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Offshore Wealth, Onshore Distrust: Evidence from the Panama Papers Scandal

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Abstract

Does information revealing that political elites act against the public interest erode institutional trust and state legitimacy? We examine this question using the Panama Papers, a major disclosure of offshore financial activities involving political leaders and senior public officials worldwide. We construct an original country-level database measuring the timing and intensity of media coverage implicating domestic political figures and combine it with survey data from the Life in Transition Survey, the British Election Study, and Eurobarometer. Across countries, samples, and empirical strategies that identify distinct margins of the scandal, we find that the revelations increased the political salience of taxation and reduced trust in presidents, governments, and parliaments. The reputational consequences thus extended beyond the individuals directly implicated to the institutions through which political authority is exercised, consistent with an erosion of perceived state legitimacy. We also find that the disclosures strengthened support for democracy, suggesting that citizens valued the capacity of democratic institutions and investigative journalism to expose elite misconduct. Yet where domestic political involvement was especially salient, satisfaction with the functioning of democracy declined.

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> *"What has also broken out of the vaults of offshore legal specialists Mossack Fonseca is one over-riding sense. The sense that normal rules do not apply to the global elite. In a new gilded age, taxes would—once again—appear to be for the little people."*
>
> —— *The Guardian*, Editorial, 4 April 2016

1 Introduction

Why do citizens accept the authority of the state and comply with its decisions? A central condition of state legitimacy is the belief that public power is exercised according to generally accepted rules and in pursuit of the public interest (e.g., Beetham, 2013). Citizens may disagree with particular policies or political leaders while continuing to regard state institutions as legitimate. However, when citizens receive evidence that those who make and enforce the rules do not consider themselves bound by them, support for state institutions may become more fragile (Besley, 2020). Revelations of misconduct by political elites can therefore affect not only evaluations of the individuals directly involved but also trust in the governments, parliaments, and other institutions through which they exercise public authority (e.g., Ares and Hernández, 2017; Solé-Ollé and Sorribas-Navarro, 2018; Rivera et al., 2026).

Tax scandals provide a particularly consequential test of this relationship. Taxation lies at the core of the social contract: citizens surrender part of their private resources in exchange for public goods, protection, and the impartial application of common rules. The willingness to sustain this exchange depends partly on reciprocity and procedural fairness—the expectation that fiscal obligations apply to political elites as well as ordinary citizens (Luttmer and Singhal, 2014; Besley, 2020; Besley and Dray, 2024). Information that political leaders have used offshore arrangements to minimize their tax liabilities, conceal assets, or circumvent domestic scrutiny may undermine this expectation. Even when particular arrangements are formally legal, they may signal that influential individuals can exploit opportunities unavailable to most citizens. Such revelations can consequently weaken the belief that the state represents a common enterprise governed by equal rules.

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The effect of these revelations on state legitimacy is nevertheless theoretically ambiguous. On the one hand, information about elite misconduct can lead citizens to revise their beliefs about the integrity of political leaders and the institutions they control. The scandal may indicate that governments and parliaments are populated by individuals who place private interests above the public interest, that tax obligations are enforced unequally, or that existing institutions are unable or unwilling to hold powerful actors accountable. Under this interpretation, exposure should reduce institutional trust and may ultimately weaken support for the political system itself.

On the other hand, the public disclosure of misconduct may demonstrate that democratic accountability mechanisms are functioning (Ferraz and Finan, 2008). Investigative journalism, political competition, parliamentary scrutiny, and judicial investigations make it possible to uncover conduct that would otherwise remain hidden (e.g., Besley and Burgess, 2002; Snyder and Strömberg, 2010; Avis et al., 2018). Citizens may therefore distinguish between the misconduct revealed by the scandal and the democratic institutions that permitted its discovery. Information about elite wrongdoing could reduce support for particular politicians or governments while leaving support for democracy unchanged—or even strengthening it—because democracy provides mechanisms through which misconduct can be exposed and sanctioned. The legitimacy consequences of political scandals thus depend on whether citizens primarily interpret them as evidence of institutional failure or as evidence that democratic monitoring is working.

