further exploration is needed to understand the broader dynamics between expert opinions, public perceptions, and macroeconomic outcomes. Additionally, examining the motivations behind experts' communications, including their biases and the potential for strategic influence, could provide deeper insights into their role as both advisors and influencers. Overall, our work adds a critical layer to the understanding of how economic policies are communicated and how those communications shape the macroeconomic landscape.

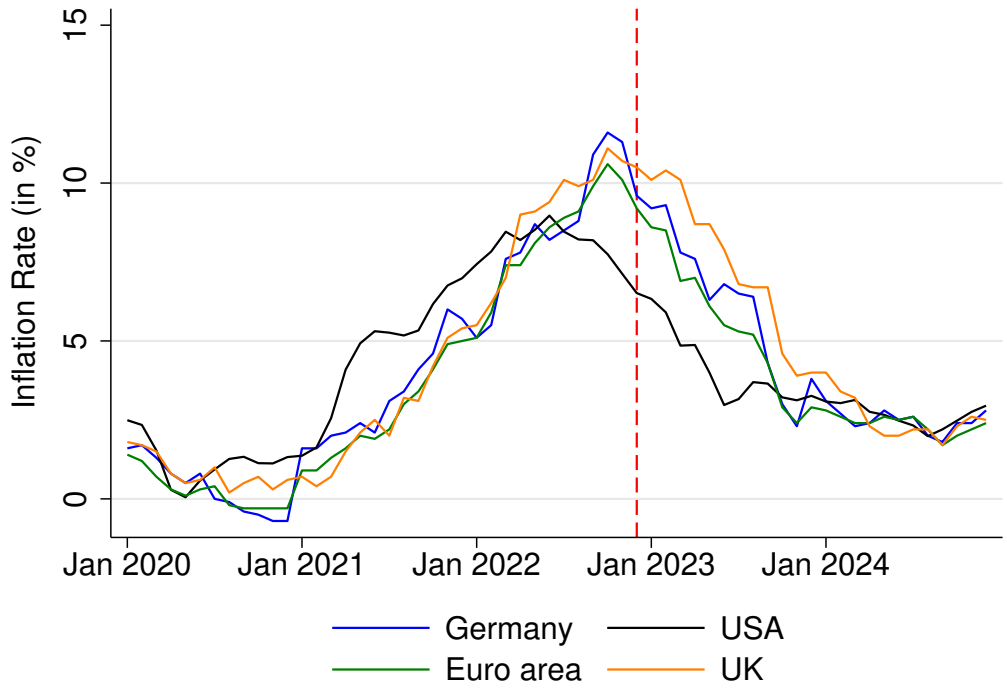
Appendix to Chapter 3

This appendix presents additional details on the surveys and additional results:

- Section 3.A provides supplementary figures.
- Section 3.B provides supplementary tables.
- Section 3.C provides additional details on the survey structure.
- Section 3.D provides extensions of the experimental results.
- Section 3.E provides survey instructions.

3.A Supplementary Figures

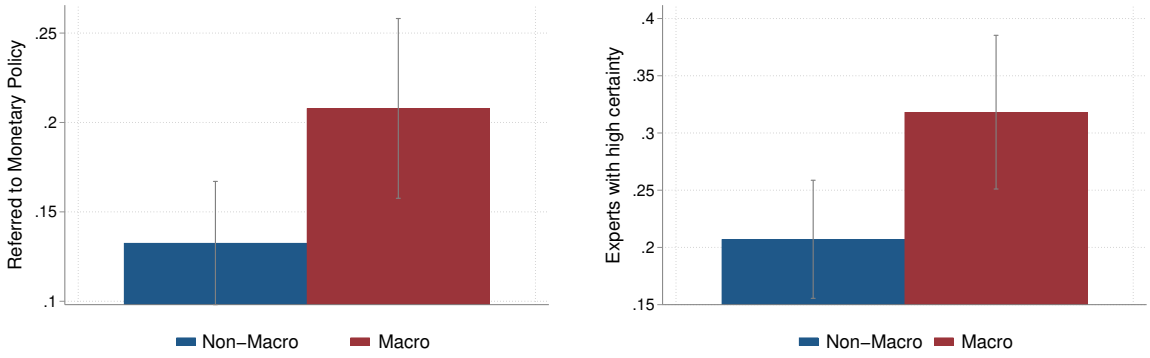
Figure 3.A.1: DEVELOPMENT OF INFLATION IN KEY REGIONS DURING 2020 TO 2024



Notes: The figure shows the development of the inflation rates in Germany, in the United States of America, the EU area, and in the UK. The dashed red line indicates the time of our main expert experiment.

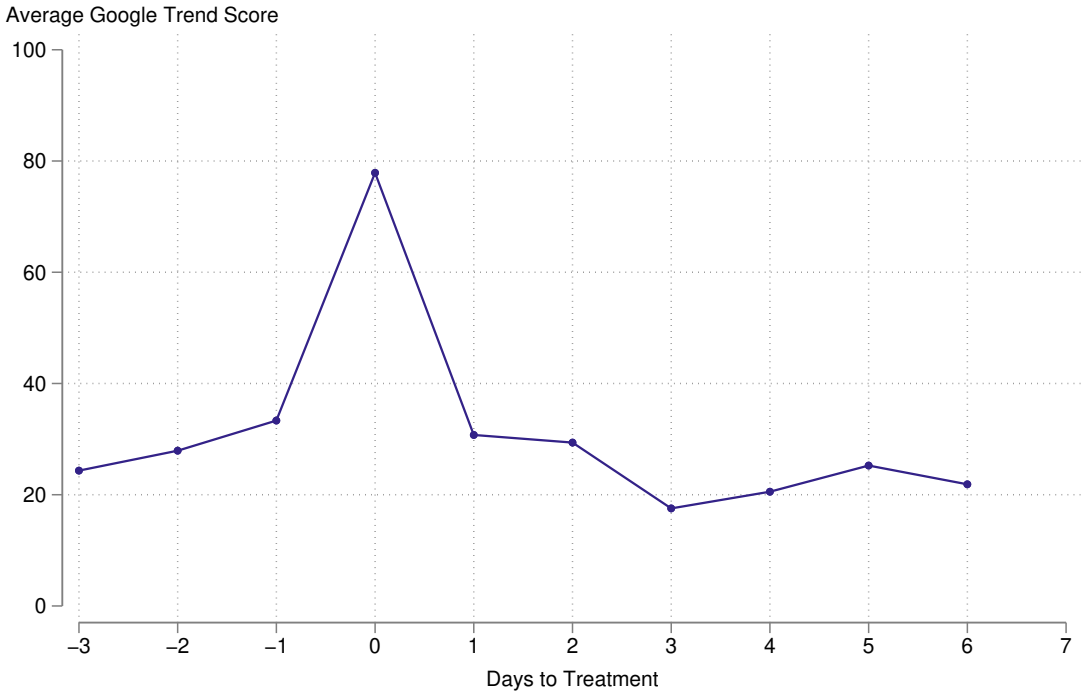
Figure 3.A.2: ATTENTION OF EXPERTS

(a) Reference to Monetary Policy **(b) (Un-)Certainty in Expectations**



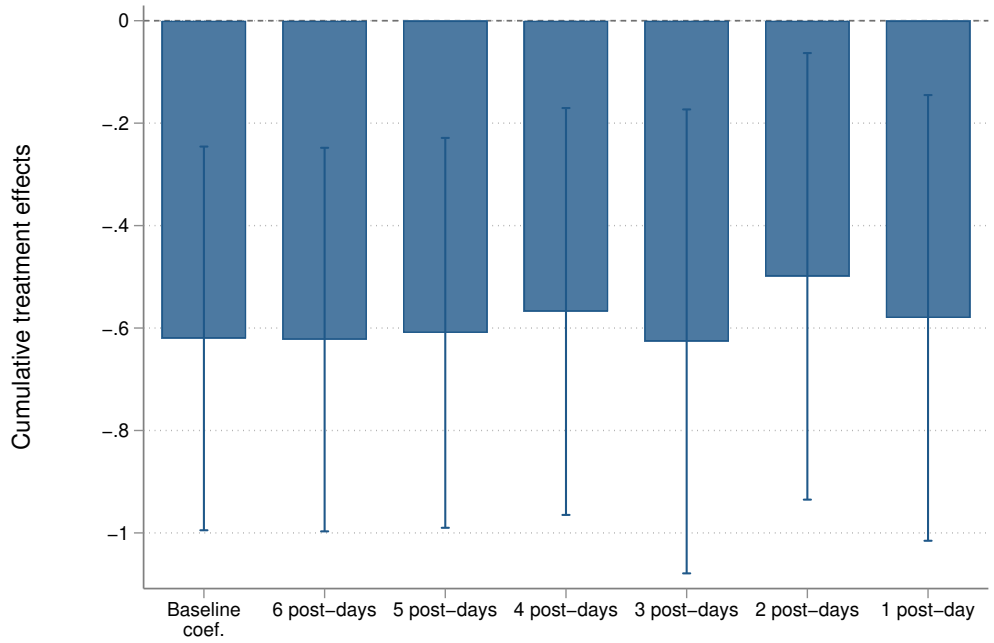
Notes: The figure shows differentials between macroeconomists and non-macroeconomists across answers to open-ended text questions on the perceived causes of inflation (Panel A) and the level of uncertainty in inflation forecasts (Panel B). We manually code the reported causes of inflation. Panel A shows the fraction of responses that refer to monetary policy in free-text answers. Panel B measures uncertainty based on integer values reported for inflation forecasts. We classify respondents as macroeconomists if they report to have a primary focus on topics with JEL code *'E: Macroeconomics and Monetary Economics'*.

Figure 3.A.3: GOOGLE TRENDS AROUND ANNOUNCEMENT



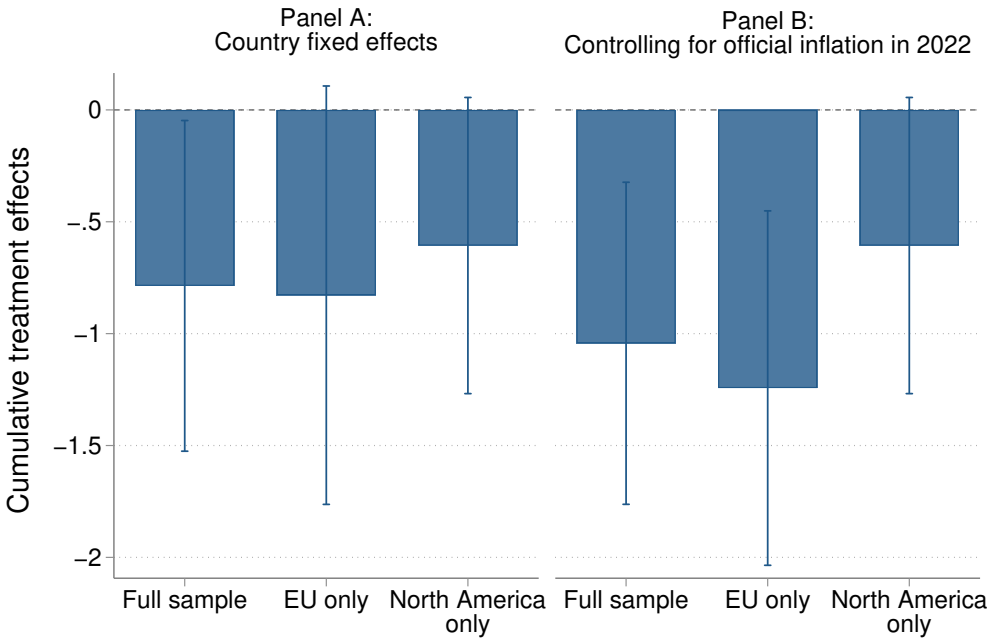
Notes: The figure shows the average Google trend score measuring the attention the respective central banks (the ECB, Fed, Norges Bank, Bank of England, and the SNB) have received in Google searches in the countries included in our survey experiment per day. Attention is measured on a scale running from 0 (no attention) to 100 (maximum of possible attention). The data for individual countries is aggregated to reflect the overall trend score across countries in our survey.

Figure 3.A.4: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—RESTRICTING POST-TREATMENT DAYS



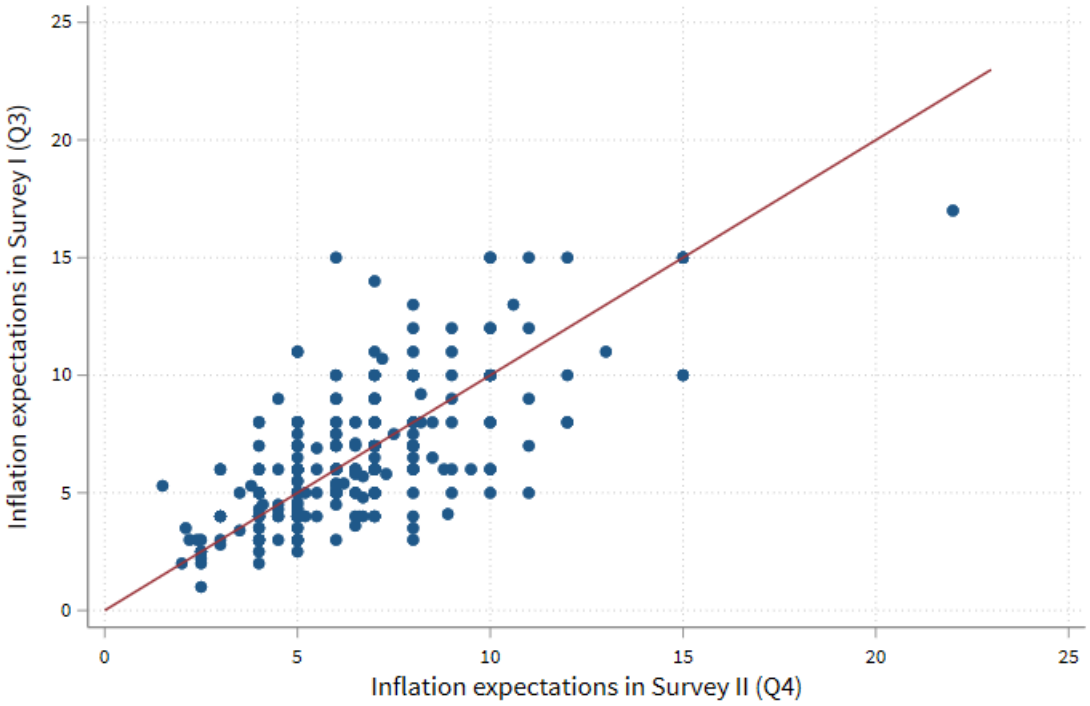
Notes: The figure shows our baseline coefficient and treatment effects when we restrict the included post-treatment days.

Figure 3.A.5: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—WITHIN-DAY RESULTS



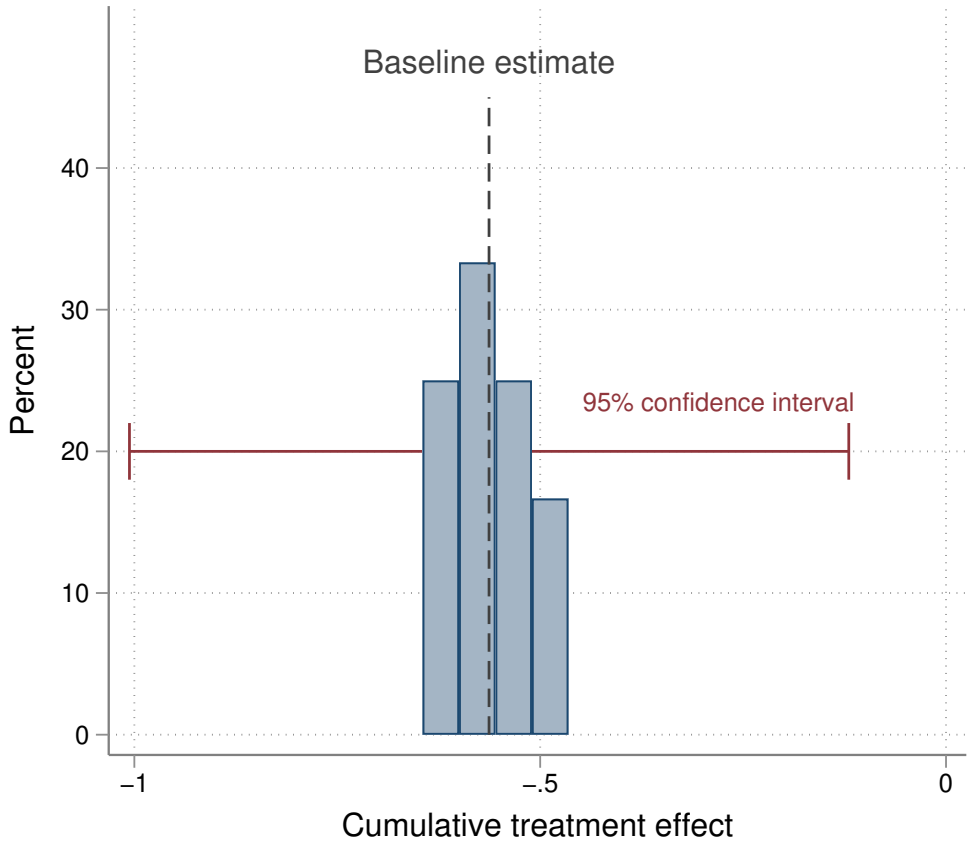
Notes: The figure visualizes the cumulative treatment effect for attentive experts when exploiting within-day variation. Panel A presents results for our baseline specification (using country fixed effects). Panel B presents results when we instead control for the official inflation rate in 2022. The sample consists of 148 experts, out of which 88 are in the treatment group and 60 are in the control group.

Figure 3.A.6: SCATTERPLOT OF PRIOR AND POSTERIOR EXPECTATIONS



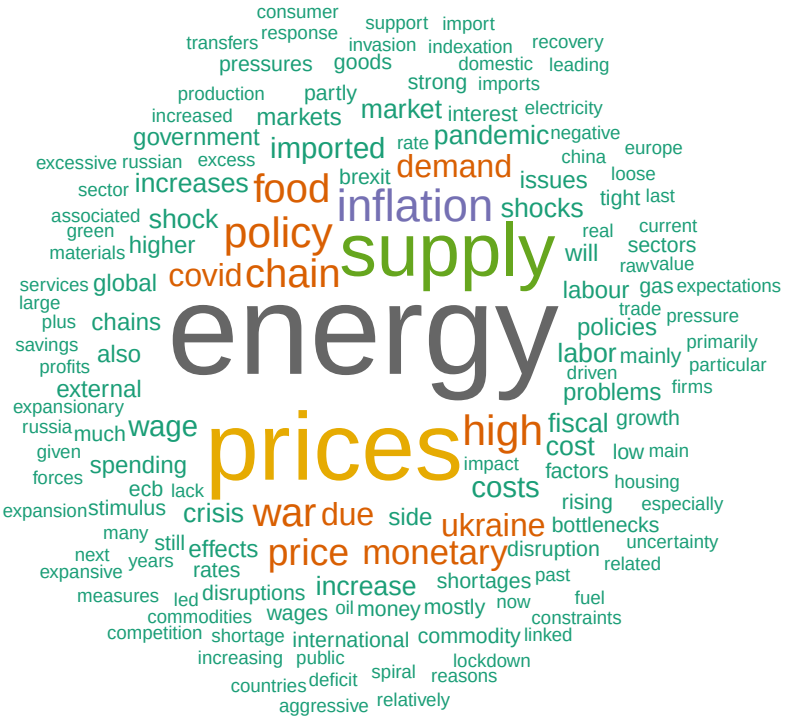
Notes: The figure plots posterior inflation expectations against prior inflation expectations for 2023. The black line represents the 45 degree line, deviations from the 45 degree line represent updating of beliefs.

Figure 3.A.7: HISTOGRAM OF PARAMETER ESTIMATES USING JACK-KNIFE REGRESSIONS



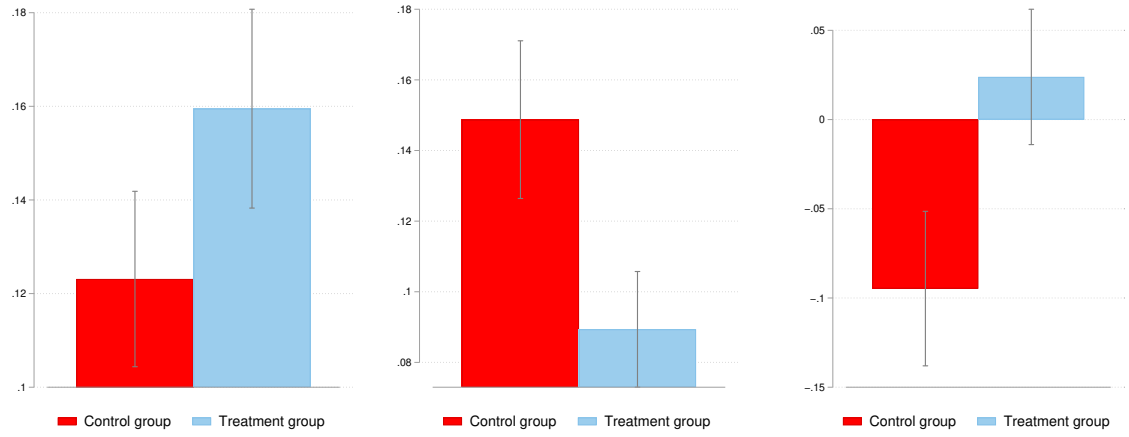
Notes: The figure shows a histogram of parameter estimates in our baseline model using jack-knife regressions, where we exclude one country at a time ('leave-one-out'). The dotted vertical line represents our baseline estimate of the cumulative treatment effect in Table 3.1, Column IV. All parameter estimates are well within the 95% confidence interval of the baseline estimate.

Figure 3.A.8: WORD CLOUD OF WORDS MENTIONED IN FREE-TEXT ANSWERS ON THE CAUSES OF INFLATION



Notes: The figure shows the words participants mention in free-text answers to the question on the causes of inflation. The size of the words corresponds with the relative frequency of the word in experts' answers. After clearing the text from stop words, we identify a total of 1,067 distinct words mentioned in the answers.