Despite extensive commentary and qualitative evidence suggesting that major political scandals damage public trust, systematic causal evidence remains limited. Identifying these effects is difficult because exposure to political misconduct is not randomly assigned. Scandals tend to emerge in countries with particular political institutions, media systems, or levels of pre-existing distrust. Moreover, changes in institutional trust around a scandal may reflect contemporaneous political and economic developments rather than the revelation itself. Existing research has documented how disclosure of corruption affects electoral behavior (e.g., Ferraz and Finan, 2025; Rivera et al., 2026) or trust in political actors (e.g., Ares and Hernández, 2017; Solé-Ollé and Sorribas-Navarro, 2018). Much less is known, however, about whether a global disclosure of elite fiscal misconduct generates reputational spillovers from the individuals implicated to the multiple state institutions through which political authority is exercised. Or whether citizens distinguish their evaluation of these institutions from their support for democracy as a political system.

In this paper, we systematically study this question by examining the Panama Papers, one of the most prominent global revelations of offshore financial activity. Publicly released on April 3, 2016, the leak comprised millions of documents obtained from the Panamanian law firm Mossack Fonseca. The documents revealed offshore entities connected to businesspeople, public officials, and political leaders across more than 200 countries and territories. They included cases involving legal tax avoidance as well as allegations of tax evasion, asset concealment, corruption, and money laundering. The scale and international coordination of the investigation transformed offshore finance from a specialized policy issue into a highly visible matter of public debate.

The Panama Papers offer an unusually informative setting for studying state legitimacy. The leak constituted a common global information shock, but its domestic political relevance varied sharply across countries and over time. In some countries, prominent national political figures were implicated immediately. In others, the involvement of domestic politicians emerged only after journalists had examined the documents for several days or weeks. Some countries experienced extensive and persistent reporting about national leaders, whereas coverage elsewhere remained focused on foreign individuals or on the global scandal more generally. This temporal and cross-country variation allows us to separate exposure to information about

domestic political elites from the general international release of the documents.

To measure this exposure, we construct a new country-day database of media coverage surrounding the leak. We collect more than 5,000 newspaper and news-agency articles published around the disclosure and use a Large Language Model (LLM), supplemented by manual validation, to identify articles concerning the Panama Papers. We then determine whether each article names a domestic political leader or high-level public official implicated in the scandal. This procedure allows us to identify both the first date on which citizens in a country received information about domestic political figures and the intensity with which these revelations were subsequently covered by the national media.

We combine these data with three surveys that were, fortuitously, administered around the time of the leak. The surveys differ in their geographical coverage, frequency, and sampling structure. We therefore employ three complementary quasi-experimental designs, each relying on a different source of variation and on the identification of complementary margins of the scandal.

Our main analysis uses the third wave of the Life in Transition Survey (LiTS III), which was being administered in 14 countries in Eastern Europe, the South Caucasus, and Central Asia when the Panama Papers were released. The global disclosure on April 3 constituted a common international information shock, but information implicating domestic political figures appeared at different dates across countries. We exploit this staggered domestication of the scandal using the dynamic difference-in-differences design developed by Callaway and Sant'Anna (2021). Respondents interviewed after the first domestic revelation are compared with respondents interviewed at the same point in time in countries where no domestic political figure had yet been implicated. Importantly, the latter respondents need not have been unaware of the Panama Papers: they may already have been exposed to the global leak. The estimand therefore captures the incremental effect associated with the scandal acquiring domestic political relevance, rather than the effect of exposure to the Panama Papers per se.

Second, we examine the United Kingdom using longitudinal data from the British Election Study (BES). The British case is especially informative because the scandal directly involved the family finances of then-Prime Minister David Cameron and received extensive national coverage. The panel structure of the survey allows us to follow the same respondents before and after the disclosure and to absorb time-invariant differences in their political attitudes through individual fixed effects. We examine changes in support for Cameron and the Conservative Party, trust in Members of Parliament, and approval of the national government. This analysis provides within-person evidence from a consolidated democracy in which the identity of the most politically salient individual implicated in the scandal was particularly clear.

Third, we use repeated cross-sectional data from Eurobarometer for 23 European countries. We combine the timing of the leak with cross-country variation in the share of Panama Papers coverage devoted to domestic political figures. Because survey-wave fixed effects absorb changes common to all countries after the disclosure, this design does not estimate the average effect of the Panama Papers. Instead, it identifies whether post-disclosure changes differ systematically with the domestic political salience of the scandal. We complement this intensive-margin analysis with a separate comparison of Galicia (Spain) and Norte (Portugal). These neighboring regions share substantial historical, cultural, and linguistic ties but differed sharply in their exposure to the scandal, providing a geographically and culturally narrower comparison.

Before examining institutional trust, we show that the leak changed the salience of taxation in public debate. In both the British Election Study and Eurobarometer, respondents became more likely to identify taxation or tax avoidance as an important issue in the immediate aftermath of the scandal. This result supports the interpretation that the Panama Papers were not merely a general political news event. They focused public attention on whether political elites were contributing fairly to the fiscal system and therefore on a central dimension of the

social contract.