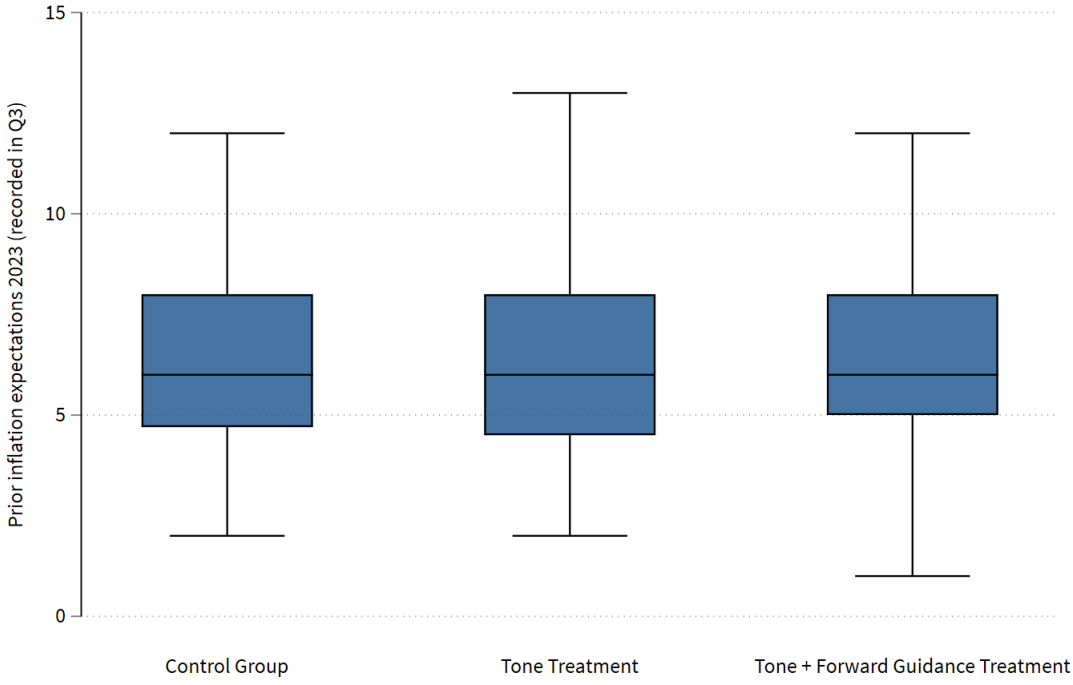
Figure 3.A.9: MONETARY POLICY ANNOUNCEMENT AND THE TONE OF ANSWERS TO OPEN-ENDED QUESTIONS.



(a) Positive sentiment. **(b) Negative sentiment.** **(c) Sentiment score.**

Notes: The figure shows the result of a sentiment analysis applied to the written text of experts answering the question about the causes of inflation. The figure relates the treatment status to the frequency of components with positive sentiment (Panel a), negative sentiment (Panel b), and the overall compound sentiment score (Panel c). The compound sentiment score reflects the polarity (positive or negative) as well as the strength of emotions experts have towards inflation.

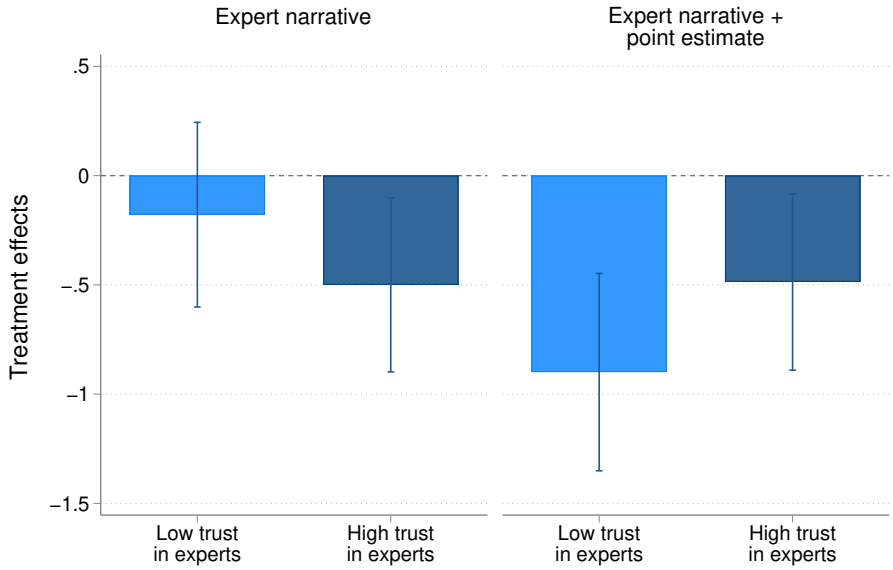
Figure 3.A.10: PRE-TREATMENT INFLATION EXPECTATIONS FOR CONTROL AND TREATMENT GROUPS IN THE FOLLOW-UP EXPERIMENT



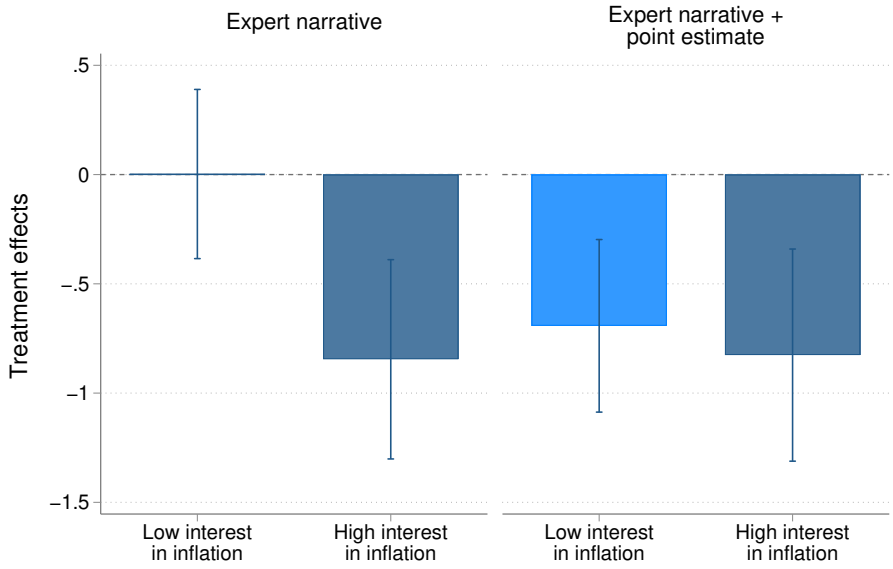
Notes: The figure shows the prior inflation expectations (elicited in the Q3 EES wave) of the participants in our follow-up experiment for the year 2023.

Figure 3.A.11: TREATMENT EFFECTS DEPENDING ON TRUST IN EXPERTS AND INTEREST IN INFLATION

(a) Trust in experts



(b) Interest in inflation



Notes: The figure visualizes treatment effects for households by low and high levels of trust in experts (Panel A) and by low and high interest in inflation (Panel B). High trust in experts and high interest in inflation takes a value of one if households' answer takes a value of 4 or 5 (on a 1-5 scale).

3.B Supplementary Tables

Table 3.B.1: SUMMARY STATISTICS: EXPERT SAMPLE

	Mean	Standard deviation	Median	Observations
Personal characteristics:				
Female	0.09	0.29	0	442
Year born	1965	11.84	1965	442
Academic characteristics:				
Macroeconomist	0.40	0.49	0	442
Citations	5,993	13,768	1,926	404
$\text{Citations}_{Academics}$	6,690	14,239	2,557	286
Twitter Follower	1,846	4,443	389	137
Type of institution:				
University	0.66	0.48	1	442
Research institute	0.12	0.33	0	442
Central Bank	0.05	0.23	0	442
Private sector	0.08	0.27	0	442
Public Sector	0.05	0.22	0	442
Location of institution:				
EMU countries	0.69	0.46	1	442
USA + Canada	0.18	0.38	0	442
Norway + UK + Switzerland	0.13	0.34	0	442

Notes: This table displays summary statistics on experts' background characteristics included in our main expert experiment in December 2022.

Table 3.B.2: REPRESENTATIVENESS OF HOUSEHOLD SURVEY

(I) **Variable**	(II) **German Population**	(III) **HH-Survey**
Male	0.50	0.51
East	0.15	0.15
Employed	0.77	0.74
Age: 18 - 29 y.	0.20	0.17
Age: 30 - 39 y.	0.19	0.21
Age: 40 - 49 y.	0.17	0.17
Age: 50 - 59 y.	0.23	0.25
Age: 60 - 70 y.	0.21	0.20
HH-income under 1,000	0.06	0.06
HH-income 1,000-2,000	0.16	0.16
HH-income 2,000-3,000	0.22	0.21
HH-income 3,000-4,000	0.21	0.21
HH-income 4,000-5,000	0.21	0.18
HH-income 5,000+	0.14	0.13
Low education	0.26	0.20
Middle education	0.33	0.37
High education	0.41	0.44
Observations		1,260

Notes: The table reports the mean levels of key biographic and geographic characteristics of the German population (Column II), and participants included in our household experiment sample (Column III).

Table 3.B.3: BALANCE TESTS EXPERT SAMPLE—SAMPLE MEANS OF CONTROL AND TREATMENT GROUP AND T-TESTS FOR DIFFERENCES IN MEAN CHARACTERISTICS

(I) **Variable**	(II) **Control (mean)**	(III) **Treatment (mean)**	(IV) **Difference (t)**
Gender (1 = female)	0.115	0.074	0.041
			(1.47)
Year born	1965	1965	-0.051
			(0.04)
Field of study (1 = Academia)	0.655	0.657	-0.002
			(0.04)
Sector (1 = Central Bank)	0.050	0.058	0.008
			(0.36)
Sector (1 = Research institute)	0.125	0.124	0.001
			(0.03)
Sector (1 = Private Sector)	0.070	0.091	-0.021
			(0.80)
Sector (1 = Public Sector)	0.055	0.050	0.005
			(0.25)
Macroeconomist	0.380	0.421	-0.041
			(0.88)
Time used for survey (in seconds)	3221	2423	797
			(0.39)
Google scholar citations (# citations)	5055	6761	1705
			(1.24)

Notes: The table reports the mean levels of key biographic, geographic, and bibliographic characteristics of participants included in our sample for the control group (Column II) and the treatment group (Column III). The differences between the means are reported in Column IV, with test statistics of a two-sample t-test reported in parentheses.

Table 3.B.4: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—RESULTS FOR EUROPE AND NORTH AMERICA

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
	Panel A: Only European experts			
Treatment (1 = Post Announcement)	0.177	0.173	0.194	0.281
	(0.284)	(0.284)	(0.284)	(0.280)
Macro	0.072	0.073	0.033	0.137
	(0.279)	(0.280)	(0.277)	(0.281)
Treatment × Macro	-0.796**	-0.789**	-0.729*	-0.868**
	(0.389)	(0.390)	(0.383)	(0.386)
Cumulative effect	-0.619**	-0.615**	-0.536**	-0.587**
	(0.270)	(0.271)	(0.268)	(0.272)
Observations (# experts)	363	363	363	363
Countries	22	22	22	22
R-Squared	0.499	0.499	0.508	0.525
	Panel B: Only North American experts			
Treatment (1 = Post Announcement)	0.160	0.183	0.275	0.289
	(0.409)	(0.410)	(0.399)	(0.406)
Macro	-0.311	-0.278	-0.081	-0.114
	(0.328)	(0.328)	(0.298)	(0.304)
Treatment × Macro	-0.819	-0.888*	-1.038**	-0.975*
	(0.492)	(0.504)	(0.514)	(0.542)
Cumulative effect	-0.659**	-0.705***	-0.763***	-0.686**
	(0.259)	(0.274)	(0.278)	(0.300)
Observations (# experts)	79	79	79	79
Countries	2	2	2	2
R-Squared	0.149	0.154	0.201	0.231

Notes: The table shows the baseline results of our main survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts in Europe (Panel A) and in North America (Panel B). The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.5: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—UPDATE OF EXPECTATIONS

Dependent variable: Update in inflation expectations (posterior - prior)				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.197	0.198	0.192	0.253
	(0.264)	(0.264)	(0.261)	(0.260)
Macro	0.016	0.018	-0.045	0.001
	(0.239)	(0.239)	(0.242)	(0.252)
Treatment × Macro	-0.784**	-0.782**	-0.708**	-0.800**
	(0.339)	(0.340)	(0.339)	(0.342)
Cumulative effect	-0.587**	-0.584**	-0.516**	-0.547**
	(0.239)	(0.240)	(0.238)	(0.248)
Observations (# experts)	301	301	301	301
Countries	24	24	24	24
R-Squared	0.471	0.471	0.491	0.497
Equal. (p-val)	0.89	0.89	0.83	0.95

Notes: The table reports the results of our survey experiment on expectation updating, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. 'Equal. (p-val)' reports p-values on a Wald test that compares the estimated parameters of Columns (I)-(IV) with the treatment effects identified in our baseline specifications in Table 3.1. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.6: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—EFFECT HETEROGENEITY

Dependent variable: Inflation expectations for the year 2023

Specification	Coefficient	(SE) p-val.	R-Squared	N
1. By gender				
Cumulative Treatment	0.988	(0.845)	0.52	442
p-value on Wald test of equal parameters		0.067		
2. By age				
Cumulative Treatment	-0.257	(0.228)	0.52	442
p-value on Wald test of equal parameters		0.179		
3. By affiliation				
Cumulative Treatment	-0.080	(0.224)	0.52	442
p-value on Wald test of equal parameters		0.032		
4. By effort				
Cumulative Treatment	-0.070	(0.174)	0.52	442
p-value on Wald test of equal parameters		0.004		
5. By citations				
Cumulative Treatment	-0.241	(0.227)	0.53	404
p-value on Wald test of equal parameters		0.157		
6. By prior expectations				
Cumulative Treatment	-0.261	(0.506)	0.64	301
p-value on Wald test of equal parameters		0.550		

Notes: The table shows the results of our estimations on the effect of the monetary policy change by the central banks in December 2022 on inflation expectations of economic experts. The table presents estimates when we explore treatment heterogeneity. Results are obtained using the full model specifications of Column (IV) of Table (3.1), including interaction terms between the treatment variable and individual-level characteristics of participants. Each specification includes the treatment variable, the moderator variable and the interaction term; for brevity, we only report the cumulative treatment effect. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The row entitled '*p-value on Wald test of equal parameters*' reports the p-value for a test of equality between the baseline estimates of Column (IV) of Table (3.1) and the parameter estimates of the individual row. 'Female' is a dummy variable that is 1 for female respondents (zero otherwise), 'Age' is a dummy variable that is 1 if respondents are older than the median age of our sample. 'Affiliation' is a dummy variable that is 1 if respondents work at a university (the largest group of respondents in our sample). 'Effort' measures the time (in seconds) respondents used to fill out our survey. 'Citations' is a dummy variable that is 1 if respondents have more Google scholar citations than the median number of our sample. Finally, prior expectations are the inflation expectations of participants elicited in the first survey (EES wave in Q3).

Table 3.B.7: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—UPDATE OF EXPECTATIONS WITHIN TREATMENT DAY

Dependent variable: Update in inflation expectations (posterior - prior)				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.637	0.653	0.748	0.847
	(0.614)	(0.619)	(0.576)	(0.691)
Macro	0.387	0.323	0.535	0.599
	(0.533)	(0.532)	(0.503)	(0.613)
Treatment × Macro	-1.293	-1.233	-1.403*	-1.600*
	(0.792)	(0.792)	(0.712)	(0.827)
Cumulative effect	-0.656*	-0.581	-0.655	-0.753*
	(0.381)	(0.388)	(0.238)	(0.409)
Observations (# experts)	98	98	98	98
Countries	21	21	21	21
R-Squared	0.547	0.549	0.565	0.578

Notes: The table reports the results of our survey experiment on expectation updating, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts using only experts that answered on the day of the central banks' announcements. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.8: FALSIFICATION — MONETARY POLICY ANNOUNCEMENT AND ECONOMIC POLICIES AND POLITICAL SITUATION

	(I) Economic Policy	(II) Future Challenges	(III) Government Performance	(IV) Political Stability
Treatment (1 = Post Announcement)	2.834	-1.234	-2.724	2.829
	(4.967)	(4.920)	(5.153)	(5.730)
Macro	0.661	0.869	2.772	3.706
	(5.569)	(5.781)	(6.146)	(6.641)
Treatment × Macro	-2.482	-4.488	-1.372	-13.220
	(7.642)	(7.872)	(8.515)	(8.715)
Cumulative effect	0.352	-5.721	-4.095	-10.390
	(5.840)	(6.116)	(6.834)	(6.511)
Observations (# experts)	388	392	380	374
Countries	23	23	23	23
R-Squared	0.216	0.188	0.193	0.244

Notes: The table shows the result of our first falsification test, reporting the effect of the December 2022 announcements of the central banks on evaluations of the quality of economic policies (column 1), how well economic policies address challenges of the future (column 2), government performance (column 3), and political stability (column IV). Answers to those four questions are on a range from -100 to +100. All models use the full baseline specification.

Table 3.B.9: MONETARY POLICY ANNOUNCEMENT AND ECONOMIC GROWTH EXPECTATIONS—RESULTS

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
	Panel A: Short-run growth expectations (year 2023)			
Treatment (1 = Post Announcement)	-0.023	-0.023	0.016	0.051
	(0.175)	(0.175)	(0.172)	(0.173)
Macro	0.011	0.007	0.019	0.057
	(0.198)	(0.197)	(0.189)	(0.192)
Treatment × Macro	-0.396	-0.402	-0.423*	-0.455*
	(0.257)	(0.257)	(0.249)	(0.251)
Cumulative effect	-0.419**	-0.425**	-0.407**	-0.404**
	(0.189)	(0.189)	(0.180)	(0.179)
Observations (# experts)	433	433	433	433
Countries	24	24	24	24
R-Squared	0.310	0.311	0.337	0.343
	Panel B: Medium-run growth expectations (year 2024)			
Treatment (1 = Post Announcement)	0.066	0.066	0.077	0.077
	(0.124)	(0.125)	(0.124)	(0.128)
Macro	0.079	0.071	0.079	0.084
	(0.147)	(0.140)	(0.139)	(0.141)
Treatment × Macro	-0.236	-0.246	-0.252	-0.258
	(0.193)	(0.191)	(0.189)	(0.195)
Cumulative effect	-0.170	-0.181	-0.175	-0.181
	(0.153)	(0.152)	(0.149)	(0.151)
Observations (# experts)	428	428	428	428
Countries	24	24	24	24
R-Squared	0.353	0.360	0.362	0.364
	Panel C: Long-run growth expectations (year 2026)			
Treatment (1 = Post Announcement)	0.171	0.170	0.192	0.171
	(0.131)	(0.132)	(0.131)	(0.136)
Macro	0.276	0.267	0.292	0.280
	(0.209)	(0.204)	(0.208)	(0.214)
Treatment × Macro	-0.627**	-0.636**	-0.652***	-0.616**
	(0.252)	(0.250)	(0.251)	(0.250)
Cumulative effect	-0.456**	-0.466**	-0.460**	-0.445**
	(0.209)	(0.207)	(0.208)	(0.204)
Observations (# experts)	419	419	419	419
Countries	24	24	24	24
R-Squared	0.280	0.286	0.294	0.298

Notes: The table shows the results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on growth expectations of economic experts. The table presents results for all specifications used in our baseline specifications for inflation expectations of Table (3.1). We report results for short-run (Panel A), medium-run (Panel B), and long-run (Panel C) growth expectations. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.10: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—ACCOUNTING FOR EXPECTATIONS ON THE GENERAL STATE OF THE MACROECONOMY

Dependent variable: Inflation expectations for the year 2023				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.121	0.121	0.145	0.211
	(0.228)	(0.228)	(0.228)	(0.230)
Macro	0.029	0.032	0.042	0.115
	(0.231)	(0.231)	(0.228)	(0.232)
Treatment × Macro	-0.646**	-0.643**	-0.642**	-0.714**
	(0.320)	(0.321)	(0.321)	(0.326)
GDP Expectations 2023	0.237***	0.238***	0.226***	0.211***
	(0.070)	(0.070)	(0.072)	(0.073)
Cumulative effect	-0.525**	-0.522**	-0.497**	-0.503**
	(0.228)	(0.229)	(0.227)	(0.229)
Observations (# experts)	433	433	433	433
Countries	24	24	24	24
R-Squared	0.516	0.516	0.521	0.530

Notes: The table shows the baseline results of our main survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts. The table reports results when we account for general expectations about the macroeconomy. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our main experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.11: FALSIFICATION — RESULTS FOR PRE-EXPERIMENTAL INFLATION EXPECTATIONS

Dependent variable: (Pre-experimental) inflation expectations			
	(I) 2022	(II) 2023	(III) 2026
Treatment (1 = Post Announcement)	0.222	0.186	0.390
	(0.251)	(0.386)	(0.261)
Macro	0.067	0.295	0.046
	(0.268)	(0.405)	(0.266)
Treatment × Macro	-0.350	-0.610	-0.763**
	(0.359)	(0.518)	(0.368)
Cumulative effect	-0.128	-0.424	-0.371
	(0.266)	(0.380)	(0.277)
Observations (# experts)	301	301	298
Countries	24	24	24
R-Squared	0.811	0.449	0.272

Notes: The table shows the result of our second falsification test, reporting the effect of the central banks' announcements in December 2022 on pre-experimental inflation expectations (elicited in Q3 2022) of economic experts.

Table 3.B.12: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—EXCLUDING EXPERTS FROM NON-EMU STATES AND NORTH AMERICA

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
	Panel A: Excluding European experts from non-EMU states			
Treatment (1 = Post Announcement)	0.280	0.276	0.322	0.341
	(0.262)	(0.262)	(0.261)	(0.260)
Macro	0.170	0.171	0.177	0.222
	(0.246)	(0.247)	(0.243)	(0.245)
Treatment × Macro	-0.960***	-0.953***	-0.944***	-0.941***
	(0.357)	(0.358)	(0.352)	(0.354)
Cumulative effect	-0.680***	-0.677***	-0.622**	-0.600**
	(0.246)	(0.247)	(0.243)	(0.245)
Observations (# experts)	383	383	383	383
Countries	21	21	21	21
R-Squared	0.468	0.469	0.482	0.493
	Panel B: Only experts from EMU members			
Treatment (1 = Post Announcement)	0.314	0.310	0.332	0.393
	(0.318)	(0.318)	(0.322)	(0.316)
Macro	0.302	0.302	0.249	0.328
	(0.299)	(0.300)	(0.297)	(0.303)
Treatment × Macro	-1.010**	-1.002**	-0.928**	-1.028**
	(0.431)	(0.432)	(0.427)	(0.431)
Cumulative effect	-0.695**	-0.692**	-0.596**	-0.635**
	(0.299)	(0.300)	(0.298)	(0.306)
Observations (# experts)	304	304	304	304
Countries	19	19	19	19
R-Squared	0.442	0.442	0.454	0.473

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts when excluding European experts from non-EMU states (Panel A) or when only using experts from EMU member states (Panel B). The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.13: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—RESTRICTED SAMPLE, COUNTRIES WITH AT LEAST TEN EXPERTS

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.228	0.228	0.284	0.280
	(0.237)	(0.237)	(0.238)	(0.240)
Macro	0.020	0.021	0.041	0.064
	(0.233)	(0.233)	(0.230)	(0.234)
Treatment × Macro	-0.708**	-0.706**	-0.725**	-0.686**
	(0.328)	(0.328)	(0.325)	(0.329)
Cumulative effect	-0.480**	-0.478**	-0.441**	-0.406*
	(0.226)	(0.227)	(0.220)	(0.223)
Observations (# experts)	402	402	402	402
Countries	16	16	16	16
R-Squared	0.471	0.471	0.485	0.494

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts using only countries with at least ten experts. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.14: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—TRUNCATED INFLATION EXPECTATIONS

Dependent variable: Inflation expectations for the year 2023				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.019	0.016	0.055	0.075
	(0.217)	(0.218)	(0.215)	(0.216)
Macro	0.039	0.040	0.059	0.113
	(0.231)	(0.231)	(0.228)	(0.229)
Treatment × Macro	-0.551*	-0.545*	-0.550*	-0.547*
	(0.306)	(0.307)	(0.303)	(0.305)
Cumulative effect	-0.532**	-0.529**	-0.494**	-0.472**
	(0.219)	(0.220)	(0.216)	(0.216)
Observations (# experts)	434	434	434	434
Countries	24	24	24	24
R-Squared	0.469	0.470	0.480	0.490

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts. The estimates are obtained using data on inflation expectations excluding extreme values (expectations are truncated at 15%). The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.15: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—HUBER ROBUST REGRESSIONS

Dependent variable: Inflation expectations for the year 2023				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.167	0.162	0.143	0.180
	(0.187)	(0.188)	(0.187)	(0.188)
Macro	0.079	0.076	0.084	0.150
	(0.222)	(0.222)	(0.223)	(0.224)
Treatment × Macro	-0.619**	-0.612**	-0.582*	-0.626**
	(0.298)	(0.299)	(0.298)	(0.300)
Cumulative effect	-0.452*	-0.449*	-0.439*	-0.446*
	(0.234)	(0.235)	(0.234)	(0.233)
Observations (# experts)	442	441	441	442
Countries	24	24	24	24
R-Squared	0.619	0.618	0.624	0.632

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts. The estimates are obtained using Huber robust regressions. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.16: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—CLUSTERING AT THE CENTRAL BANK LEVEL

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.173	0.171	0.202	0.247
	(0.174)	(0.173)	(0.177)	(0.171)
Macro	0.001	0.002	0.001	0.068
	(0.275)	(0.276)	(0.238)	(0.241)
Treatment × Macro	-0.794**	-0.788**	-0.770**	-0.810**
	(0.209)	(0.207)	(0.190)	(0.186)
Cumulative effect	-0.620***	-0.617***	-0.568***	-0.563***
	(0.095)	(0.095)	(0.085)	(0.092)
Observations (# experts)	442	442	442	442
Countries	24	24	24	24
R-Squared	0.507	0.507	0.517	0.527

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts when clustering standard errors at the level of countries' central bank. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other.

Table 3.B.17: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—CONTROLLING FOR TENURE EFFECTS

Dependent variable: Inflation expectations for the year 2023

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
Treatment (1 = Post Announcement)	0.178	0.174	0.206	0.251
	(0.240)	(0.241)	(0.239)	(0.238)
Macro	0.051	0.053	0.055	0.121
	(0.235)	(0.235)	(0.233)	(0.236)
Treatment × Macro	-0.835**	-0.829**	-0.813**	-0.852***
	(0.327)	(0.328)	(0.324)	(0.325)
Cumulative effect	-0.657***	-0.654***	-0.607***	-0.601***
	(0.228)	(0.228)	(0.225)	(0.225)
Observations (# experts)	442	442	442	442
Countries	24	24	24	24
R-Squared	0.509	0.510	0.520	0.530

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts controlling for experts' survey tenure. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.18: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—ALTERNATIVE FIXED EFFECTS

Dependent variable: Inflation expectations for the year 2023				
	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
	Panel A: Using Region fixed effects			
Treatment (1 = Post Announcement)	0.129	0.120	0.163	0.244
	(0.287)	(0.288)	(0.287)	(0.287)
Macro	0.169	0.168	0.167	0.305
	(0.306)	(0.307)	(0.303)	(0.301)
Treatment × Macro	-1.011**	-0.997**	-0.966**	-1.044***
	(0.407)	(0.408)	(0.400)	(0.402)
Cumulative effect	-0.882***	-0.877***	-0.803***	-0.800***
	(0.283)	(0.283)	(0.279)	(0.279)
Observations (# experts)	442	442	442	442
Countries	24	24	24	24
R-Squared	0.248	0.249	0.269	0.294
	Panel B: Controlling for inflation in 2022			
Treatment (1 = Post Announcement)	0.141	0.137	0.167	0.236
	(0.270)	(0.270)	(0.269)	(0.270)
Macro	0.183	0.183	0.121	0.211
	(0.269)	(0.270)	(0.267)	(0.269)
Treatment × Macro	-0.947**	-0.941**	-0.904**	-0.977**
	(0.381)	(0.382)	(0.375)	(0.378)
Cumulative effect	-0.806***	-0.804***	-0.737***	-0.741***
	(0.269)	(0.270)	(0.267)	(0.269)
Observations (# experts)	442	442	442	442
Countries	24	24	24	24
R-Squared	0.325	0.325	0.350	0.363

Notes: The table shows the baseline results of our survey experiment, reporting the effect of the central banks' announcements in December 2022 on inflation expectations of economic experts when using region fixed effects (Panel A) or controlling for the official inflation rate in the experts' countries in 2022 (Panel B). The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.19: MONETARY POLICY ANNOUNCEMENT AND INFLATION EXPECTATIONS OF ECONOMIC EXPERTS—RESULTS FOR MEDIUM-TERM AND LONG-TERM EXPECTATIONS

Dependent variable: Inflation expectations for the years 2024 and 2026

	(I) Parsimonious	(II) + Effort	(III) + Biography	(IV) + Affiliation
	Panel A: Medium-term inflation expectations (year 2024)			
Treatment (1 = Post Announcement)	0.121	0.118	0.130	0.129
	(0.226)	(0.226)	(0.225)	(0.225)
Macro	-0.220	-0.219	-0.232	-0.197
	(0.232)	(0.233)	(0.228)	(0.226)
Treatment × Macro	-0.801**	-0.795**	-0.752**	-0.715**
	(0.330)	(0.331)	(0.321)	(0.318)
Cumulative effect	-0.680***	-0.676***	-0.622***	-0.586**
	(0.239)	(0.239)	(0.237)	(0.233)
Observations (# experts)	437	437	437	437
Countries	24	24	24	24
R-Squared	0.327	0.328	0.345	0.361
	Panel B: Long-term inflation expectations (year 2026)			
Treatment (1 = Post Announcement)	0.110	0.108	0.113	0.125
	(0.206)	(0.207)	(0.205)	(0.211)
Macro	-0.448**	-0.447**	-0.437**	-0.405**
	(0.193)	(0.193)	(0.194)	(0.198)
Treatment × Macro	-0.252	-0.246	-0.216	-0.226
	(0.276)	(0.277)	(0.265)	(0.272)
Cumulative effect	-0.142	-0.138	-0.103	-0.101
	(0.177)	(0.178)	(0.178)	(0.176)
Observations (# experts)	433	433	433	433
Countries	24	24	24	24
R-Squared	0.225	0.226	0.256	0.266

Notes: The table shows the results of our main survey experiment, reporting the effect of the central banks' announcements in December 2022 on medium-run (Panel A) and long-run (Panel B) inflation expectations of economic experts. The table presents results from four specifications. We start with a parsimonious model that examines the mean difference in inflation expectations between the two waves of our experiment, conditional on country fixed effects (Column I). We gradually augment this model by introducing the effort of respondents, measured via the time (in seconds) participants used to answer the survey (Column II), biographic characteristics of respondents (Column III), and fixed effects for the affiliations of experts (Column IV). Biographic information includes participants' gender and age, affiliations include academia, central banks, private sector, public sector, research institute, and other. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.20: BALANCE TESTS EXPERTS FOLLOW-UP—SAMPLE MEANS OF CONTROL AND TREATMENT GROUPS AND T-TESTS FOR DIFFERENCES IN MEAN CHARACTERISTICS

(I) **Variable**	(II) **Control (mean)**	(III) **Treatment I (mean)**	(IV) **Treatment II (mean)**	(V) **Difference (t)**
Gender (1 = female)	0.102	0.141	0.155	-0.014 (0.33)
Year born	1967	1964	1967	-2.73 (2.11)
Field of study (1 = Academia)	0.547	0.622	0.594	0.028 (0.48)
Sector (1 = Central Bank)	0.070	0.055	0.084	-0.029 (0.93)
Sector (1 = Research institute)	0.164	0.142	0.110	0.032 (0.81)
Sector (1 = Private Sector)	0.148	0.118	0.116	0.002 (0.05)
Sector (1 = Public Sector)	0.062	0.055	0.071	-0.016 (0.54)
Time used for survey (in seconds)	566	4909	19721	-14812 (1.54)
Google scholar (# citations)	4872	3108	2754	353 (0.45)

Notes: The table reports the mean levels of key biographic, geographic, and bibliographic characteristics of participants included in our follow-up experiment sample for the control group (Column II) and the two treatment groups (Column III and IV). The differences between the means of the treatment groups are reported in Column V, with test statistics of a two-sample t-test reported in parentheses.

Table 3.B.21: FOLLOW-UP EXPERIMENT—TABLE RESULTS

Dependent variable: Inflation expectations			
	(I) 2023	(II) 2024	(III) 2026
	Panel A: Tone Treatment		
Treatment arm 1: Non-macro	-0.361	-0.137	0.122
	(0.275)	(0.251)	(0.254)
Treatment arm 1: Macro	-0.172	-0.312	0.012
	(0.310)	(0.298)	(0.247)
	Panel B: Tone + Forward-Treatment		
Treatment arm 2: Non-macro	-0.517**	-0.275	0.039
	(0.260)	(0.281)	(0.284)
Treatment arm 2: Macro	-0.026	-0.050	0.248
	(0.258)	(0.288)	(0.315)
Observations (# experts)	409	404	395
Countries	22	22	22
R-Squared	0.67	0.51	0.37

Notes: The table shows the results of our follow-up survey experiment, reporting the effects of the two treatment arms (relative to the control group) on inflation expectations of economic experts. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.22: BALANCE TESTS HOUSEHOLD SURVEY—SAMPLE MEANS OF CONTROL AND TREATMENT GROUPS AND T-TESTS FOR DIFFERENCES IN MEAN CHARACTERISTICS

(I) **Variable**	(II) **Control (mean)**	(III) **Treatment I (mean)**	(IV) **Treatment II (mean)**	(V) **Treatment III (mean)**	(VI) **Difference (t)**
Gender (1 = male)	0.490	0.503	0.538	0.503	0.024 (0.75)
Age	45.799	45.391	45.604	45.088	-0.439 (0.47)
East-Germany	0.153	0.151	0.160	0.148	0.000 (0.00)
Household size	2.465	2.490	2.528	2.610	0.078 (0.93)
Migration background	0.086	0.112	0.097	0.079	0.010 (0.53)
High income	0.296	0.333	0.299	0.324	0.022 (0.74)
High education	0.382	0.462	0.447	0.453	0.071 (2.21)
Time used for survey (in seconds)	1701	1720	1964	1109	-104 (0.13)

Notes: The table reports the mean levels of key biographic and geographic characteristics of participants included in our first household experiment sample for the control group (Column II) and the three treatment groups (Columns III, IV, and V). The differences between the means of the control group and all treatment groups are reported in Column (VI), with test statistics of a two-sample t-test reported in parentheses.

Table 3.B.23: EXPERTS' NARRATIVES AND HOUSEHOLDS' INFLATION EXPECTATIONS — ROBUSTNESS

	(I) Post. inflation exp.	(II) Qualitative post. (z-scored)	(III) Uncertainty
	Panel A: Huber Regressions		
Interest hike (1 = Treatment 1)	-0.134	0.013	-0.047
	(0.174)	(0.080)	(0.077)
Expert Narrative (1 = Treatment 2)	-0.312*	0.150*	0.021
	(0.174)	(0.080)	(0.077)
Expert Narrative & point estimate	-0.693***	0.293***	0.020
(1 = Treatment 3)	(0.175)	(0.081)	(0.078)
Observations	1,260	1,260	1,260
R-Squared	0.130	0.133	0.178
Day Fixed Effects	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes
Controls	Yes	Yes	Yes
	Panel B: Trimmed (prior) expectations (<=30%)		
Interest hike (1 = Treatment 1)	-0.160	0.013	-0.073
	(0.179)	(0.078)	(0.078)
Expert Narrative (1 = Treatment 2)	-0.279*	0.142*	0.071
	(0.167)	(0.076)	(0.079)
Expert Narrative & point estimate	-0.578***	0.266***	-0.014
(1 = Treatment 3)	(0.181)	(0.082)	(0.080)
Observations	1,166	1,166	1,166
R-Squared	0.204	0.132	0.115
Day Fixed Effects	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The table shows treatment effects of our three treatment arms on future inflation expectations. Panel A presents results using Huber regressions to control for influential outliers. Panel B presents results using a trimmed sample, where we exclude respondents with prior inflation expectations above 30%. The dependent variable in Column 1 is the mean of respondents' subjective probability distribution of the inflation rate in 2023. The dependent variable in Column 2 is the z-scored agreement with the following statement 'The inflation rate in 2023 will be lower than in 2022'. The dependent variable in Column 3 is the standard deviation of respondents' subjective probability distribution of the inflation rate in 2023. All regressions include the following set of control variables: gender, age, household income, prior inflation expectations, household size, education levels, migration background, region, trust level in experts, survey time, and day fixed effects. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.24: INFLATION EXPECTATIONS AND SPENDING — INSTRUMENTAL VARIABLE RESULTS WHEN CONTROLLING FOR SECOND-ORDER MOMENTS

	(1) Durable goods (z-scored)	(2) Restaurant (z-scored)	(3) Supermarket (z-scored)	(4) Leisure (z-scored)
	Panel A: First stage			
Treatment (1 = Narrative & point estimate)	-0.681***	-0.681***	-0.681***	-0.681***
	(0.159)	(0.159)	(0.159)	(0.159)
	Panel B: Second stage			
Inflation expectations	-0.124	-0.306**	-0.027	-0.229**
	(0.106)	(0.118)	(0.100)	(0.108)
F-value	18.40	18.40	18.40	18.40
Observations	943	943	943	943
Day Fixed Effects	Yes	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: The table shows the results of our household survey experiment, reporting the effect of the 'Expert narrative & point estimate' treatment arm on inflation expectations (Panel A), and the subsequent effects on households' spending intentions (Panel B) when controlling for the second-order moment of households' inflation expectations. Column 1 reports the effect of reduced inflation expectations on the z-scored agreement to the question, whether respondents think that 'now is a good or bad time to buy durable goods such as cars, furniture or household appliances?'. Columns 2 to 4 report the effect of reduced inflation expectations on z-scored answers to the question whether respondents 'intend to spend more, less or about the same on [item] in the next 4 weeks? Compare your intentions with what you have spent in the last 4 weeks.' All regressions include the following set of control variables: gender, age, household income, prior inflation expectations, household size, education levels, migration background, region, trust level in experts, survey time, and day fixed effects. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

Table 3.B.25: INFLATION EXPECTATIONS AND SPENDING — INSTRUMENTAL VARIABLE RESULTS WITH MORE RESTRICTIVE COMPARISON

	(1) Durable goods (z-scored)	(2) Restaurant (z-scored)	(3) Supermarket (z-scored)	(4) Leisure (z-scored)
	Panel A: First stage			
Treatment (1 = Narrative & point estimate)	-0.747***	-0.747***	-0.747***	-0.747***
	(0.189)	(0.189)	(0.189)	(0.189)
	Panel B: Second stage			
Inflation expectations	-0.162	-0.268**	-0.073	-0.220**
	(0.111)	(0.116)	(0.106)	(0.108)
F-value	15.56	15.56	15.56	15.56
Observations	625	625	625	625
Day Fixed Effects	Yes	Yes	Yes	Yes
Survey Time	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: The table shows the results of our household survey experiment, reporting the effect of the 'Expert narrative & point estimate' treatment arm on inflation expectations (Panel A), and the subsequent effects on households' spending intentions (Panel B). Column 1 reports the effect of reduced inflation expectations on the z-scored agreement to the question, whether respondents think that 'now is a good or bad time to buy durable goods such as cars, furniture or household appliances?'. Columns 2 to 4 report the effect of reduced inflation expectations on z-scored answers to the question whether respondents 'intend to spend more, less or about the same on [item] in the next 4 weeks? Compare your intentions with what you have spent in the last 4 weeks.' All regressions include the following set of control variables: gender, age, household income, prior inflation expectations, household size, education levels, migration background, region, trust level in experts, survey time, and day fixed effects. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses.

3.C Survey Structure

This section describes the structure of our expert and household surveys in greater detail. The survey instructions of each survey are listed as Supplementary Material E.

C.I Expert surveys

Survey I: Prior expectations

The first survey (EES wave Q3 2022) elicits prior inflation expectations of participants before the main experiment. In a typical experiment with households, we would elicit priors and posteriors in the same survey. When working with experts who have strong economic and methodological training, eliciting priors and posteriors within the same survey risks revealing the experimental nature of the study, potentially leading to experimenter demand effects. Measuring prior inflation expectations also enables us to run a falsification test of our main experimental results.

The question on inflation is a regular part of the EES and the survey did not include any additional information for respondents, who all received the same questionnaire. The wording of the inflation questions is identical to the questions on inflation we pose in our main experiment one quarter later.

Survey II: Main experiment

The second survey (EES wave Q4 2022) includes our main experiment. We distribute the second survey in two waves. We randomly assign the universe of North American and European experts that regularly participate in the EES to a control and a treatment group. The first wave of experts was surveyed shortly before the announcement of the policy rate change of the ECB, the Fed, the Bank of England, the Norwegian Norges Bank, and the Swiss National Bank. The second wave of experts was surveyed directly after the interest rate changes were announced. Such a two-wave survey design offers the advantage that individuals cannot sort into treatment. Comparing mean expectations between groups conditional on fixed effects for countries and individual-level observable characteristics delivers our main treatment effects.

Participants in Europe assigned to the control group were polled between December 13, 2022, 7:00 a.m. (CET), and December 15, 2022, 14:59 p.m. (CET). Participants in Northern America, where the monetary policy announcement took place one day earlier, were assigned to the control group between December 12, 2022, 1:00 p.m.

(CET), and December 14, 2022, 07:59 p.m. (CET). The control group consists of $N = 200$ experts (163 in Europe, 37 in Northern America).

The treatment group was surveyed in wave 2, immediately after the press conferences at which the change in monetary policy was announced. We started surveying participants for the second wave at 9 p.m. on December 14, 2022 (CET) for the Northern American experts, and at 3 p.m. on December 15, 2022 (CET) for the European experts. The treatment group consists of $N = 242$ experts (200 in Europe, 42 in Northern America).

Randomization. In our baseline experiment, randomization was achieved using a software-based random assignment of the experts in our database to the two waves of our survey. Given this process, participants in our experiment are roughly equally split into a treatment and a control group. We distributed our survey online via the software *qualtrics*, the most often used software for survey experiments (Fuster and Zafar, 2023).

Obfuscation. All participants in our experiment were blind to the goal of our analysis. Our experimental setup included several elements of obfuscation. The EES is regularly conducted on a quarterly basis. In each wave of the EES, participants are asked about their evaluation of national economic policies. Since Q2 of 2022, the EES also regularly includes questions on participants' inflation expectations. The questions on inflation expectations included in our main survey experiment are identical to the regularly posed questions in the waves before our main experiment. This setting eliminates concerns about experimenter demand, which would typically be present when participants anticipate that they are part of an experiment.

Questions. Our survey includes three blocks of questions, eliciting participants' inflation expectations, beliefs about the causes of inflation, and expectations about the general state of the macroeconomy (see Supplementary Material E for details and instructions):

(1) The first set of questions elicits respondents' short-term, medium-term, and long-term inflation expectations.

(2) In the second part of the survey, participants are asked to describe their perceived causes of inflation. Participants are asked to give a brief description of the causes of inflation in a free-text entry box.

(3) To distinguish monetary components from real economic outcomes, our survey also asks respondents about their expectations regarding GDP growth in 2023, 2024, and 2026. In line with the questions for inflation rates, we complement the question on growth expectations with open-ended text questions about the primary drivers of GDP growth.

Survey III: Follow-up experiment

We run a follow-up survey after our main experiment to study two questions. First, we wanted to know whether experts who have been *inattentive* in the main experiment react similarly to the attentive experts in our main experiment when being provided with the information conveyed in the December 2022 monetary policy announcement by the ECB. Second, we also wanted to quantify the relative importance of the hawkish tone (*tone treatment*) and the path surprise component (*forward guidance treatment*).

We survey experts from the same countries that are included in the baseline sample of our main experiment and randomly allocate experts in a control group and two treatment arms. We provide a first treatment group with information that re-activates the hawkish tone of the central banks' communication using a key quote from the ECB's governing council statement on December 15. For a second treatment group, we supplement the information about the tone with the forward guidance provided by the ECB (in form of a quote). We conduct this experiment in a follow-up to measure the effects of these components absent of the monetary policy uncertainty resolved in our main experiment.

Treatments and control group. In the control group ($N = 127$), we elicit inflation expectations for 2023, 2024, and 2026 of all experts in the control group without additional information. Treatment arm 1 includes the *tone treatment* ($N = 128$). Experts in the first treatment arm received input that highlighted the tone of the original interest rate announcement: "*Inflation remains an important topic in Europe. For instance, in a statement ahead of the December 2022 press conference, the ECB governing council announced that 'inflation remains far too high'*". Treatment arm 2 tests the *forward guidance treatment* ($N = 154$). Experts in the second treatment arm received input that highlighted the *tone* and the *path surprise component* in the forward guidance of the original interest rate announcement: "*Inflation remains an important topic in Europe. For instance, in a statement ahead of the December 2022 press conference, the ECB governing council announced that 'inflation remains far too high' and that interest rates would continue to rise by 0.5 percentage points 'for a period*

of time'." The second treatment hence allows to quantify the *additional* effect of the forward guidance (beyond the effect of the tone).

Obfuscation. Our follow-up survey also incorporated some elements of obfuscation. Before we elicit experts' inflation expectations, we asked all participants whether they *'think that the measures taken by the ECB are enough to tackle inflation?'*. By doing so, we obscure the direct relationship between the provided information included in our treatment arms and the elicitation of inflation expectations.