We then show, using three empirical settings that identify different margins of exposure to the Panama Papers, that domestic revelations reduced trust in state institutions. In the Life in Transition Survey, the first domestic political revelation is associated with a sharp decline in institutional trust relative to countries where such a revelation had not yet appeared. The probability of reporting trust in the president falls by 23.8 percentage points, trust in the government by 34.4 points, and trust in Parliament by 23.3 points. Because respondents in the comparison countries may already have been exposed to the global Panama Papers disclosure, these estimates capture the incremental effect of the scandal acquiring domestic political relevance.

The British evidence reveals a similar, though different, type of evidence. Support for David Cameron decreases by 7.7 percentage points, and support for the Conservative Party falls by 4.1 percentage points. These politically targeted responses are accompanied by a 12.1 percentage-point decline in trust in Members of Parliament and a 5.4 percentage-point reduction in government approval. Because this design has no contemporaneous untreated group, these estimates represent within-person pre/post changes rather than the same treatment effect identified in LiTS.

Eurobarometer identifies yet another margin. Post-disclosure trust declines more in countries where a larger share of Panama Papers coverage concerns domestic political figures: a one-standard-deviation increase in the domestic-coverage share is associated with an additional decline of approximately 0.8 percentage points in trust in government and 0.6 points in trust in Parliament. A separate regional difference-in-differences comparison shows substantially more negative post-disclosure changes in Galicia than in neighboring Norte. We interpret the Galicia–Norte estimates as a distinct regional contrast rather than as larger estimates of the same parameter identified in the cross-country Eurobarometer specification.

Complementary results reveal an important distinction between trust in incumbent institutions and support for democracy. In LiTS, the incremental domestic-revelation effect increases support for democracy as the preferred political system, while the BES documents an increase in satisfaction with democracy within individuals after the UK disclosure. These patterns are consistent with a monitoring interpretation, whereby the public revelation of misconduct demonstrates the capacity of democratic institutions and a free press to uncover elite behavior. Eurobarometer identifies a different margin: satisfaction with democracy declines more after the disclosure in countries where domestic political involvement receives a larger share of Panama Papers coverage. The Galicia–Norte comparison yields a similar differential pattern. These findings are consistent with the possibility that particularly salient domestic involvement shifts attention from the accountability benefits of disclosure toward the failure of institutions to prevent the underlying behavior.

Finally, we ask whether the reputational consequences of the scandal depend on the strength of the democratic institutions already in place. Stronger democratic institutions may help contain the spillover from misconduct by individual political elites to the legitimacy of the institutional system as a whole. Using pre-scandal measures of democratic quality from V-Dem, we find that the decline in institutional trust following the first domestic political revelation in the LiTS sample is generally attenuated in countries with stronger democratic institutions. The pattern is clearest for trust in government and the president, while it is less uniform for Parliament, and is corroborated by complementary specifications that retain the continuous variation in democratic quality. We interpret this heterogeneity as evidence of an attitudinal dimension of democratic resilience: stronger democratic institutions may not only constrain political actors, but also make institutional legitimacy more resilient to revelations of misconduct by the individuals who temporarily occupy them.

The paper contributes to four related bodies of research. First, it contributes to the political

economy of state legitimacy and the social contract. Existing work emphasizes how institutional performance, public-good provision, reciprocity, and democratic experience shape citizens' willingness to trust and support the state (Kuziemko et al., 2015; Solé-Ollé and Sorribas-Navarro, 2018; Acemoglu et al., 2020; Besley, 2020; Claassen, 2020; Acemoglu et al., 2025; Rivera et al., 2026). We show that information about how political elites comply with the fiscal rules they impose on others is another important determinant of institutional trust. Even formally legal offshore arrangements may signal unequal access to financial secrecy. Our results therefore indicate that the perceived legitimacy of the state depends not only on what the state provides but also on whether its representatives appear to be subject to the same obligations as ordinary citizens.

Second, we contribute to the literature on political information and accountability (e.g., Besley and Burgess, 2002; Besley and Prat, 2006; Snyder and Strömberg, 2010; Enikolopov et al., 2011). Previous studies show that information about corruption can affect voting, electoral selection, and politicians' careers (Ferraz and Finan, 2008; Avis et al., 2018; Chong et al., 2015; Ajzenman, 2021). We examine a broader consequence: whether information about elite misconduct spills over from implicated individuals to governments, parliaments, and the democratic system. Our results show that political scandals can produce such spillovers. Citizens may interpret exposure as evidence of institutional failure or as evidence that investigative journalism and democratic scrutiny work. We show that the intensity of the scandal, as well as the quality of democratic institutions, shapes the tension between the accountability benefits of disclosure and the institutional failures it reveals.