C.II Household survey

Information provision experiment

Socioeconomic characteristics. Our survey consists of 1,260 German households that we recruited in September 2023 via Respondi. We start the survey by asking respondents about their socioeconomic characteristics. Using those answers, we demonstrate that the sample is representative of the German population, and that observable characteristics are well balanced across treatment arms. At the end of the survey, we additionally ask for households' spending intentions, media consumption, and trust in experts and journalists.

Attention check. All participants had to pass an attention check that requires respondents to promise to answer truthfully.

Prior beliefs. We elicit respondents' prior beliefs on the inflation rate in 2023 (via a point estimate). We also ask them about how certain they are in their prediction and similarly elicit respondents' beliefs about future monetary policy.

Treatments and control group. Respondents in our experiment are allocated randomly into an active control group and three treatment arms. Participants had to actively confirm that they read and understood the respective information.

Treatment 1—Interest hike ($N = 318$): The first treatment intervention informs individuals that the European Central Bank has raised interest rates one week before. This treatment arm serves as a benchmark to assess whether individuals are able to link the objective information about an increase in interest rates to inflation expectations. According to the results for households obtained in our natural updating setting (see

Section 3.3.5), we expect no effect from this treatment arm, serving as a benchmark for the two other treatments that involve expert explanations.

Treatment 2—Expert narrative ($N = 317$): The second treatment intervention provides an expert narrative. We inform participants that we recently run a recent survey among leading experts and that these experts told us that an increase in interest rates, as implemented by the European Central Bank last week, lowers the inflation rate as it reduces the demand for goods. This treatment condition allows us to inspect the causal effect of expert explanations, which decode otherwise abstract monetary policy measures, on inflation expectations.

Treatment 3—Expert narrative & point estimate ($N = 311$): The third treatment arm combines the expert narrative with a point estimate, for which we inform participants that leading economists expect inflation rates to be 1.2 percentage points lower following an interest rate hike by the central bank (similar to last week's decision by the ECB). This intervention enables us to examine the combined effect of narratives and numerical point estimates and to compare it to the impact of a standalone narrative (Treatment 2).

Control group ($N = 314$): The control group receives the (irrelevant) information that the population has grown again recently.

Posterior beliefs. We elicit households' posterior beliefs both qualitatively and quantitatively. Respondents are asked first whether they agree that '*the inflation rate in 2023 will be lower than in 2022*' (on a scale from 'completely disagree' to 'completely agree'). For the quantitative posterior, we use a distributional question, where respondents assign probabilities to different bins of potential future inflation rates. The different question-type helps to limit experimenter demand effects and additionally allows to calculate the standard deviation of households' inflation rate expectations to assess whether the treatment increased the uncertainty of expectations. Similarly, we elicit households' posterior beliefs about the future of monetary policy.

3.D Extensions of our main Expert Experiment

We present extensions and robustness tests to examine the sensitivity of our main results to changes in the empirical strategy. To economize space in the main paper, we present details on these results in this appendix.

Within day results

Our survey design is based on two control days, one treatment day, and up to seven post-treatment days. A concern may be that the results are driven by other events that took place during our survey period. We conduct two empirical exercises to examine this threat.

First, we examine how treatment effects change once we gradually restrict the included post-treatment survey days. Figure (3.A.4) shows that the treatment effects are completely stable when we restrict the post-treatment survey period.

Second, we leverage within treatment-day variation to fully rule out that confounding events are driving the results. Specifically, we restrict the sample to the day of the central banks' announcements. We therefore compare respondents who filled out the survey on the same day, but either before or after the central banks' decisions. The sample consists of 148 experts, out of which 88 are in the treatment group and 60 are in the control group.

The results show again that attentive experts react strongly to the new monetary signal, which is in contrast to the behavior of non-attentive experts. Figure (3.A.5) visualizes the treatment effect for attentive experts when estimating our baseline specification with country fixed effects (Panel A), and when we, due to the smaller sample, instead control for the official inflation rate in 2022 (Panel B). Exploiting within-day variation, the results point to even larger treatment effects, especially for Europe (up to a 1.2 percentage point reduction in mean expectations) – consistent with the notion that the tone in the ECB's statement and the provided forward guidance have been especially important. It is therefore highly unlikely that confounding events, or anticipation, are driving our results.

Treatment heterogeneity

In our baseline model specification, we interact the treatment indicator with a dummy whether the experts primarily work on topics relating to macroeconomics and mone-

tary policy. The key argument is that attentiveness to monetary policy signals varies between non-macroeconomists and macroeconomists. The experts in our sample differ, however, along a number of other observable characteristics. We examine whether there is heterogeneity in the treatment effects across these characteristics, by interacting the treatment indicator of our benchmark model specification of equation (3.1) with individual-level characteristics of respondents:

$$\pi_{ei}^{\tau} = \gamma^{\tau} T_e + \omega C_e + \lambda (T_e \times C_e) + \mathbf{A}_i \boldsymbol{\lambda} + \mathbf{X}_e \boldsymbol{\mu} + \varepsilon_{ei}^{\tau}, \tag{3.6}$$

where C_e denotes socio-demographic and academic characteristics of participants. Table (3.B.6) reports the cumulative treatment effects, along with p-values on a Wald test inspecting whether the baseline parameter estimate and the coefficients obtained in our heterogeneity exercise are statistically distinguishable. The results show that none of the cumulative treatment effects turn out to be statistically significant. We find no treatment heterogeneity regarding gender, age, affiliation, citations or the time experts spend to answer our survey. We also find that the treatment effect does not vary with prior inflation expectations. In addition, the Wald test in most cases clearly rejects the null of equality between the estimated parameter on the treatment status and the baseline treatment effect reported in Table (3.1). We hence conclude that the treatment effects for macroeconomists occur due to different attentiveness and not due to differences in other observable characteristics.

Temporal structure of the treatment effects

Our benchmark estimates present treatment effects for short-term inflation expectations for the year 2023. We now examine the temporal structure of the treatment by studying medium-term (for the year 2024) and long-term (for the year 2026) inflation expectations. Table (3.B.19) reports re-estimates of the benchmark specifications for medium-term and long-term inflation expectations.

The results show that the announcement of the monetary policy change also decreased medium-term inflation expectations by a similar magnitude (-0.6 percentage points). The announcement of the monetary policy change, however, had no effect on long-term inflation expectations of economic experts. The differences in means between the control and the treatment group of our survey experiment are close to zero in all model specifications for 2026. The results suggest that the central bank announcements only affected short- and medium-term inflation expectations, but had no impact on the formation of expectations for longer periods. This finding is plausible, given

that the primary purpose of the policy change was to bring down inflation in the short run. Against the backdrop of recent work showing that short-term expectations matter most for actual inflation (Werning, 2022), the temporal structure of our results imply that monetary policy can have important effects on the macroeconomy.

Expectations about the general macroeconomy

In addition to eliciting inflation expectations, we also ask respondents for their projections on economic growth in their countries. Eliciting expectations about the general macroeconomy allows us to account for real-economic components of participants' inflation expectations. These broader macroeconomic expectations allow us to run complementary analyses that deliver two additional pieces of evidence: First, relating treatment status to expectations about economic growth, we measure whether the announcement specifically adjusted inflation expectations, or whether it also impacted the more general beliefs about future macroeconomic developments. Second, conditioning our benchmark specifications on expectations about the future state of the macroeconomy allows us to disentangle the treatment effects from real-economic components and tells us whether the results are driven by more favorable expectations about the general economic environment.

Table (3.B.9) in the appendix presents the results when regressing expectations about GDP growth for 2023 on treatment status, using specifications identical to our baseline estimates for inflation expectations of Table (3.1). Consistent with the findings for inflation expectations, we find that no treatment effects for non-macroeconomists. Experts with a macroeconomics-background, however, update their short-run expectations about the general macroeconomy in response to the monetary policy change, and are also somewhat more pessimistic about long-run growth until 2026 after the interest rate increase.

Our main results regarding inflation expectations persist when we separate the effect of the monetary policy treatment from general macroeconomic expectations (see Table 3.B.10). The results show that higher growth expectations are significantly associated with higher inflation expectations, both in economic and in statistical terms. The close entanglement between growth expectations and inflation expectations underscores that real-economic components matter for the formation of expectations about inflation. Importantly, the effects of the monetary policy announcement are qualitatively identical to the baseline results. The estimated parameters point to a slightly lower treatment effect per model specification, but we find based on Wald tests that the estimated effects are statistically indistinguishable from those obtained by our baseline models.

Construction of forecast errors

To assess whether experts in the treatment group exhibit lower forecasting errors, we calculate the following measure for every expert:

$$\text{error}_{ei} = \left| \frac{\pi_{ei}^{\text{exp.}} - \pi_i^{2023}}{\pi_i^{2023}} \right| \times 100, \tag{3.7}$$

where we take experts' inflation expectations for the year 2023 and subtract and divide by the realized inflation rate in 2023. We use the same formula throughout the paper to calculate forecast errors.

Re-running our baseline specification using the forecasting error as the dependent variable, we find that expectations of attentive experts in the treatment group were significantly more accurate. Attentive experts in the treatment group exhibit 7.5 percentage points lower forecast errors ($t = 2.04$). When looking at *changes* in the forecast errors (vis-à-vis forecast errors of prior expectations), forecast errors are even smaller (10.8 percentage points lower forecast errors; $t = 1.92$).

3.E Survey Instructions

E.I Main Expert Experiment (EES Q4 2022)

1.1 Start page

Dear {e://Field/RecipientFirstName} {e://Field/RecipientLastName},

The ifo Institute and IWP invite you to contribute your expertise to the **Economic Experts Survey (EES)**. The EES tracks the quality of economic policy and political performance as assessed by economic experts worldwide. The survey runs quarterly and contains four core questions. If you like to elaborate on your answers, please use the text boxes provided underneath each question. After completing the survey, you will be automatically redirected to the **latest survey report for Q3 2022**.

Completing the survey takes **less than three minutes**. Your responses will be handled confidentially in accordance with the highest data protection standards.

You have been identified as an expert for {e://Field/Article} . Please confirm or choose another country.

{e://Field/country_new}	Other country

1.2 EES core questions

1. How do you rate {e://Field/Article} {e://Field/ChosenCountry} current economic policy?

Worse -100 -80 -60 -40 -20 0 20 40 60 80 Better 100

Please compare to the last quarter.

What are your main considerations? (optional)

2. How well does {e://Field/Article} {e://Field/ChosenCountry}{e://Field/Genitiv} current economic policy address the challenges of the future?

Worse -100 -80 -60 -40 -20 0 20 40 60 80 Better 100

Please compare to the last quarter.

What are your main considerations? (optional)

3. How do you rate the performance of {e://Field/Article} {e://Field/ChosenCountry} {e://Field/Genitiv} current government?

Worse -100 -80 -60 -40 -20 0 20 40 60 80 Better 100

Please compare to the last quarter.

What are your main considerations? (optional)

4. How do you rate the stability of {e://Field/Article} {e://Field/ChosenCountry} {e://Field/Genitiv} current political situation?

Less stable
-100 -80 -60 -40 -20 0 20 40 60 80 100 More stable

Please compare to the last quarter.

What are your main considerations? (optional)

1.3 Macroeconomic expectations

1. Which average inflation rate (in %) do you expect for {e://Field/Article} {e://Field/ChosenCountry}

in the year 2023?
in the year 2024?
in the year 2026?

In your opinion, what are the current reasons for inflation in {e://Field/Article} {e://Field/ChosenCountry}?

2. Which average GDP growth rate (in %) do you expect for {e://Field/Article} {e://Field/ChosenCountry}

in the year 2023? ☐
in the year 2024? ☐
in the year 2026? ☐

In your opinion, what are the current drivers of GDP growth in {e://Field/Article} {e://Field/ChosenCountry}?

☐

1.3 Socioeconomic characteristics

1. Which gender describes you best?

male

female

prefer to self-describe

☐

2. In which year were you born?

☐

3. In which country were you born?

☐

4. In which field(s) of economics are you active?
Please choose up to three fields (JEL Codes).

(A) General Economics and Teaching	(F) International Economics	(K) Law and Economics	(P) Economic Systems
(B) History of Economic Thought, Methodology, and Heterodox Approaches	(G) Financial Economics	(L) Industrial Organization	(Q) Agricultural and Natural Resource Economics • Environmental and Ecological Economics
(C) Mathematical and Quantitative Methods	(H) Public Economics	(M) Business Administration and Business Economics • Marketing • Accounting • Personnel Economics	(R) Urban, Rural, Regional, Real Estate, and Transportation Economics
(D) Microeconomics	(I) Health, Education, and Welfare	(N) Economic History	(Y) Miscellaneous Categories
(E) Macroeconomics and Monetary Economics	(J) Labor and Demographic Economics	(O) Economic Development, Innovation, Technological Change, and Growth	(Z) Other Special Topics

5. In which sector are you most active?

- Academia
- Research institute / Think tank
- Public Sector
- Central Bank
- Private Sector

E.II. Follow-up Expert Experiment (EES Q1 2023)

2.1 Start page

Identical to main experiment.

2.2 EES core questions

Identical to main experiment.

2.3 Treatments and Inflation Expectations

Treatment T1

Inflation remains an important topic in Europe. For instance, in a statement ahead of the December 2022 press conference, the ECB governing council announced that "inflation remains far too high".

Treatment T2

Inflation remains an important topic in Europe. For instance, in a statement ahead of the December 2022 press conference, the ECB governing council announced that "inflation remains far too high" and that interest rates would continue to rise by 0.5 percentage points "for a period of time".

Inflation Expectations

1. Do you think that the measures taken by the ECB are enough to tackle inflation?

Yes

No

Don't know

2. Which average inflation rate (in %) do you expect for {e://Field/Article} {e://Field/ChosenCountry}

in the year 2023? ☐
in the year 2024? ☐
in the year 2026? ☐

In your opinion, what are the current drivers of inflation in {e://Field/Article} {e://Field/ChosenCountry}?

☐

2.4 Socioeconomic characteristics

Identical to main experiment.

E.III Household Information Provision Experiment (translated)

3.1 Socioeconomic characteristics

Which gender would you rather identify with?

Male	Female

In what year were you born?

In which federal state do you live?

What is your highest completed professional qualification or your highest completed university degree?

No general school leaving certificate (yet), still a student in general education school	Secondary (elementary, primary) school leaving certificate without completed apprenticeship/vocational training	Secondary (elementary, primary) school leaving certificate with completed apprenticeship/vocational training
Secondary school without Abitur (Realschulabschluss/Mittlere Reife/Oberschule) or equivalent qualification	Abitur, (technical) university entrance qualification without university studies	Studies (university, college, university of applied sciences, polytechnic)

How many people live in your household?

Do you have a migration background?

Yes	No

Which title best describes your professional status?

Full-time employee	Part-time employee	Self-employed	Unemployed	Retired	Student	Other

What was your monthly net household income in 2021, i.e. the income after taxes and deductions for all household members?

Please include regular payments such as pensions, housing benefit, child benefit, BAföG, maintenance payments, etc.! If you don't know the exact amount: Please estimate the monthly amount.

3.2 Attention Check

We care about the quality of our survey data. To get the most accurate picture of your opinion, it is very important that you carefully answer each question in this survey.

Do you promise to answer the questions in this survey carefully?

- I can't promise.
- Yes, I will answer carefully.
- No, I will not answer carefully.

3.3 Prior Beliefs

What do you think the inflation rate will be in Germany in 2023?

[ ] %

How certain are you that the inflation rate you expect will occur?

Very uncertain	Rather uncertain	Rather certain	Very certain

In the following question, we will ask you to think about the percentage probability that something will happen in the future. Your answers can range from 0 to 100, where 0 means it is extremely unlikely and 100 means it is extremely certain.

Examples:
- 2 or 5 percent can mean "quite unlikely".
- 18 percent, for example, can indicate a "low probability".
- 47 or 52 percent can mean a "50:50 chance".
- 83 percent, for example, can mean a "very high probability".
- 95 or 98 percent can mean "almost certain".

How likely do you think it is that the European Central Bank will pursue an expansionary monetary policy in the coming months?
An expansionary monetary policy means that more money is created and thus interest rates are lowered.

Unlikely — Very certain
0 10 20 30 40 50 60 70 80 90 100

3.4 Treatments

Treatment T1: Interest rate hike

We would now like to give you some information about the monetary policy of the European Central Bank. We would like to ask you to take a moment to read the information carefully.

Note: The information will only be shown to you once and you will not be able to go back.

The European Central Bank raised interest rates last week.

I have read and understood the information ○

Treatment T2: Narrative treatment

We would now like to provide you with information on expert assessments of the European Central Bank's monetary policy. We would like to ask you to take a moment and read the information carefully.

Note: The information will only be shown to you once and you will not be able to go back.

In a recent survey, we asked leading economic experts about the impact of monetary policy. These experts told us that raising interest rates, as the European Central Bank did last week, lowers the inflation rate because it reduces demand for goods.

I have read and understood the information ○

Treatment T3: Narrative & point estimate

We would now like to give you some information about the monetary policy of the European Central Bank. We would like to ask you to take a moment to read the information carefully.

Note: The information will only be shown to you once and you will not be able to go back.

In a recent survey, we asked leading economic experts about the impact of monetary policy. These experts told us that following an interest rate hike by the European Central Bank, similar to last week's decision by the European Central Bank, their inflation rate expectations are now 1.2 percentage points lower.

I have read and understood the information ○

Control Group

We would now like to give you some information about population development in Germany. We would like to ask you to take a moment and read the information carefully.

Note: The information will only be shown to you once and you will not be able to go back.

The population in Germany has recently grown again.

I have read and understood the information ○

3.5 Posterior Beliefs

To what extent do you agree with the following statement:

The inflation rate in Germany will be lower in 2023 than the inflation rate in 2022.

Completely disagree	Rather disagree	I neither agree nor disagree	Rather agree	Completely agree

In this question we present you **eleven possible scenarios** .

Please tell us **how likely** you think **each scenario is to occur**. Please enter the number that indicates the percentage probability you give to each scenario.

The probabilities of the eleven scenarios must add **up to 100 percent**.

The inflation rate in Germany will be in 2023...

Scenario 1: ... between 0 and 1 percent.	0
Scenario 2: ... between 1 and 2 percent.	0
Scenario 3: ... between 2 and 3 percent.	0
Scenario 4; ... between 3 and 4 percent.	0
Scenario 5: ... between 4 and 5 percent.	0
Scenario 6: ... between 5 and 6 percent.	0
Scenario 7: ... between 6 and 7 percent.	0
Scenario 8: ... between 7 and 8 percent.	0
Scenario 9: ... between 8 and 9 percent.	0
Scenario 10: ... between 9 and 10 percent.	0
Scenario 11: ... exceed 10 percent	0
Total	0

In this question we present you with **three possible scenarios** .

Please tell us **how likely** you think **each scenario is to occur** . Please enter the number that indicates the percentage probability you give to each scenario.

The probabilities of the three scenarios must add **up to 100 percent** .

The monetary policy of the European Central Bank in the coming months will...

Scenario 1: ... be restrictive: Less money is put into the market to raise interest rates, which lowers inflation.	0
Scenario 2: ... be unchanged.	0
Scenario 3: ... be expansionary: More money is put into the market to lower interest rates, which increases inflation.	0
Total	0

Do you think now is a good or bad time to buy durable goods such as cars, furniture, or household appliances?

Very bad timing	Rather bad timing	Neither good nor bad timing	Rather good timing	Very good timing

Do you intend to spend more, less, or about the same on the following things **in the next 4 weeks**? Compare your intentions with what you spent in the last 4 weeks...

	Spend much less	Spend a little less	Spend about the same amount	Spend a little more	Spend much more
for restaurant visits	○	○	○	○	○
for supermarket shopping	○	○	○	○	○
for leisure activities such as going to the cinema or sporting events	○	○	○	○	○

3.6 Final information

How often do you get information from the media (newspapers, radio, TV, social media) about the following topics?

	Never	Once a month	Once a week	Several times a week	Daily
Politics	○	○	○	○	○
News about the economy	○	○	○	○	○
Inflation	○	○	○	○	○

How much do you trust assessments from...

	not at all	rather less	neutral	rather	very
... experts?	○	○	○	○	○
... journalists?	○	○	○	○	○

Chapter 4

Intellectual Reparations: Mapping a Large-Scale Program of Knowledge Transfers

This chapter is based on co-authored work with Sebastian Hager and Marcel Schlepper (see Hager et al., 2024). I am grateful for financial support from the Dr. Hans Riegel Foundation.

4.1 Introduction

The Second World War (WWII) was a time of rapid technological innovation (e.g., Gross and Sampat, 2023). The advancement in technology did not stop with the war's end. Beginning in the immediate post-war period, the United States and the Soviet Union competed for global leadership in military and industrial technology. Allied post-war reparation programs explicitly targeting German science and technological know-how became a key tool in this race. Most famously, as part of the U.S. 'Operation Paperclip' and the Soviet 'Operation Osoaviakhim' thousands of German scientists were taken to work abroad on long-range missiles, aeronautics, and space programs (see e.g., Uhl, 2001; Jacobsen, 2014). But these were not the only programs of 'intellectual reparations' that focused on German science and technology in the immediate post-war era.

A lesser-known program under the labels of BIOS, CIOS, and FIAT directly targeted Germany's industry – collecting detailed information on the know-how of individual firms and inventors. With the landing of the Western Allies in France in 1944, British and U.S. military forces immediately started to gather intelligence on German technology that could be useful for winning WWII. Soon, the Western Allies expanded their investigations to non-military technology. Between 1945 and 1947, U.S. and British investigators visited firms and production sites across Germany to gather any information that could be useful for private industries. One Washington official has called it 'the first orderly exploitation of an entire country's brainpower' (Walker, 1946).

In this paper, we provide the first systematic and quantitative analysis of this unprecedented program of intellectual reparations. Based on a hand-collected dataset on nearly all investigations of German firms, we present a series of findings on the extent of this program. We present facts on which German firms, industries, and technologies were investigated. Moreover, by linking the investigated German firms to the investigating U.S. firms, we can directly trace and analyse the resulting knowledge transfers.

We assemble a novel database on nearly all British and U.S. investigations of firms in Germany between 1945 and 1947. Based on archival sources, we hand-collect and digitise more than 90% of the reports written by British and U.S. investigating teams. These reports contain the findings of the investigations and explicitly list the set of investigated German firms. In total, the database contains 3,873 investigation reports and more than 20,000 firm investigations.

We present findings on three main aspects of the investigations. First, we study which firms in Germany were investigated. The reports detail the names of the investigated German firms, their locations, and which individuals were interviewed during the investigations. This allows us to map out the extent of the investigations. We show that firms were visited across Germany, with industrial centers being targeted the most. The British and U.S. investigators focused in particular on investigations in their own occupation zones.

Second, we analyse which technologies and industries were targeted by the investigations. We link the reports database to archival documents containing an industry categorisation. This allows us to consistently group reports into major industries. We find that the mechanical engineering and chemical industries were investigated most often. We also document that military-related industries were more strongly investigated in the early phase of the program. After WWII the focus shifted to industries with civilian applications whose know-how would be of more use for private industries in the United Kingdom and the United States.

Third, we observe which U.S. firms were involved in the investigations. Based on a list of U.S. personnel who were sent to Germany, we observe the employers of U.S. investigators. We link this information to our main database and thus observe which U.S. firms sent investigators to which German firms. Hence, we can directly measure knowledge transfers between German and U.S. firms. In further work, this can be used to examine the effects of access to German technological know-how on U.S. innovation and industrial performance.

These findings contribute to two literatures. First, we contribute to a broad literature on industrial policy in post-war settings. A large literature has studied the macroeconomic effects of post-war policies such as the Marshall Plan (see e.g., De Long and Eichengreen, 1991; Eichengreen and Uzan, 1992). Recent literature has focused on the microeconomic effects of industrial policy in post-war Europe (e.g., Poege, 2022) and the effects of war-time or post-war R&D spending on U.S. innovation (Gross and Sampat, 2023; Kantor and Whalley, 2024). While a few papers have explicitly studied the effects of war reparations on development (e.g., Mitrunen, 2024), this is the first paper to explicitly study intellectual reparations in the form of firm-to-firm knowledge transfers. We contribute to the literature on post-war industrial policy by assembling and analysing a novel database on one of the largest programs of intellectual reparations.

Second, we contribute to the literature on international technology diffusion.[1] Since knowledge transfers are difficult to measure directly, one strand in the literature focuses on the effects of migration on international knowledge diffusion (e.g., Hornung, 2014; Moser et al., 2014; Kerr et al., 2016). Recent papers in the literature have focused on knowledge transfers at the firm level (Giorcelli, 2019) or on the effects of industrial espionage (Glitz and Meyersson, 2020). In this paper, we study a government-backed program of firm-to-firm knowledge transfers. We contribute by building a dataset that allows us to link the investigating U.S. firms and the investigated German firms, thus observing knowledge transfers directly.

4.2 Historical Background

4.2.1 World War II and Reparations

The unconditional surrender of the German armed forces on 8 May 1945 marked the end of WWII in the European theatre. In June 1945, Germany was divided into four occupation zones governed under military law by France, the Soviet Union, the United Kingdom, and the United States. Four years later, they were succeeded by the founding of an East and a West German state.

After the war, Germany had to pay reparations to the Allied powers. For example, territory was ceded to Poland and the labour of German prisoners of war held in Allied nations was exploited. The Allies also agreed on a reduction of the German economic potential which allowed for the dismantling of German plants. While dismantling was most pronounced in the French and Soviet zones, it also happened in the British and U.S. zones. Especially for the United States, however, it soon became evident that heavy physical reparations would not offer many benefits due to the vast geographic distance. Instead, they set their sight on intellectual reparations such as research results, intellectual property, and technical innovation (Gimbel, 1990; O'Reagan, 2021). In line with international law, they initially aimed for innovation which had been applied to the military. However, even while the war was still ongoing, they had already extended the program to also include German civilian industry.

[1] For a comprehensive review of the literature, see for example Comin and Mestieri (2014).

4.2.2 German Know-How as Intellectual Reparations

As part of the Allied war effort, the United Kingdom and the United States decided in summer 1944 to form a combined scientific and technical intelligence unit, the so-called Combined Intelligence Objectives Subcommittee (CIOS). Although CIOS was designed as a joint civilian-military effort, its initial focus was to gather intelligence on Germany's military innovations in areas such as weapons, radar technology, and jet engines (Gimbel, 1990). It was also responsible to locate, detain, and interrogate German scientists and technicians who could posses such information. Its mission was to gather intelligence that could benefit the war against Japan. After the Allied landing in Normandy in the summer of 1944, CIOS sent its first specialist teams of investigators to Paris and soon afterward to other liberated cities, such as Nancy, Luxembourg, and Brussels (Glatt, 1994). Already in September 1944, first calls emerged to also investigate 'economic and industrial intelligence targets of vital postwar interest, but not of immediate military value' (Gimbel, 1990, p. 6).

To deal with the expansion of the program, the United Kingdom and the United States each created further agencies staffed with intelligence officers, military personnel, and civilians.[2] Their tasks included the compilation of a target list, the selection of individuals for the missions to Germany, and the training of investigators. They were also responsible for seizing and securing important targets, organising travel logistics and hospitality, and facilitating the distribution of findings through reports. After the war had ended, in July 1945 the combined headquarter of Allied forces and other combined organisations such as CIOS were terminated. To continue the intellectual reparation program, both the United Kingdom and the United States shifted the responsibility to national agencies. The two most notable ones were the U.S. FIAT (Field Intelligence Agency, Technical) and the British BIOS (British Intelligence Objectives Subcommittee), which lent their names to the reports published on the investigations.

In late 1946, the military government in charge of Germany started to lobby for the program's termination (Gimbel, 1990). Over time, strategic reservations against the political and economic costs of the intellectual reparation program had increased. Firstly, in the emerging new geopolitical environment West Germany was no longer regarded as a defeated enemy but as a potential ally against the Soviet Union. Secondly, the reduction of occupation costs required a recovery of the German economy. The extraction of reparations was counter-productive in this regard. In February 1947, FIAT published a 'last call for Germany' (Gimbel, 1990, p.111) to invite U.S. companies

[2]Details on all involved agencies and their particular responsibilities can be found in Gimbel (1990), Glatt (1994), and O'Reagan (2021).

to participate in a field trip. The United Kingdom and the United States agreed in February 1947 that no new investigations were permitted after 15 May of that year. All investigations were terminated by 30 June 1947 – less than a month after the Marshall Plan had been announced.

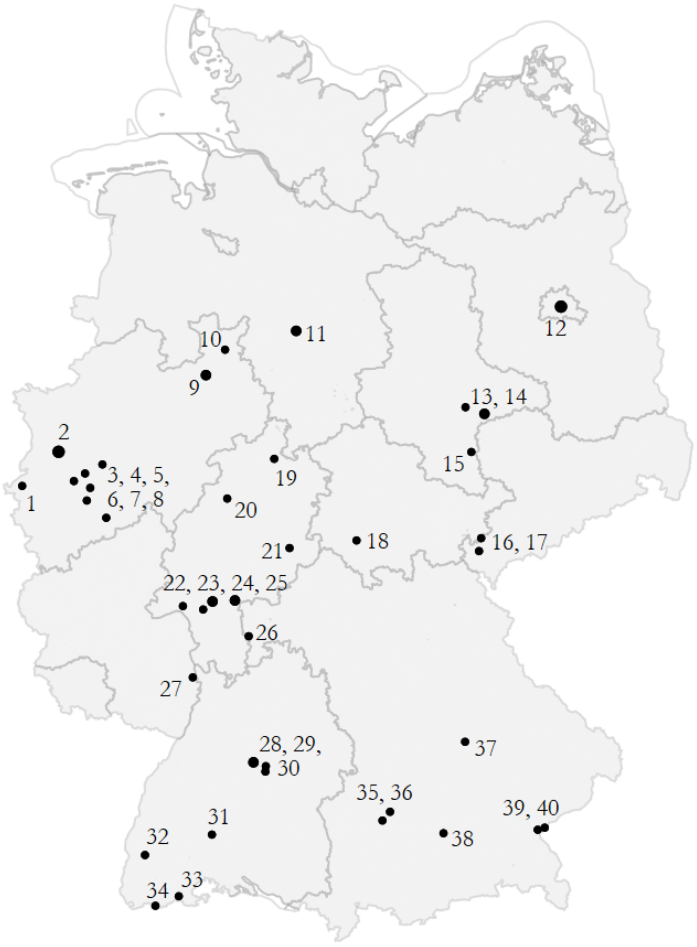
Figure 4.1: CIOS INVESTIGATION TEAM, SUMMER 1945

Notes: This figure shows members of the team reporting on their investigations on synthetic fibre developments in CIOS report XXXIII-50. The investigations took place from 28 June to 14 September 1945. The team included five U.S. and four British investigators. The picture shows the investigators in military uniforms: Leroy H. Smith, Dr. G. Preston Hoff, Dr. Joseph B. Quig, Dr. Jan J. Schilthuis, Dr. Dan B. Wicker (all U.S.) and Dr. Rowland Hill, Dr. F. Stanley Brown, Geoffrey Loasby, and Dr. David Traill (all British). The picture has been provided by the Deutsche Museum in Munich - for details see Section 4.A.

By that time, teams of British and U.S. private-industry employees had investigated German firms for over two years. For instance, four British and five U.S. investigators shown in Figure 4.1 travelled through Germany from 28 June to 14 September 1945. They gathered information on synthetic fibre developments presented in CIOS report XXXIII-50. In total, they investigated 56 firms located throughout Germany – from the Ruhr area to Munich (see Figure 4.2). They even investigated a few firms located in what was to become the Soviet zone – before the area was handed over by U.S. troops. While the investigators were employees of private companies, they were given military authority and uniforms for their investigations in Germany. In line with military law, German firms had to share all requested information. However, information was overwhelmingly shared voluntarily, for example, to gain preferential treatment by the occupiers. CIOS report XXXIII-50 states 'when the individual found the investigators [...] could use military force if necessary, we had no further trouble and received

accurate information. In most cases, the Germans involved were quite proud of their research work and development and were happy to talk about them.'

Figure 4.2: EXEMPLARY TRAVEL ROUTE: CIOS REPORT XXXIII-50

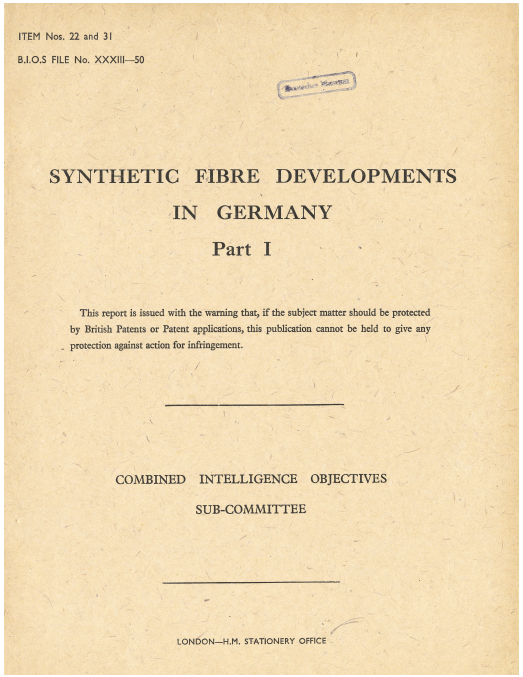


(1) Vereinigte Glanzstoff-Fabriken AG	(11) D. Wollwäscherei und -kämmerei	(21) Val. Mehler Segeltuchweberei AG	(31) I.G. Farbenindustrie AG
(2) Joh. Kleinewefers Söhne	(11) Continental Gummiwerke AG	(22) I.G. Farbenindustrie AG	(32) Rhodiaseta AG
(2) Rheinische Kunstseide AG	(12) Spinnfaser AG	(23) Vereinigte Glanzstoff-Fabriken AG	(33) Lonza-Werk
(2) Rheinische Zellwolle AG	(12) KWI für Physikalische Chemie	(24) I.G. Farbenindustrie AG	(34) Lonzana AG für Acetatprodukte
(3) Kampf & Spindler	(13) Eilfeld AG	(24) I.G. Farbenindustrie AG	(35) I.G. Farbenindustrie AG
(4) I.G. Farbenindustrie AG	(14) I.G. Farbenindustrie AG	(25) Condux-Werke (H.A. Merges KG)	(36) Augsburger Kammgarn-Spinnerei
(5) Glanzstoff-Courtaulds	(14) I.G. Farbenindustrie AG	(25) W.C. Heraeus GmbH	(37) Süddeutsche Zellwolle AG
(6) I.G. Farbenindustrie AG	(15) I.G. Farbenindustrie AG	(26) Vereinigte Glanzstoff-Fabriken AG	(38) Agfa Camerawerke
(7) I.G. Farbenindustrie AG	(16) Vereinigte Glanzstoff-Fabriken AG	(27) I.G. Farbenindustrie AG	(39) I.G. Farbenindustrie AG
(8) Rheinische Zellwolle AG	(17) Sächsische Zellwolle AG	(28) Werner & Pfleiderer	(40) Dr. Alexander Wacker GmbH
(9) Hermann Windel GmbH	(18) Thüringische Zellwolle AG	(28) Maschinenfabrik Gg. Kiefer GmbH	
(9) Seydel Maschinenfabrik GmbH	(19) Spinnfaser AG	(29) Fritz Müller Pressenfabrik	
(10) C.A. Delius & Söhne	(20) I.G. Farbenindustrie AG	(30) Zellwoll-Lehrspinnerei	

Notes: This figure shows all firm investigations in Germany covered in CIOS report XXXIII-50. Larger dots indicate that multiple firms were located in one municipality. The map is based on the municipality and state structure from 2023. Appendix Table 4.C.1 lists the respective dates of each investigation (from 28 June to 14 September 1945).

Figure 4.3: EXAMPLE: CIOS REPORT XXXIII-50 ON SYNTHETIC FIBRE DEVELOPMENT

(a) Cover page



ITEM Nos. 22 and 31

B.I.O.S FILE No. XXXIII—50

SYNTHETIC FIBRE DEVELOPMENTS IN GERMANY

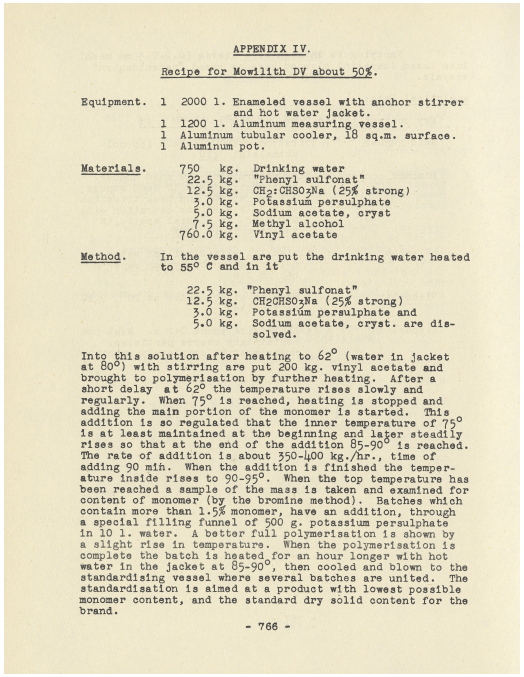
Part I

This report is issued with the warning that, if the subject matter should be protected by British Patents or Patent applications, this publication cannot be held to give any protection against action for infringement.

COMBINED INTELLIGENCE OBJECTIVES SUB-COMMITTEE

LONDON—H.M. STATIONERY OFFICE

(b) Product recipe



APPENDIX IV.

Recipe for Mowilith DV about 50%.

Equipment. 1 2000 l. Enameled vessel with anchor stirrer and hot water jacket.
1 1200 l. Aluminum measuring vessel.
1 Aluminum tubular cooler, 18 sq.m. surface.
1 Aluminum pot.

Materials. 750 kg. Drinking water
22.5 kg. "Phenyl sulfonat"
12.5 kg. CH_2:$CHSO_3Na$ (25% strong)
3.0 kg. Potassium persulphate
5.0 kg. Sodium acetate, cryst
7.5 kg. Methyl alcohol
760.0 kg. Vinyl acetate

Method. In the vessel are put the drinking water heated to 55° C and in it

22.5 kg. "Phenyl sulfonat"
12.5 kg. CH_2CHSO_3Na (25% strong)
3.0 kg. Potassium persulphate and
5.0 kg. Sodium acetate, cryst. are dissolved.

Into this solution after heating to 62° (water in jacket at 80°) with stirring are put 200 kg. vinyl acetate and brought to polymerisation by further heating. After a short delay at 62° the temperature rises slowly and regularly. When 75° is reached, heating is stopped and adding the main portion of the monomer is started. This addition is so regulated that the inner temperature of 75° is at least maintained at the beginning and later steadily rises so that at the end of the addition 85-90° is reached. The rate of addition is about 350-400 kg./hr., time of adding 90 min. When the addition is finished the temperature inside rises to 90-95°. When the top temperature has been reached a sample of the mass is taken and examined for content of monomer (by the bromine method). Batches which contain more than 1.5% monomer, have an addition, through a special filling funnel of 500 g. potassium persulphate in 10 l. water. A better full polymerisation is shown by a slight rise in temperature. When the polymerisation is complete the batch is heated for an hour longer with hot water in the jacket at 85-90°, then cooled and blown to the standardising vessel where several batches are united. The standardisation is aimed at a product with lowest possible monomer content, and the standard dry solid content for the brand.

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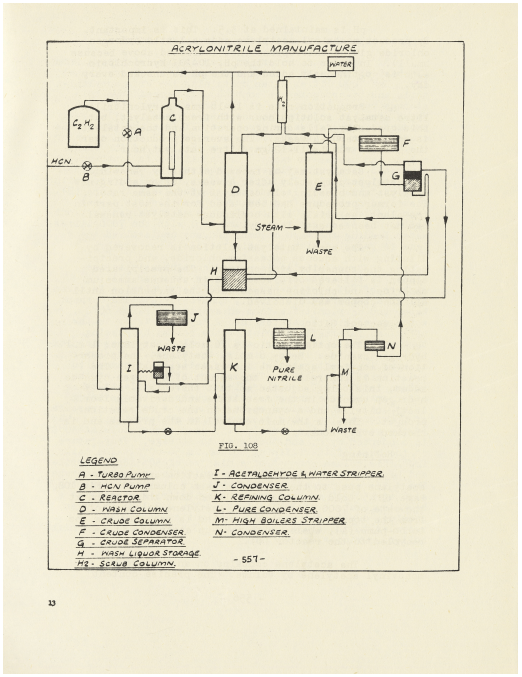
(c) Plant structure



Notes: This figure presents pages from CIOS report XXXIII-50 which covers three months of investigations on more than 1,000 pages. Panel (a) shows the cover page including the title 'Synthetic Fibre Developments in Germany'. Panel (b) presents the recipe for the glue Mowilith DV which is according to the report an innovation on 'the most important single product manufactured' by I.G. Farbenindustrie AG in Höchst. Panel (c) sketches the plant for the production of the chemical intermediate acrylonitrile at I.G. Farbenindustrie AG in Leverkusen. The report includes dozens of such recipes and plant sketches as well as pictures and drawings of machinery. The pictures have been provided by the Deutsche Museum in Munich - for details see Section 4.A.

4.2.3 Reports on Investigations

The United States attempted to distribute the knowledge gained through intellectual reparations. With Executive Order 9604 from summer 1945, U.S. President Truman ordered to release all scientific and industrial information obtained from the enemy to the public (Gimbel, 1990). This included the so-called 'final reports' which British and U.S. investigators were supposed to write on the German firms they had investigated (Gimbel, 1990). Written under the joint CIOS label in early 1945, the first of the reports were meant for circulation within the government only (Glatt, 1994). Following Truman's Executive Order, both governments decided to publish the reports. The reports provided the means to share the knowledge gathered in Germany with the wider British and U.S. industry – beyond those firms sending their own investigators. After the CIOS program had ended, both the investigations and the reports were administrated separately by BIOS and FIAT. The governments continued, however, to exchange reports which then could be read and bought in public libraries both across the United Kingdom and the United States.

Many reports contained commercially exploitable business secrets. For example, the CIOS report XXXIII-50 includes product recipes and plant sketches from I.G. Farbenindustrie AG, which was among the globally leading chemical companies at the time (see Figure 4.3). After reading this report, one American manufacturer remarked: 'This report would be worth twenty million dollars to my company if it could have it exclusively' (Walker, 1946). Also, the Soviet Union purchased every single report (Walker, 1946). Experts reviewing some of the findings at the I.G Farbenindustrie plants judged that 'this windfall information will advance the American dye industry by at least 5 years, and will save millions of dollars in terms of new products and man-hours of research' (U.S. Department of Commerce, 1947). Information in the reports were considered 'so valuable that to get it a single day ahead of a competitor may be worth thousands of dollars' (Walker, 1946). An U.S. aircraft company responded to the question whether the information in the reports had made the company any money with: 'Yes — at least a hundred thousand dollars' (Walker, 1946). Others were less optimistic on the overall value of reports. O'Reagan (2021) suggests that the most important findings were not written in reports and were rather kept secret by the investigators. He also argues that no matter how well a report was written, firms could not reproduce the described technology. Irrespective of the reports' value to firms not involved in the investigations, reports such as CIOS XXXIII-50 illustrates well that many investigators collected important technical know-how in Germany.

4.3 Data

4.3.1 Data Sources

Investigation Reports

The final reports are key to understanding the extent of the investigation program. They contain all relevant information on the names and locations of the German firms that were investigated. While investigators could purposely not report on specific discoveries made at individual firms, they had to fully document their itinerary. As transportation was provided by government agencies, these were well-informed about the German firms each investigator had investigated. The investigation reports, hence, provide the full picture on which German firms were investigated by which investigators.

We digitise information from the full stock of BIOS, CIOS, and FIAT reports archived at the Deutsches Museum in Munich and the Imperial War Museum in London.[3] We processed more than 150,000 pages of paper. Our database is the first systematic and comprehensive collection of information on the investigations. For each final report, we collect information on the investigators (names, military ranks, private-sector employers), the investigations (date and location), investigated German firms (names and interviewed employees), and the publication itself (report number, title, date, classification, and pages). Generally, the reports were supposed to include all this information to be informative for intelligence agencies and private businesses alike. Figure 4.4 shows how this information is presented in the reports.

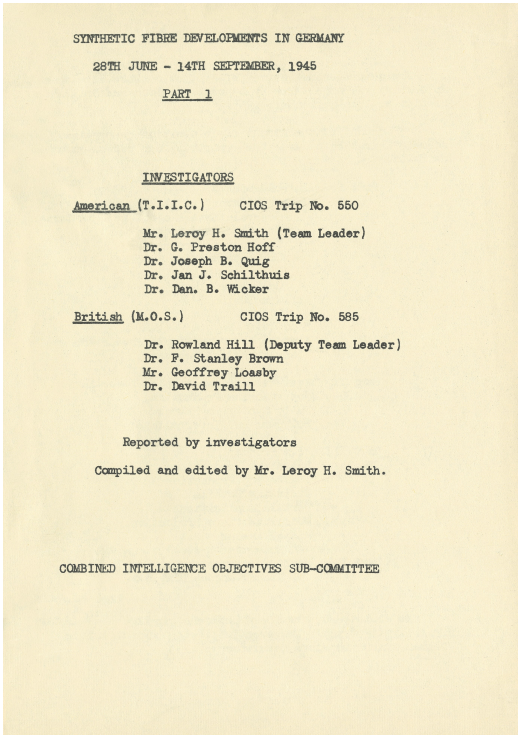
Going beyond the illustrative example provided by CIOS report XXXIII-50, the reports are in general relating well to a program aimed at collecting insights into the German industry. This can be shown by analysing the words employed most often in the reports' titles (see Figure 4.B.2 in the Appendix). The most used words are either economic or corporate terms (e.g., manufacture, industry, production, research, and development), describe the approach to collect information (e.g., investigation, interrogation, and report) or refer to a German firm's name (e.g., *Farbenindustrie*).

Data coverage. To assess the completeness of our dataset, we distinguish between published and non-published reports. Out of 2,726 published reports more than 99% are included in our dataset (see Table 4.C.2 in the Appendix). However, not every

[3]The reports have never been digitised, forcing us to manually extract information from the physical copies of the reports. In fact, the archives in the United States have not cataloged the reports yet, with the exception of CIOS at NARA and FIAT at the Library of Congress.

Figure 4.4: EXAMPLE: EXTRACTING INFORMATION FROM REPORTS

(a) Investigators



SYNTHETIC FIBRE DEVELOPMENTS IN GERMANY

28TH JUNE - 14TH SEPTEMBER, 1945

PART 1

INVESTIGATORS

American (T.I.I.C.) CIOS Trip No. 550

Mr. Leroy H. Smith (Team Leader)
Dr. G. Preston Hoff
Dr. Joseph B. Quig
Dr. Jan J. Schilthuis
Dr. Dan. B. Wicker

British (M.O.S.) CIOS Trip No. 585

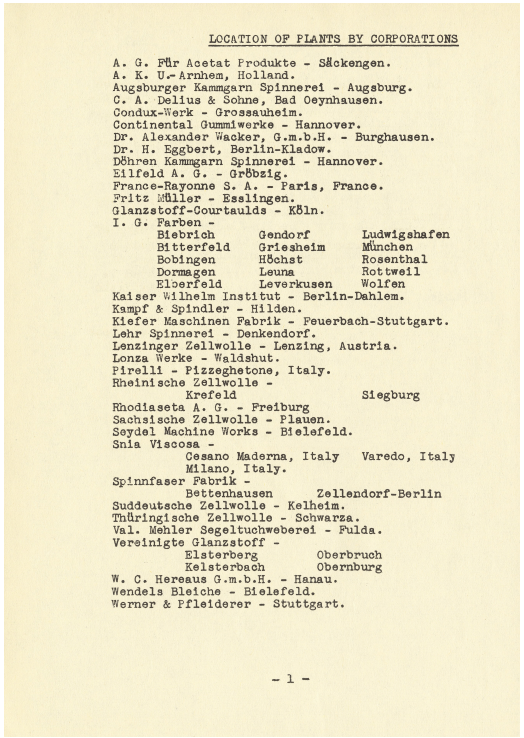
Dr. Rowland Hill (Deputy Team Leader)
Dr. F. Stanley Brown
Mr. Geoffrey Loasby
Dr. David Traill

Reported by investigators

Compiled and edited by Mr. Leroy H. Smith.

COMBINED INTELLIGENCE OBJECTIVES SUB-COMMITTEE

(b) German firms



LOCATION OF PLANTS BY CORPORATIONS

A. G. Für Acetat Produkte - Säckengen.
A. K. U.-Arnhem, Holland.
Augsburger Kammgarn Spinnerei - Augsburg.
C. A. Delius & Sohne, Bad Oeynhausen.
Condux-Werk - Grossauheim.
Continental Gummiwerke - Hannover.
Dr. Alexander Wacker, G.m.b.H. - Burghausen.
Dr. H. Eggbert, Berlin-Kladow.
Döhren Kammgarn Spinnerei - Hannover.
Eilfeld A. G. - Gröbzig.
France-Rayonne S. A. - Paris, France.
Fritz Müller - Esslingen.
Glanzstoff-Courtaulds - Köln.
I. G. Farben -

Biebrich	Gendorf	Ludwigshafen
Bitterfeld	Griesheim	München
Bobingen	Höchst	Rosenthal
Dormagen	Leuna	Rottweil
Elberfeld	Leverkusen	Wolfen

Kaiser Wilhelm Institut - Berlin-Dahlem.
Kampf & Spindler - Hilden.
Kiefer Maschinen Fabrik - Feuerbach-Stuttgart.
Lehr Spinnerei - Denkendorf.
Lenzinger Zellwolle - Lenzing, Austria.
Lonza Werke - Waldshut.
Pirelli - Pizzeghetone, Italy.
Rheinische Zellwolle -
Krefeld Siegburg
Rhodiaseta A. G. - Freiburg
Sachsische Zellwolle - Plauen.
Seydel Machine Works - Bielefeld.
Snia Viscosa -
Cesano Maderna, Italy Varedo, Italy
Milano, Italy.
Spinnfaser Fabrik -
Bettenhausen Zellendorf-Berlin
Suddeutsche Zellwolle - Kelheim.
Thüringische Zellwolle - Schwarza.
Val. Mehler Segeltuchweberei - Fulda.
Vereinigte Glanzstoff -
Elsterberg Oberbruch
Kelsterbach Obernburg
W. C. Hereaus G.m.b.H. - Hanau.
Wendels Bleiche - Bielefeld.
Werner & Pfleiderer - Stuttgart.

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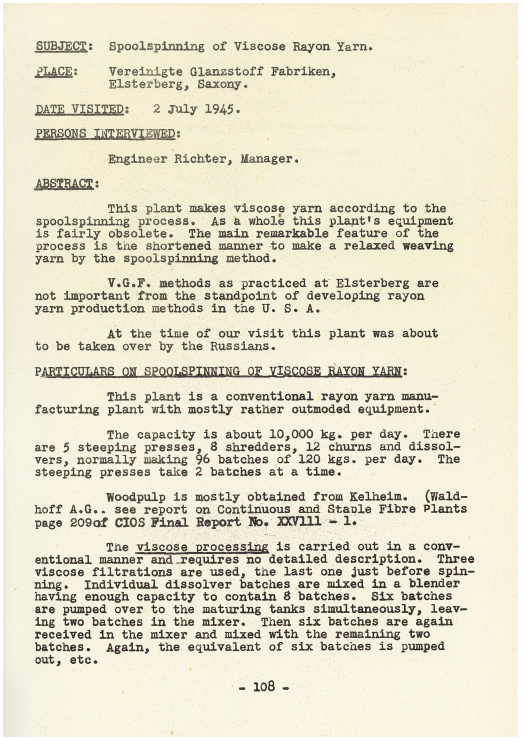
(c) Details of investigation



SUBJECT: Spoolspinning of Viscose Rayon Yarn.

PLACE: Vereinigte Glanzstoff Fabriken,
Elsterberg, Saxony.

DATE VISITED: 2 July 1945.

PERSONS INTERVIEWED:

Engineer Richter, Manager.

ABSTRACT:

This plant makes viscose yarn according to the spoolspinning process. As a whole this plant's equipment is fairly obsolete. The main remarkable feature of the process is the shortened manner to make a relaxed weaving yarn by the spoolspinning method.

V.G.F. methods as practiced at Elsterberg are not important from the standpoint of developing rayon yarn production methods in the U. S. A.

At the time of our visit this plant was about to be taken over by the Russians.

PARTICULARS ON SPOOLSPINNING OF VISCOSE RAYON YARN:

This plant is a conventional rayon yarn manufacturing plant with mostly rather outmoded equipment.

The capacity is about 10,000 kg. per day. Taere are 5 steeping presses, 8 shredders, 12 churns and dissolvers, normally making 96 batches of 120 kgs. per day. The steeping presses take 2 batches at a time.

Woodpulp is mostly obtained from Kelheim. (Waldhoff A.G.. see report on Continuous and Staple Fibre Plants page 209of CIOS Final Report No. XXVlll - 1.

The viscose processing is carried out in a conventional manner and requires no detailed description. Three viscose filtrations are used, the last one just before spinning. Individual dissolver batches are mixed in a blender having enough capacity to contain 8 batches. Six batches are pumped over to the maturing tanks simultaneously, leaving two batches in the mixer. Then six batches are again received in the mixer and mixed with the remaining two batches. Again, the equivalent of six batches is pumped out, etc.

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Notes: This figure shows how the relevant information is presented based on the CIOS report XXXIII-50. Panel (a) presents the names of the British and U.S. investigators who were part of the investigations. For the U.S. investigators George P. Hoff, Leroy H. Smith, Dr. Joseph B. Quig, Dr. Jan J. Schilthuis, and Dr. Dan B. Wicker, we know their private-sector U.S. employers from the investigator list. Panel (b) lists the German firms and their locations which have been investigated by this group. Panel (c) presents details on the investigation at 'Vereinigte Glanzstoff Fabriken' in Elsterberg on 2 July 1945 including the name and position of the German personnel that was interviewed. The pictures have been provided by the Deutsche Museum in Munich - for details see Section 4.A.

report has been published and made available to industry. Some reports were classified due to the sensitivity of the content or were not considered to contain sufficiently valuable technical information. These reports were only circulated within the administration. Our dataset includes another 1,150 of these non-published reports. A few reports, however, were of such low quality that they were not even circulated within the administration. With only limited copies of these reports being produced, some were not archived and, hence, can not be included in our dataset.[4] Naturally, our dataset does not cover investigations for which no report has been written. While every investigator was expected to submit a report, not everyone followed the rules. Yet our dataset still covers 89% of all potential reports.[5] In terms of firm investigations, the covered share should be even higher as the number of German firms in published reports is twice the number in non-published reports. Also, we do not expect strategic reasons for not submitting a report or writing it in poor quality, because investigators who wanted to hide their findings could have also written a proper report without sharing any valuable information.

Summary statistics. The initial dataset includes 3,873 reports. Not all of these reports allow us to extract information on investigated German firms. Some reports describe investigations at administrative and military offices, others translate scientific papers, or just do not mention individual firms. 352 reports do not cover investigations of firms (see Figure 4.5a). With 13% each, the share of such reports is largest for CIOS and FIAT. For CIOS this is mainly driven by reports on the government and military, while for FIAT those are primarily translations of scientific papers. In addition, 160 reports are exclusively on investigations of firms which are located outside of Germany (see Figure 4.5b). This is mainly relevant for CIOS reports as these were written before Germany was liberated.

Our final dataset consists of 3,361 reports which all contain information on investigations of firms located in Germany. Figure 4.5c shows the share of published reports. Overall, 70% of the reports have been made available in public libraries and offered for sale. The largest share of reports was published for BIOS. Reasons for a report not being published were low quality, little insights, or a classification due to sensitive content. 529 reports were classified, which implies that sensitive content was the main

[4]The archive of the Imperial War Museum in London, which is one of our data sources, is generally considered to be the archive with the most complete collection of final reports.

[5]We arrive at this number by taking the highest report number for each label (BIOS, CIOS, and FIAT) to calculate the amount of potentially missing reports. This is the most conservative estimate for completeness as it assumes that missing report numbers do actually exist. Missing report numbers could, in contrast, also be explained by reports being merged or teams not travelling to Germany. Hence, 89% should be seen as the lower bound for how complete our data collection is.

Figure 4.5: INFORMATION ON REPORTS

Notes: This figure shows in Panel (a) and (b) how we reduce the initial dataset to the final dataset. Panel (c) and (d) present summary statistics for the final dataset. Our initial dataset includes 3,873 reports: 1,756 BIOS, 974 CIOS, and 1,143 FIAT. Not all these reports contain valuable information on investigations of German firms. Panel (a) shows the number of reports which are not on a firm investigation. Panel (b) shows the number of reports that focus on firms located outside of Germany. Panels (c) and (d) do not include these 'not relevant' reports. For the final dataset, Panel (c) shows the share of reports published in libraries and offered for sale to companies. Panel (d) shows the share of reports initially classified as Secret, Confidential, or Restricted.

reason for reports not being published (see Figure 4.5d). Non-published and classified reports remain in our dataset as they provide valuable information on who was potentially able to extract knowledge from firms in Germany.

Investigator List

While reports mention the investigators who were collecting information on German firms, most reports do not state their employers. We collect data on all U.S. investigators from an index of U.S. personnel published in May 1947.[6] This document by the

[6]For details on the employed archival sources see Appendix 4.A.

Department of Commerce lists the technical and administrative personnel associated with the investigations. Figure 4.6 is an excerpt from this list showing the name and location of a U.S. firm employing one of the investigators. It shows the entry for a U.S. investigator named LeRoy Smith, who worked at American Viscose Corporation in Roanoke, Virginia. The figure also lists three of the reports on investigations which LeRoy Smith was involved in.

As no investigators were permitted to enter Germany after May 1947, the list is a near-to-complete collection of civilian investigators. It allows us to understand which U.S. firms were involved in the investigation. Out of 1,143 FIAT reports and 974 CIOS reports, we are able to link 900 (79%) and 354 (36%) reports to someone on the investigator list, respectively.[7] This is a relatively high share, as many investigations were also conducted by technically educated soldiers (for FIAT and CIOS) and by British investigators (for CIOS).

Figure 4.6: INVESTIGATOR LIST

SMITH, LeRoy H. General Manager American Viscose Corp. Roanoke, Virginia	14473	FIAT 147	Behnsen Continuous Shredder, Wolfgang Boi Hanau (Microfilm .50 Photostat $1.00)
	14517	FIAT 35	Bobbin Finning Process of Viscose Rayon Textile Yarn and of Yarn for Tire Cord at Snia Viscosa Cesano Laderno (Microfilm .50 Photostat $1.00)
	1113	FIAT 10	Continuous Process for Spinning Viscose Yarn at Zellwolle Lenzing Aktiengellschaft Lenzing Oberdonau Austria (Microfilm .50 Photostat $1.00)

Notes: This figure shows an excerpt from the investigator list. From this, we learn that Leroy H. Smith, an investigator from CIOS report XXXIII-50, was a general manager at American Viscose Corp. in Roanoke, Virginia. The company was a large rayon producer which relates well to the subject Mr. Smith was investigating in Germany. The company is also among the top three U.S. firms conducting investigations in Germany as shown in Table 4.2.

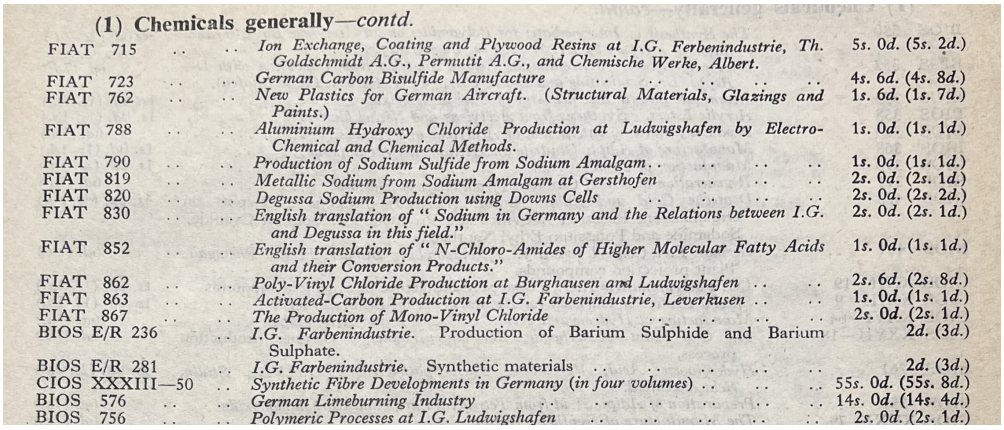
Topic List

The confidential topic list by H.M. Stationary provides an overview of the BIOS, CIOS, and FIAT reports that have been published in the United Kingdom until October 1949. It groups the reports into 21 categories such as Agriculture, Metal Industries, and Optical and Mechanical Precision Instruments. It also provides an even more detailed grouping into 221 subcategories. Figure 4.7 shows an excerpt for the category Chemicals, which lists the numbers and names of reports belonging to this category.

[7]No list of British investigators and their employers is archived at The National Archives in London.

Most reports are assigned to one category (47%) or two categories (39%), with 15% of the reports being assigned to more than two categories.

Figure 4.7: TOPIC LIST



(1) Chemicals generally—*contd.*

FIAT 715	*Ion Exchange, Coating and Plywood Resins at I.G. Ferbenindustrie, Th. Goldschmidt A.G., Permutit A.G., and Chemische Werke, Albert.*	5*s.* 0*d.* (5*s.* 2*d.*)
FIAT 723	*German Carbon Bisulfide Manufacture*	4*s.* 6*d.* (4*s.* 8*d.*)
FIAT 762	*New Plastics for German Aircraft. (Structural Materials, Glazings and Paints.)*	1*s.* 6*d.* (1*s.* 7*d.*)
FIAT 788	*Aluminium Hydroxy Chloride Production at Ludwigshafen by Electro-Chemical and Chemical Methods.*	1*s.* 0*d.* (1*s.* 1*d.*)
FIAT 790	*Production of Sodium Sulfide from Sodium Amalgam*	1*s.* 0*d.* (1*s.* 1*d.*)
FIAT 819	*Metallic Sodium from Sodium Amalgam at Gersthofen*	2*s.* 0*d.* (2*s.* 1*d.*)
FIAT 820	*Degussa Sodium Production using Downs Cells*	2*s.* 0*d.* (2*s.* 2*d.*)
FIAT 830	*English translation of "Sodium in Germany and the Relations between I.G. and Degussa in this field."*	2*s.* 0*d.* (2*s.* 1*d.*)
FIAT 852	*English translation of "N-Chloro-Amides of Higher Molecular Fatty Acids and their Conversion Products."*	1*s.* 0*d.* (1*s.* 1*d.*)
FIAT 862	*Poly-Vinyl Chloride Production at Burghausen and Ludwigshafen*	2*s.* 6*d.* (2*s.* 8*d.*)
FIAT 863	*Activated-Carbon Production at I.G. Farbenindustrie, Leverkusen*	1*s.* 0*d.* (1*s.* 1*d.*)
FIAT 867	*The Production of Mono-Vinyl Chloride*	2*s.* 0*d.* (2*s.* 1*d.*)
BIOS E/R 236	*I.G. Farbenindustrie.* Production of Barium Sulphide and Barium Sulphate.	2*d.* (3*d.*)
BIOS E/R 281	*I.G. Farbenindustrie.* Synthetic materials	2*d.* (3*d.*)
CIOS XXXIII—50	*Synthetic Fibre Developments in Germany (in four volumes)*	55*s.* 0*d.* (55*s.* 8*d.*)
BIOS 576	*German Limeburning Industry*	14*s.* 0*d.* (14*s.* 4*d.*)
BIOS 756	*Polymeric Processes at I.G. Ludwigshafen*	2*s.* 0*d.* (2*s.* 1*d.*)

Notes: This figure shows an excerpt from the topic list. We learn that the CIOS report XXXIII-50 is categorised as dealing with Chemicals - more specifically its subcategory Generally. The report is also grouped to one further category and three further subcategories, which are all not shown in this figure.

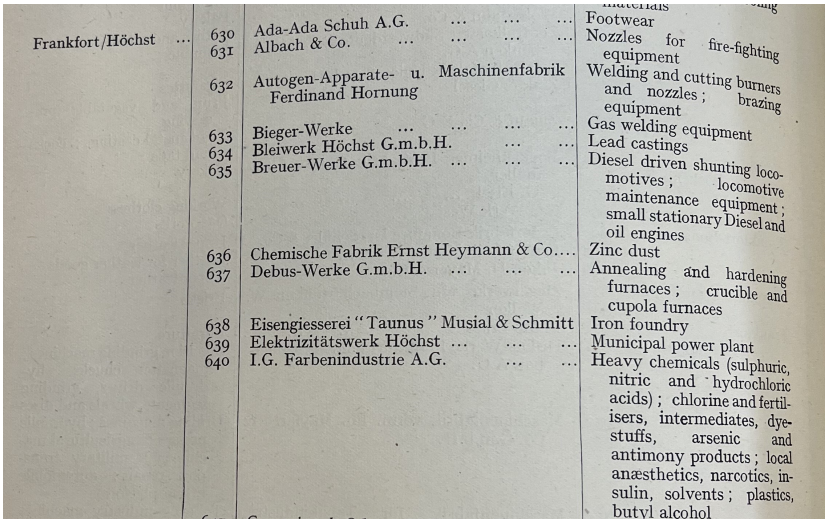
Target List

The German Industry Survey provides an index of more than 6,000 relevant German firms which has been compiled by the Foreign Office and the Ministry of Economic Warfare. It is structured by geographic area in five books and excludes the Soviet zone. Gimbel (1990) refers to this as blue books providing guidance for the investigations. The target list was prepared in late 1944 and early 1945 (Gimbel, 1990). Initially, it was supposed to only include targets of military value which could benefit the war against Japan. But before the war had ended, it was already extended to also include the most relevant private-sector German firms which were known to Allied intelligence. The target list allows to differentiate between investigated German firms that were designated as a target before the war was over and those for which the decision to investigate came later. Figure 4.8 shows an excerpt from the target list for the Hesse area providing names and location of firms as well as their products.

4.3.2 Final Dataset on Knowledge Transfers

Our dataset is centered around the investigations at German firms between 1945 and 1947. This dataset can be linked to other data on firm outcomes for U.S. firms that have sent their employees as investigators to Germany. To construct our dataset in

Figure 4.8: TARGET LIST

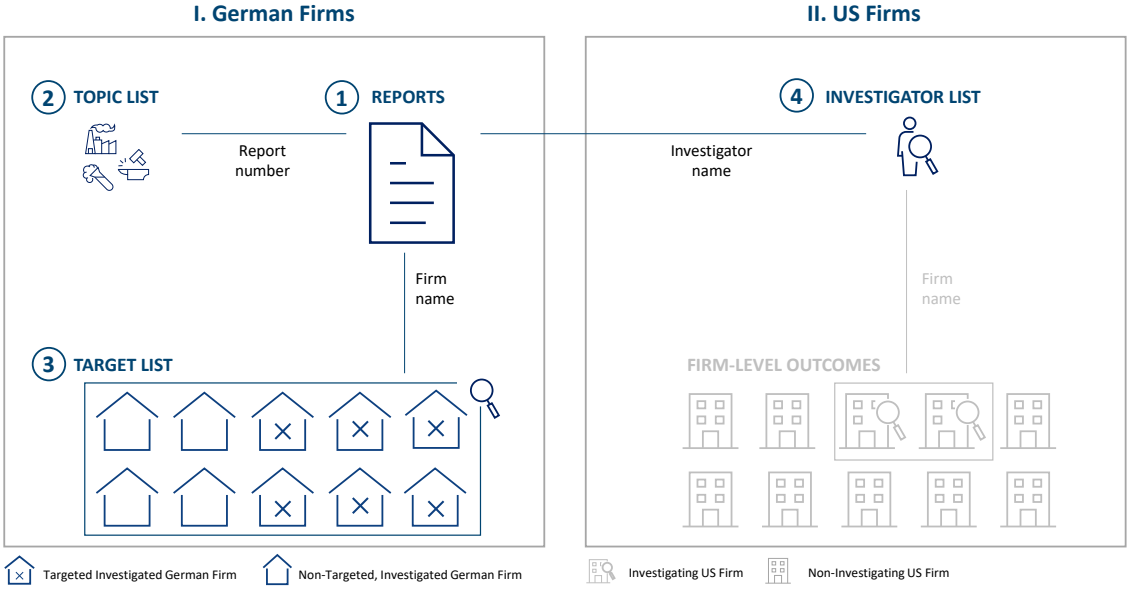


Frankfort/Höchst ...	630	Ada-Ada Schuh A.G.	Footwear
	631	Albach & Co.	Nozzles for fire-fighting equipment
	632	Autogen-Apparate- u. Maschinenfabrik Ferdinand Hornung	Welding and cutting burners and nozzles; brazing equipment
	633	Bieger-Werke	Gas welding equipment
	634	Bleiwerk Höchst G.m.b.H.	Lead castings
	635	Breuer-Werke G.m.b.H.	Diesel driven shunting locomotives; locomotive maintenance equipment; small stationary Diesel and oil engines
	636	Chemische Fabrik Ernst Heymann & Co....	Zinc dust
	637	Debus-Werke G.m.b.H.	Annealing and hardening furnaces; crucible and cupola furnaces
	638	Eisengiesserei "Taunus" Musial & Schmitt	Iron foundry
	639	Elektrizitätswerk Höchst	Municipal power plant
	640	I.G. Farbenindustrie A.G.	Heavy chemicals (sulphuric, nitric and hydrochloric acids); chlorine and fertilisers, intermediates, dyestuffs, arsenic and antimony products; local anæsthetics, narcotics, insulin, solvents; plastics, butyl alcohol

Notes: This figure shows an excerpt from the target list. We learn that I.G. Farbenindustrie AG was a suggested target for the city of Frankfurt. It was also visited by the investigators reporting on their findings in CIOS report XXXIII-50.

such a way, we combine four distinct archival sources collected from three archives in Germany and the United Kingdom. To the best of our knowledge, none of these data sources has been employed for research before.

Figure 4.9: LINKING DATA SOURCES



Notes: This figure shows how our dataset is constructed by linking four archival data sources. While the dataset is centred on German firms, the investigator list builds a bridge to U.S. firms. This allows to link investigated German firms to U.S. firms that sent their employees as investigators to Germany.

Figure 4.9 illustrates how the four data sources form the final dataset on the investigations. The main dataset is extracted from investigation reports that were written

by British and U.S. investigators on German firms (see (1) in Figure 4.9). The reports provide the name and location of the investigated firms. Each report is assigned a unique number which allows us to match them to the topic list prepared by the British government (2). The topics are informative about the industry a German firm was active in. Based on whether a firm's name is included on a target list prepared before the end of the war, we also know whether a firm was deliberately targeted by British and U.S. investigators or merely visited by chance (3). Crucially, the investigator list allows us to link U.S. investigators to the U.S. firms they were employed at (4). In the future, we can add any firm-level data to the U.S. side of our dataset.

Overall, the key contribution of our dataset is to connect investigated German firms with investigating firms from the United States. Our dataset is informative about which U.S. firms received deep knowledge about specific German firms by sending their employees to conduct detailed investigations. For example, using balance sheet data we can investigate the effects of the investigations on the overall performance of participating firms. Alternatively, patent data can be used to study follow-on effects on U.S. innovation.

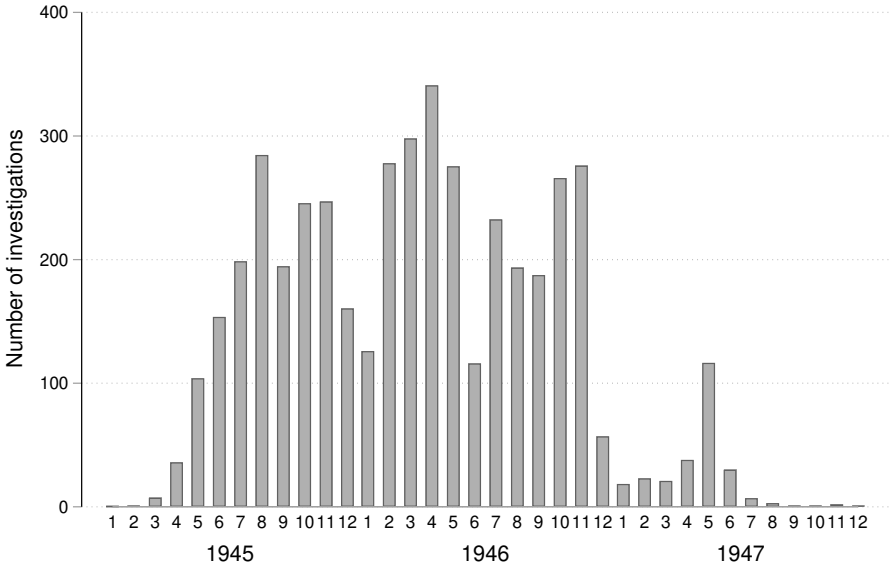
4.4 Mapping the Intellectual Reparations Program

In total, more than 20,000 investigations of German firms were conducted by British and U.S. investigators. The cumulative time spent on the investigations was 25,000 days (based on the subset of 50% of investigations which can be dated). Accordingly, the effort arising from this program was immense – both for the investigators and their administrations, but also for the German firms that had to receive and answer to the investigators. The busiest period lasted for 18 months from May 1945 to November 1946, when every month at least 100 dated investigations were conducted (see Figure 4.10).

While some investigations took place before the German surrender, the program increased in scale just when hostilities had ended in the European war theatre, which shows its predominantly civilian character. The last investigators were permitted to enter Germany in May 1947 and the program was terminated in July 1947. However, throughout 1947 the number of investigations was already low – with a small peak before the program's end. Figure 4.10 illustrates these temporal patterns.

On average, an investigator team included three members, but numbers varied between one and eleven investigators. Since travel could be organised more easily from the United Kingdom, British investigator teams were usually larger than those from

Figure 4.10: NUMBER OF INVESTIGATED GERMAN FIRMS OVER TIME



Notes: This figure shows the number of firm investigations by British and U.S. investigators in Germany for each month between 1945 and 1947. The date is available for close to half of the investigations, implying that only a subset of investigations is shown here.

the United States. In total, the investigators spoke to more than 29,000 employees of German firms. As most reports did not just include the names but also the occupations of these German employees, we can classify them in two groups: those with a managerial and those with a technical background. Based on this grouping, we observe that investigators interviewed German employees with managerial and with technical backgrounds to a similar degree. The share of employees with a doctoral degree among the interviewed was 40%. This shows that investigators did not just desire a superficial understanding of the business a firm was conducting, but rather to spot technical and scientific innovation that may prove valuable for British and U.S. industry.

Our data suggests that investigating teams were operating under a busy schedule. Given the limited capacity to host and transport investigators, the trips were rather short. On the report level, the average days spent on investigations was 14 and the median was five. During this time, investigators attempted to see many German firms rather than just a few. The average number of firms investigated per report is six and the median is two. The travel itinerary in Section 4.2.2 illustrates how an investigating team traveled through the whole of Germany. The most common scenario was that investigators spent just one day at a German firm and very rarely was it more than five days (see Figure 4.B.3 in the Appendix). The short time period spent at each German

firm suggests that a high level of cooperation from the investigated German firms was necessary for the investigators to gain deeper insights.

4.4.1 Which German Firms were Investigated?

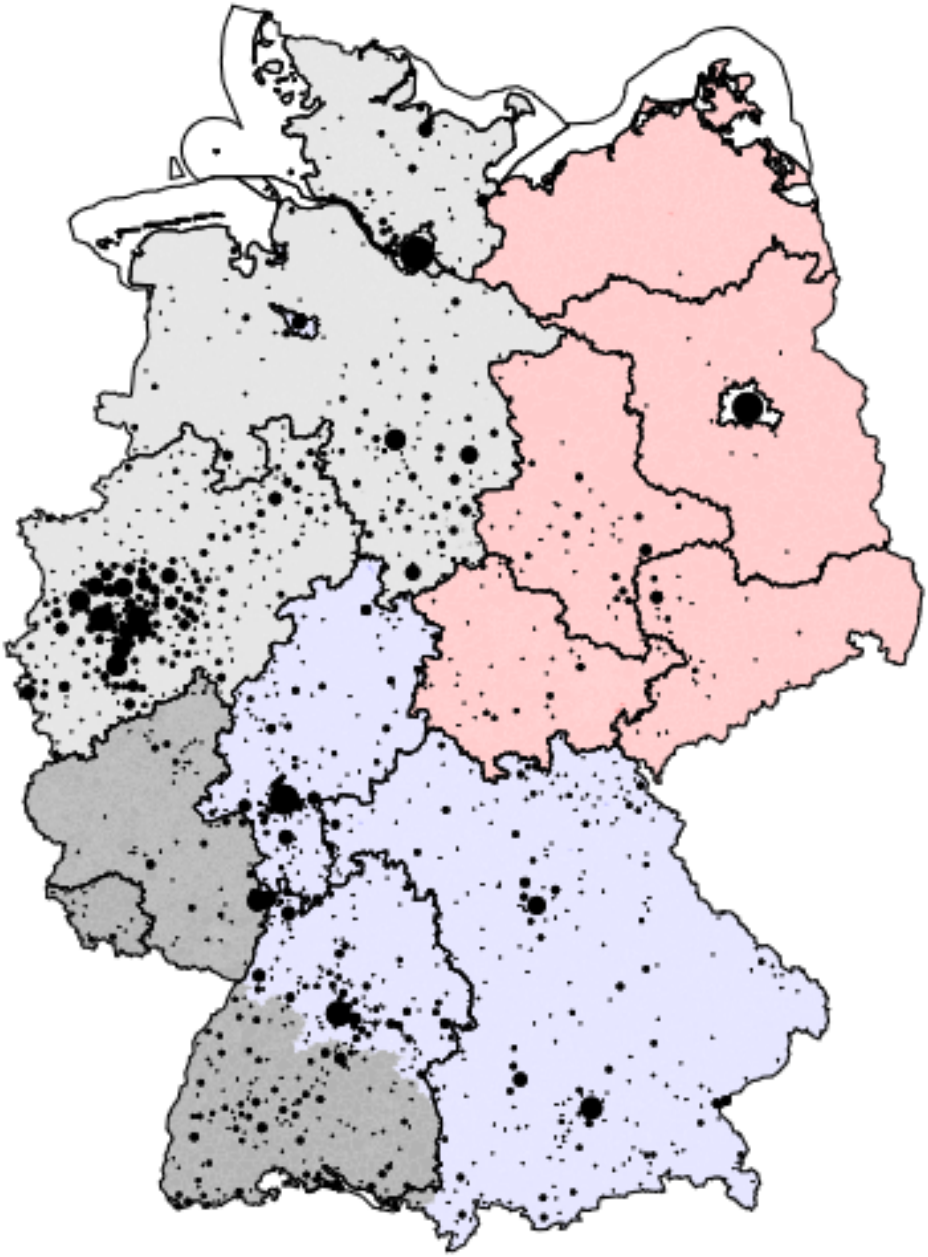
We geolocate all investigated German firms.[8] Our findings show that investigations took place all over Germany (see Figure 4.11). In fact, investigated firms were located in more than 1,200 different municipalities. There is, however, a concentration of investigations in large municipalities. With 1,100 investigations, Hamburg is the most visited municipality, followed by Berlin (880), Frankfurt (860), and Düsseldorf (590). But also smaller municipalities with important firms were subject to frequent investigations. For example, the relatively unknown Krefeld and Ludwigshafen on the Rhine are among the top ten investigated municipalities with more than 350 investigations each. Both host a site of I.G. Farbenindustrie which was the most investigated German firm.

We show that also firms in some parts of the Soviet zone were investigated by British and U.S. investigators. Plotting the location of investigations over time shows, however, that these investigations in the Soviet zone occurred nearly exclusively in 1945 (see Figure 4.B.4 in the Appendix). The reason is that the respective territory was initially held by U.S. troops and, hence, could be investigated until July 1945 when it was handed over to the Soviets in exchange for access to Berlin. Apart from this, the pattern of municipalities visited by investigating firms remained broadly similar between 1945 and 1946. For 1947, the sharp decline in the number of investigations is also visible in the maps.

The majority of investigations by British and U.S. investigators was conducted in their own respective zones (see Figure 4.B.5 in the Appendix). For example, 58% of the firms investigated by British teams (BIOS) were located in the British zone. Due to agreements between the United States and the United Kingdom, their investigators could also investigate pre-approved firms in the other's zone. In fact, 28% of the firms investigated by British investigators were located in the U.S. zone. For firms visited by U.S. investigators, the shares of 44% being in the U.S. and 37% being in the British zone are even closer. The low number of investigations in the French zone shows that investigation trips were to a certain degree limited by access to the desired German firms. This is well illustrated with the Soviet zone. While 20% of the CIOS investigations were of firms located in the Soviet zone, the number dropped to almost

[8] We employ and, hence, refer to the current municipality structure from 2023.

Figure 4.11: LOCATION OF INVESTIGATIONS IN GERMANY



Notes: This figure shows the location of investigations. The size of the dots is proportional to the number of investigations in each municipality. Germany was divided into four occupation zones: Blue is the U.S. zone, light grey is the British zone, dark grey is the French zone, and red is the Soviet zone. The zone borders are based on a map by Kunz (2004). Investigations are extracted from the reports of BIOS, CIOS, and FIAT which include British and U.S. investigators, but not French or Soviet ones. The map is based on German borders as well as the municipality and state structure from 2023. Figure 4.B.4 in the Appendix shows the location of investigations for the years 1945, 1946, and 1947 separately. Figure 4.B.5 in the Appendix shows aggregate statistics for investigations by occupational zone.

none after U.S. troops had retreated from that area. At the same time, Berlin became available for investigations by BIOS and FIAT teams which chose around 7% of their investigations to cover firms in Berlin.

Table 4.1 presents the ten German firms that have been investigated most often. The single most investigated German firm is I.G. Farbenindustrie AG. With around 1,300 investigations it comprises more than 5% of the overall program. For the remaining nine firms, the number of investigations is in the range of 80 to 200. As the program lasted for around two years, that is for each of the firms on average one to two investigations per week. All investigations at these ten German firms started either before the German surrender in May 1945 or immediately afterward. For four of the ten firms, the investigations had ended already in 1946, while they lasted for the remaining six until the end of the program in mid-1947. All firms were included on the target list which was compiled by U.S. intelligence in 1944. The ten most investigated firms were primarily associated with the electrical engineering and chemicals industries. Rheinmetall-Borsig AG was the only firm belonging to the category of armaments and ammunition.

Table 4.1: TOP 10 INVESTIGATED GERMAN FIRMS

	Name	Location	Number	First	Last	Target	Industry
1	I.G. Farbenindustrie AG	Frankfurt	1,312	09.03.45	20.06.47	Y	Chemical
2	Friedrich Krupp AG	Essen	195	14.04.45	03.06.47	Y	Metal
3	Dynamit Nobel AG	Troisdorf	140	28.04.45	27.05.47	Y	Chemical
4	Siemens Halske AG	Berlin	119	09.03.45	13.05.47	Y	E. Engineering
5	Siemens-Schuckertwerke AG	Berlin	114	15.04.45	22.04.47	Y	E. Engineering
6	Degussa GmbH	Frankfurt	112	09.03.45	13.12.46	Y	Chemical
7	Vereinigte D. Metallwerke GmbH	Frankfurt	99	28.05.45	04.11.46	Y	M. Engineering
8	Robert Bosch GmbH	Stuttgart	99	22.05.45	02.12.46	Y	E. Engineering
9	AEG AG	Berlin	93	08.05.45	13.05.47	Y	E. Engineering
10	Rheinmetall-Borsig AG	Düsseldorf	84	23.04.45	09.10.46	Y	Armaments

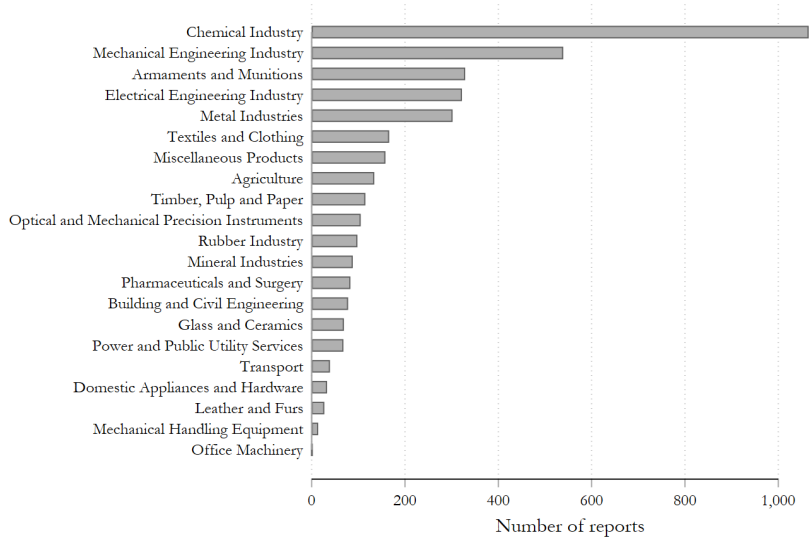
Notes: This table shows the ten German firms which have been investigated most often. As ten to 61 locations of each firm have been investigated, the table presents only the most investigated location. 'Number' gives the total number of investigations. The start date of the first investigation is given in 'First' and the finishing date of the last investigation is given in 'Last'. The information on whether a firm was a target is based on the target list. The industry is given as the category from the topic list which was most often assigned to a report on the firm. 'E. Engineering' is Electrical Engineering and 'M. Engineering' is Mechanical Engineering.

4.4.2 Which Industries were Targeted?

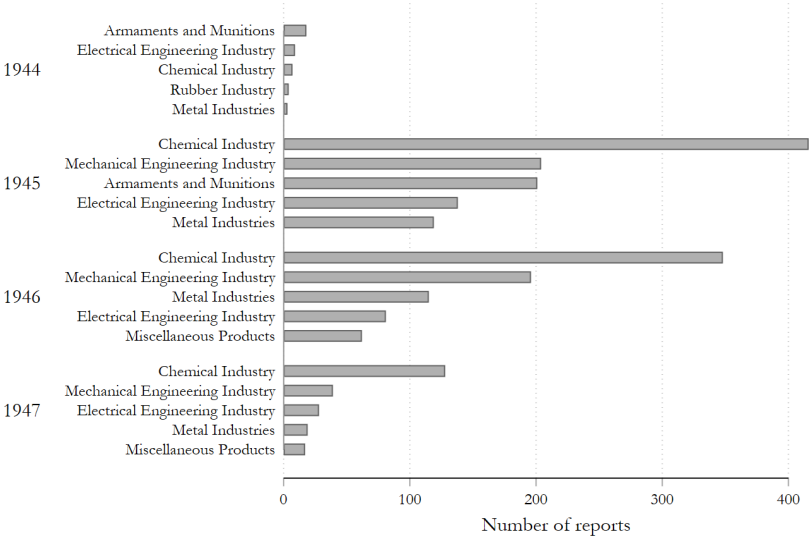
Based on the topics assigned to reports, we can examine which industries were predominantly investigated. With more than 1,000 reports being devoted to it, the chemical industry received the most attention from British and U.S. investigators (see Figure

Figure 4.12: INDUSTRY FOCUS OF INVESTIGATIONS

(a) Investigated industries



(b) Top five investigated industries over time



Notes: This figure shows the distribution of investigations over industries. Panel (a) plots the number of reports belonging to each of the 21 topic categories from the topic list. This information is only available for published reports. Panel (b) plots the number of reports for the top five categories from 1944 to 1947. The report date is available for close to half of reports, implying that Panel (b) shows a subset of reports from Panel (a). To illustrate the shift in the program's topical focus, we also include those reports from 1944 which are on firms and military infrastructure outside of Germany. Most reports are assigned to one category (47%) or two categories (39%), with 15% of reports assigned to more than two categories.

4.12a). Also, the mechanical engineering, the electrical engineering, and the metal industries are with 300 to 550 reports among the five most investigated industries. The target list shows that chemicals, engineering, and metals at that time were the most important industries of the German economy. In total, 2,900 of the 6,100 firms mentioned in the target list belong to either of these industries. The investigators, however, had a disproportionately large interest in the chemical industry. According to the target list, the chemical industry represented 11% of the firms considered to be most relevant, but 21% of the reports are about the chemical industry. Relative to the share of engineering and metal firms on the target list (26% and 10%), the share of reports on these industries is disproportionately small (17% and 6%).

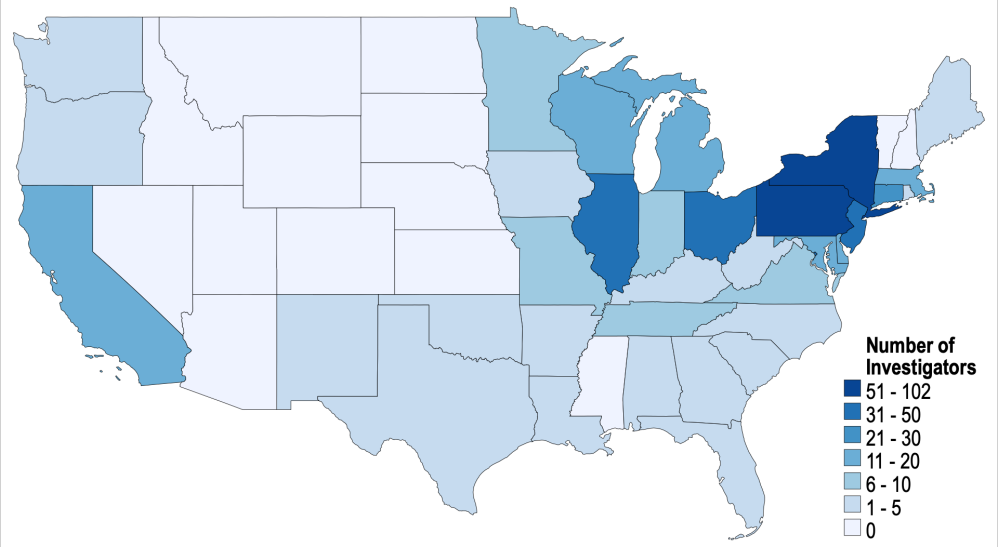
With its non-civilian products, the armaments and munition industry is an exception among the five most investigated industries. Figure 4.12b presents the development of reports dedicated to the top five industries over time. It shows that the defence industry was only of interest in the initial phase of the program in 1944 and 1945. Neither in 1946 nor in 1947 were armaments and munitions part of the top five investigated industries. This suggests that the program's concentration on civilian industries increased over time – especially after both Germany and Japan had surrendered in 1945.

4.4.3 Which U.S. Firms sent Investigators?

In the last part of this paper, we connect investigating U.S. firms to investigated German firms. This allows us to observe specific knowledge transfers. Based on the investigator list, we observe the employers for 491 U.S. investigators. Most of these investigators were sent by employers located in New York (102 investigators), Pennsylvania (61), and Washington, D.C. (42). Table 4.C.3 in the Appendix lists the top ten states in terms of employed investigators. Figure 4.13 indicates that most investigators came from the East Coast and the Midwest. 15 states did not host a single firm sending an investigator to Germany. This geographic pattern even persists when accounting for population size (see Figure 4.B.6 in the Appendix).

Table 4.2 lists the ten U.S. firms which have conducted most investigations in Germany as part of the intellectual reparation program. The number of total investigations by their employees varies between 62 and 201. Nine of the ten most active U.S. firms were located either on the East Coast or in the East of the Midwest. In line with the generally strong focus on the chemical industry, a majority of the ten firms was mainly involved with investigations of German firms operating in this industry. In addition, two firms focused on the mechanical engineering industry, one on the timber, pulp,

Figure 4.13: NUMBER OF U.S. INVESTIGATORS BY STATE



Notes: This figure shows the number of investigators from each U.S. state. The information on the location of investigators is drawn from the investigator list.

Table 4.2: TOP 10 INVESTIGATING U.S. FIRMS

	Name	Location	Number	First	Last	Topic
1	E. I. duPont de Nemours & Comp.	DE, NJ	201	27.06.45	30.07.47	Chemicals
2	American Viscose Corp.	DE, VA, WV	152	27.06.45	06.09.45	Chemicals
3	Westinghouse Electric Corp.	NJ, NY, PA	102	01.06.45	14.11.45	M. Engineering
4	The American Enka Corp.	NC	93	27.06.45	06.09.45	Chemicals
5	American Lumber & Treating Comp.	IL	90	04.06.45	05.10.45	Timber
6	The Dow Chemical Comp.	MI	77	09.05.45	12.01.46	Chemicals
7	Newport News Shipbuilding	VA	74	24.06.45	02.08.45	Armaments
8	American Cyanamid Comp.	CT, NJ, NY	71	09.05.45	31.08.46	Chemicals
9	Allis-Chalmers Comp.	IL, WI	64	01.06.45	21.11.45	M. Engineering
10	Standard Oil Comp.	LA, NJ	62	25.03.45	30.09.46	Chemicals

Notes: This table shows the ten U.S. firms which have conducted the most investigations in Germany. U.S. firms have been merged to investigation reports based on the investigator list. 'Location' gives the states where the firms' investigators are based at. 'Number' gives the total number of investigations. The start date of the first investigation is given in 'First' and the finishing date of the last investigation is given in 'Last'. The industry is given as the category from the topic list which was most often assigned to a report written by an employee of each firm. The full name of the U.S. firm at position seven is Newport News Shipbuilding & Dry Dock Company. 'M. Engineering' is Mechanical Engineering and 'Timber' is Timber, Pulp and Paper.

and paper industry, and one on the armaments industry. All ten firms started their investigations early in mid-1945 and mostly finished in the same year. Only one of the ten firms was still conducting investigations in 1947. In contrast, three firms finished their investigations within just three months.

4.5 Discussion and Conclusion

In this paper, we present the first systematic and quantitative analysis of the intellectual reparations program faced by the German industry in the aftermath of WWII. We assemble a novel database covering more than 90% of the reports on British and U.S. investigations of German firms. We then present a series of facts on the regional, industrial, and technological breadth of this unprecedented program. Our findings contribute to a broader literature on both the German economic miracle and the emergence of the United States as the world's leading economic power after WWII.

A further contribution of our newly assembled database lies in observing which U.S. firms were involved in the investigations. By linking the investigated German firms to the investigating U.S. firms, we can observe and analyse the resulting knowledge transfers. In further work, we can examine the effects of access to German technological know-how on industrial performance and innovation in the United States. For example, using balance sheet data on U.S. firms, we can study the effects of the investigations on the performance of participating firms. Alternatively, using patent data we can investigate the effects of the knowledge transfers on follow-on innovation in the United States and thereby examine the long-run effect of the intellectual reparations on U.S. innovation.

Appendix to Chapter 4

This appendix presents details on data collection and additional results:

- Section 4.A provides details on the sources of the historical data.
- Section 4.B provides additional figures.
- Section 4.C provides additional tables.

4.A Sources of Historical Data

Reports: We digitise the whole stock of FIAT, BIOS, and CIOS final reports from the archives of the Deutsche Museum in Munich and the Imperial War Museum in London.

Investigator List: We source and digitise the information on the names of the investigators and their employers from a report by the Department of Commerce from 1947 ('Reports resulting from the investigation of German technology, 1945-1946, and index of personnel'). The document is available at the library of UC Berkeley (call number: T26.G3.U5.).

Topic List: We source and digitise the information on the topics of each of the FIAT, BIOS, and CIOS published final reports from the 'Reports on German and Japanese Industry - Classified Lists No. 18-20' by the H.M. Stationary Office from 1948 to 1951. The documents are available in the Rare Books section of the University of Cambridge library (catalogue number: OPR.2.67).

Target List: We source and digitise the information on the target list from the 'Economic Survey' and 'Zone handbooks' of Germany by the Foreign Office and the Ministry of Economic Warfare from 1945. The documents are available at the Imperial War Museum in London and offer a subject index and complete index of firms (catalogue number: LBY 28768-13).

Sources for Figures:

- Figure 4.1: Deutsches Museum, München, Archiv, CD 91638
- Figure 4.3a: Deutsches Museum, München, Archiv, CD 91634
- Figure 4.3b: Deutsches Museum, München, Archiv, CD 91640
- Figure 4.3c: Deutsches Museum, München, Archiv, CD 91639
- Figure 4.4a: Deutsches Museum, München, Archiv, CD 91635
- Figure 4.4b: Deutsches Museum, München, Archiv, CD 91636
- Figure 4.4c: Deutsches Museum, München, Archiv, CD 91637
- Figure 4.B.1: Deutsches Museum, München, Archiv, CD 91633

4.B Supplementary Figures

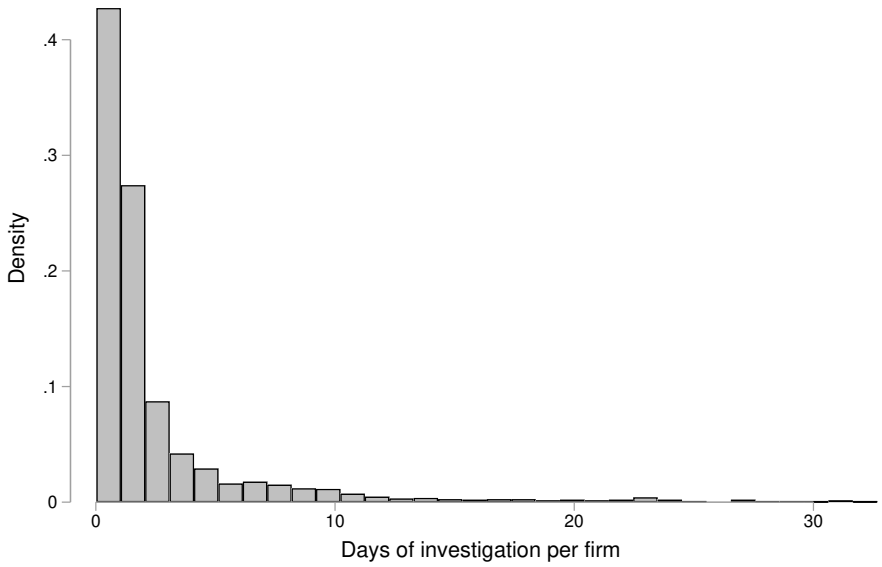
Figure 4.B.1: BIOS INVESTIGATION TEAM, SUMMER 1945

Notes: This figure shows members of the investigation team from BIOS report 300 on the German automotive industry. Investigations took place from 24 June to 1 October 1945. The report with 130 pages includes 78 investigations at firms such as BMW, Daimler Benz, M.A.N., Maybach, Opel, Porsche, Volkswagen, and Zahnradfabrik. 24 investigators and 7 liaison officers split into seven teams. The 24 investigators all came from private British industry such as Rolls Royce and Vauxhall. The names and private-sector employers of the members from the depicted 'Team C' are V.W. Pilkington (Leyland Motors Ltd.), F. Grimshaw (Leyland Motors Ltd.), G.J. Rackham (Associated Equipment Co. Ltd.), and Captain T.H.P. Cain. All wore military uniforms. The picture has been provided by the Deutsche Museum in Munich - for details see Section 4.A.

Figure 4.B.2: WORD CLOUD: TITLES OF REPORTS

Notes: This figure shows a word cloud of the words which have most often been used in the title of BIOS, CIOS, and FIAT reports. Larger font size implies that a word has been used more often.

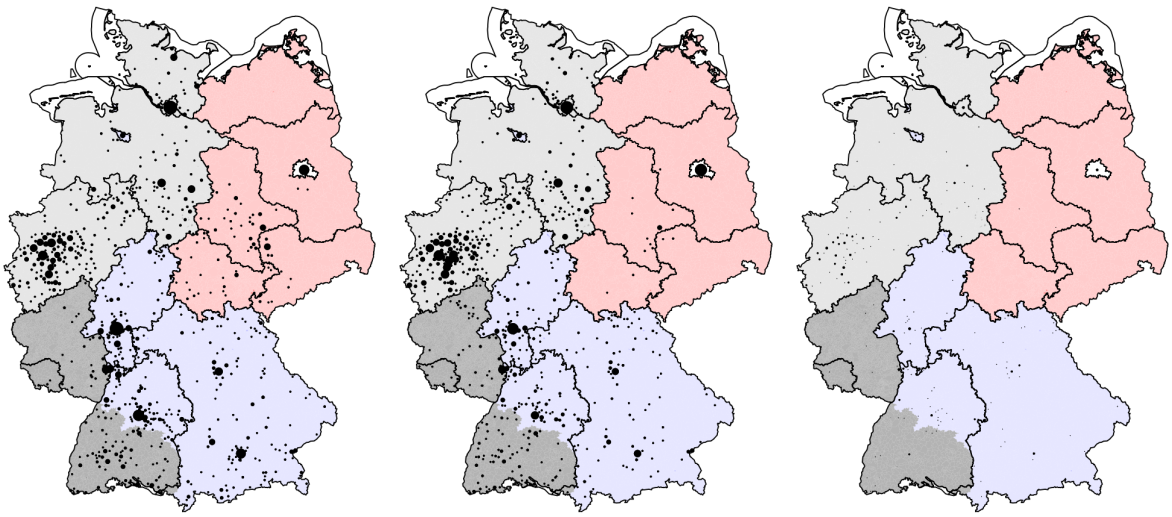
Figure 4.B.3: TIME SPENT ON INVESTIGATIONS



Notes: This figure shows the distribution of days that were spent investigating an individual firm. The date of investigation is available for close to 50% of investigations, implying that only a subset of all investigations can be employed for this analysis.

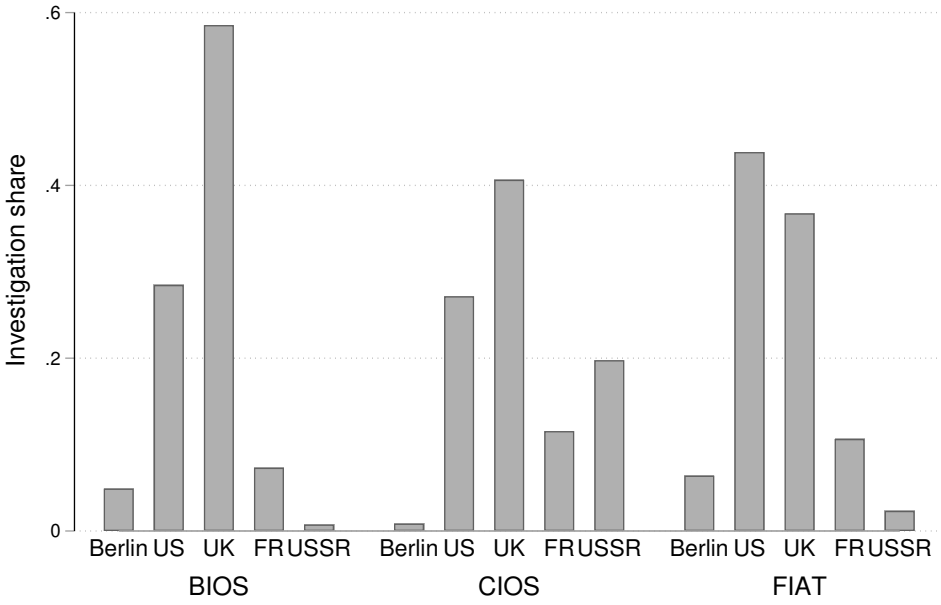
Figure 4.B.4: LOCATION OF INVESTIGATIONS BY YEAR

(a) 1945 **(b) 1946** **(c) 1947**



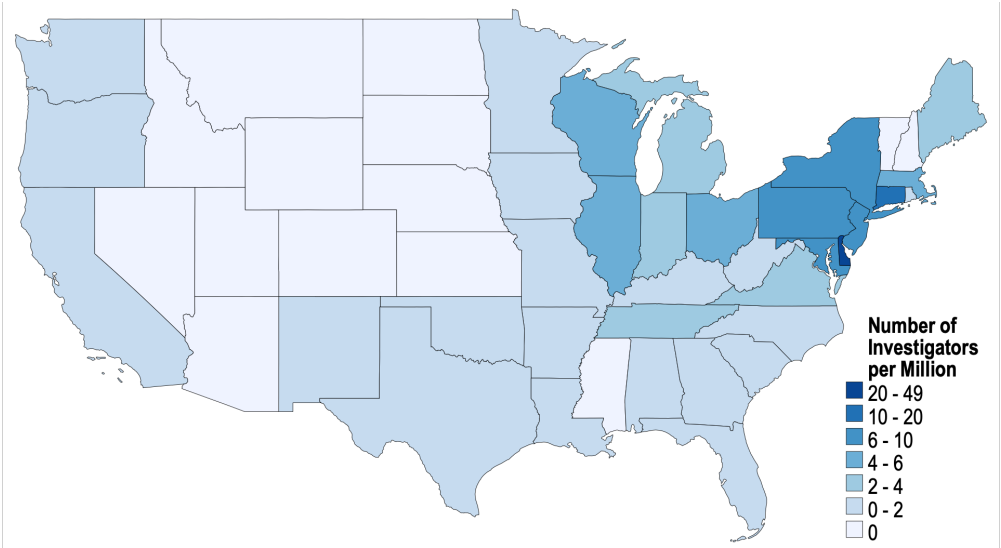
Notes: This figure shows the location of investigations for each year of the program. The size of the black dots is proportional to the number of investigations in each municipality. The scale is similar for all three sub-figures. The date of investigation is available for close to 50% of investigations, implying that only a subset of the investigations from Figure 4.11 is shown here. Germany was divided into four occupation zones: Blue is the U.S. zone, light grey is the British zone, dark grey is the French zone, and red is the Soviet zone. The zone borders are based on a map by Kunz (2004). Investigations are extracted from the reports of BIOS, CIOS, and FIAT which include British and U.S. investigators, but not French or Soviet ones. The maps are based on German borders as well as the municipality and state structure from 2023.

Figure 4.B.5: LOCATION OF INVESTIGATIONS BY ZONE



Notes: This figure shows the share of investigations occurring in each zone and Berlin separately for BIOS, CIOS, and FIAT. The assigning of municipalities to occupational zones is based on a map by Kunz (2004).

Figure 4.B.6: NUMBER OF INVESTIGATORS PER INHABITANTS BY STATE



Notes: This figure shows the number of investigators from each U.S. state per one million inhabitants. The information on the location of investigators is drawn from the investigator list. The number of inhabitants is for the year 1940 and it is taken from U.S. Bureau of the Census (1950).

4.C Supplementary Tables

Table 4.C.1: DETAILS ON EXEMPLARY TRAVEL ROUTE — CIOS REPORT XXXIII-50

Number	Location	Firm	Date
1	Heinsberg	Vereinigte Glanzstoff-Fabriken AG	16.8.1945
2	Krefeld	Joh. Kleinewefers Söhne	16.8.1945
2	Krefeld	Rheinische Kunstseide AG	15.8.1945
2	Krefeld	Rheinische Zellwolle AG	15.8.1945
3	Hilden	Kampf & Spindler	14.8.1945
4	Dormagen	I.G. Farbenindustrie AG	10.8.1945-16.8.1945
5	Köln	Glanzstoff-Courtaulds	15.8.1945
6	Leverkusen	I.G. Farbenindustrie AG	10.8.1945-11.8.1945
7	Wuppertal	I.G. Farbenindustrie AG	13.8.1945
8	Siegburg	Rheinische Zellwolle AG	17.8.1945; 21.8.1945
9	Bielefeld	Hermann Windel GmbH	9.8.1945
9	Bielefeld	Seydel Maschinenfabrik GmbH	9.8.1945
10	Bad Oeynhausen	C.A. Delius & Söhne	7.8.1945
11	Hannover	Döhren Wollwäscherei und -kämmerei	8.8.1945
11	Hannover	Continental Gummiwerke	10.8.1945-11.8.1945
12	Berlin	Spinnfaser AG	30.8.1945
12	Berlin	KWI für Physikalische Chemie	29.8.1945
13	Südliches Anhalt	Eilfeld AG	2.7.1945
14	Bitterfeld-Wolfen	I.G. Farbenindustrie AG	27.6.1945; 28.8.1945-1.7.1945
15	Leuna	I.G. Farbenindustrie AG	2.7.1945; 25.8.1945
16	Elsterberg	Vereinigte Glanzstoff-Fabriken AG	2.7.1945
17	Plauen	Sächsische Zellwolle AG	2.7.1945
18	Schwarza	Thüringische Zellwolle AG	2.7.1945-3.7.1945
19	Kassel	Spinnfaser AG	3.7.1945
20	Rosenthal	I.G. Farbenindustrie AG	15.8.1945
21	Fulda	Val. Mehler Segeltuchweberei AG	22.8.1945
22	Wiesbaden	I.G. Farbenindustrie AG (Kalle & Co.)	4.8.1945
23	Kelsterbach	Vereinigte Glanzstoff-Fabriken AG	5.9.1945
24	Frankfurt am Main	IG Farben	3.8.1945-4.8.1945; 17.8.1945
25	Hanau	Condux-Werk (Herbert A. Merges KG)	25.9.1945
25	Hanau	W.C. Heraeus GmbH	6.9.1945
26	Obernburg a.Main	Vereinigte Glanzstoff-Fabriken AG	1.9.1945-2.9.1945
27	Ludwigshafen	I.G. Farbenindustrie AG	4.8.1945; 21.8.1945-25.8.1945
28	Stuttgart	Werner & Pfleiderer	30.8.1945
28	Stuttgart	Maschinenfabrik Gg. Kiefer GmbH	30.8.1945
29	Esslingen am Neckar	Fritz Müller Pressenfabrik	29.8.1945
30	Denkendorf	Zellwoll-Lehrspinnerei	31.8.1945
31	Rottweil	I.G. Farbenindustrie AG	28.8.1945
32	Freiburg	Rhodiaseta AG	20.9.1945
33	Waldshut-Tiengen	Lonza-Werk	19.8.1945
34	Bad Säckingen	Lonzana AG für Acetatprodukte	19.8.1945
35	Bobingen	I.G. Farbenindustrie AG	26.7.1945-30.7.1945
36	Augsburg	Augsburger Kammgarn-Spinnerei	27.7.1945
37	Kelheim	Süddeutsche Zellwolle AG	28.7.1945
38	München	Agfa Camerawerke (I.G. Farben)	8.7.1945-9.7.1945; 28.7.1945
39	Burgkirchen a.d.Alz	I.G. Farbenindustrie AG	10.7.1945-11.7.1945; 13.7.1945
40	Burghausen	Dr. Alexander Wacker GmbH	2.7.1945; 12.7.1945; 17.7.1945; 1.8.1945-3.8.1945

Notes: This table lists all locations, firms, and dates of the investigations shown in Figure 4.2.

Table 4.C.2: COMPARISON: OUR DATASET VS. UNIVERSE OF REPORTS

		CIOS	BIOS	FIAT	Total
Published	In Dataset	573	1,463	687	2,723
	Maximum	574	1,465	687	2,726
	Share (in %)	100	100	100	100
Not Published	In Dataset	401	293	456	1,150
	Maximum	532	436	653	1,621
	Share (in %)	75	67	70	71
Total	In Dataset	974	1,756	1,143	3,873
	Maximum	1,106	1,901	1,340	4,347
	Share (in %)	88	92	85	89

Notes: This table compares the number of reports in our dataset with the maximum number of potentially written reports. The maximum number is taken from the report with the highest numerical value. The calculated share is a lower bound. For example, a report may be missing if two reports were merged and published under one number only. The information whether a report has been published was taken from the topic list.

Table 4.C.3: TOP 10 STATES BY NUMBER OF INVESTIGATORS

	State	Investigators	Inhabitants
1	New York	102	13,479,142
2	Pennsylvania	61	9,900,180
3	Washington D.C.	42	663,091
4	Ohio	37	6,907,612
5	Illinois	33	7,897,241
6	New Jersey	30	4,160,165
7	Connecticut	23	1,709,242
8	Massachusetts	19	4,316,721
8	Michigan	19	5,256,106
10	Wisconsin	18	3,137,587

Notes: This table shows the top ten U.S. states by the number of investigators sent to Germany. The information on the location of investigators is drawn from the investigator list. The number of inhabitants is for the year 1940 and it is taken from U.S. Bureau of the Census (1950).

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