In this respect, our paper also speaks to a body of research on democratic resilience by identifying a previously understudied attitudinal dimension of institutional resilience. Existing work primarily examines why some democratic systems withstand autocratization and institutional erosion, emphasizing the role of judicial constraints, accountability institutions, and inherited institutional strength (Persson and Tabellini, 2009; Fearon, 2011; Merkel and Lührmann, 2021; Boese et al., 2021; Laebens and Lührmann, 2021). We instead examine whether these institutional characteristics also shape how citizens respond to a sudden shock to political legitimacy. We find that the decline in institutional trust following revelations of elite misconduct is substantially attenuated in countries with more consolidated democratic institutions. This suggests that strong democratic institutions not only constrain political actors; they may also insulate the legitimacy of the institutional system from the misconduct of the individuals who temporarily occupy it.

Finally, we contribute to research on offshore finance and, specifically, to research examining the political consequences of the Panama Papers. Ait Bihi Ouali (2020) and Helgason and Mérola (2022) study the impact on voting and redistribution preferences. Guriev et al. (2021) show that Panama Papers revelations increase perceptions of corruption in the United States more where mobile broadband is available. We distinguish the global disclosure from subsequent revelations implicating domestic politicians. Our database on local articles identifies both the first domestic revelation and the share of coverage devoted to national political figures. Combining these measures with staggered exposure in LiTS, Eurobarometer exposure comparisons, and complementary British within-person evidence allows us to examine institutional trust across different political environments. The convergence of the results across these settings indicates that revelations of unequal fiscal behavior can erode institutional trust, while the effects on democratic support depend on the scale and domestic concentration of the misconduct revealed.

The remainder of the paper is organized as follows. Section 2 provides the context and outlines the theory. Section 3 describes the data and Section 4 the empirical strategies. In Section 5, we present our main results. Supplementary materials are reported in the Online Appendix.

2 The Panama Papers scandal: background and theory

2.1 What are the Panama Papers

The Panama Papers are the name given to 11.5 million documents—approximately 2.6 terabytes of data—leaked in 2016 from Mossack Fonseca, a Panamanian law firm specializing in the creation and administration of offshore entities.[1] Some of the documents date back to the 1970s and contain information on more than 214,000 offshore companies, trusts, foundations, and other legal entities connected to clients in over 200 countries and territories (Obermaier and Obermayer, 2017).

Offshore financial arrangements are not inherently illegal and may serve legitimate purposes, including international investment, estate planning, and the management of assets across jurisdictions. Their opacity, however, can also facilitate aggressive tax avoidance, tax evasion, asset concealment, corruption, and money laundering. The Panama Papers revealed the extent to which political leaders, senior public officials, and individuals closely connected to them had access to these arrangements. Their political significance therefore lay not only in the disclosure of potentially unlawful conduct, but also in the evidence they provided about elites' ability to use opaque financial structures largely inaccessible to ordinary citizens.

Although earlier investigations had exposed offshore companies, secret tax agreements, and concealed bank accounts, the scale, international reach, and public visibility of the Panama Papers were unprecedented.[2] By connecting prominent political figures across countries and ideological affiliations to similar offshore practices, the leak transformed offshore finance from a specialized legal and financial issue into a prominent public question concerning fiscal inequality, political privilege, and elite accountability.

2.2 Disclosure and political consequences

The Panama Papers became public on April 3, 2016, following an investigation coordinated by the International Consortium of Investigative Journalists (ICIJ). More than a year earlier, an anonymous source had provided the documents to the German newspaper *Süddeutsche Zeitung*, which shared them with a network of media organizations around the world. After jointly examining the files, participating outlets began publishing their findings simultaneously on April 3.

The initial release generated exceptional international attention. Searches relating to the Panama Papers, tax avoidance, and tax havens increased sharply following the disclosure, as shown in Figure 1. The leak did not, however, produce a uniform information shock across countries. The first reports established the global scale of the scandal and implicated several internationally prominent figures, but the involvement of domestic politicians emerged at different times. Because of the volume and complexity of the documents, journalists continued to identify and report country-specific cases throughout April and May 2016. Citizens in some countries therefore learned almost immediately that national political figures were implicated, whereas comparable information appeared only days or weeks later elsewhere.

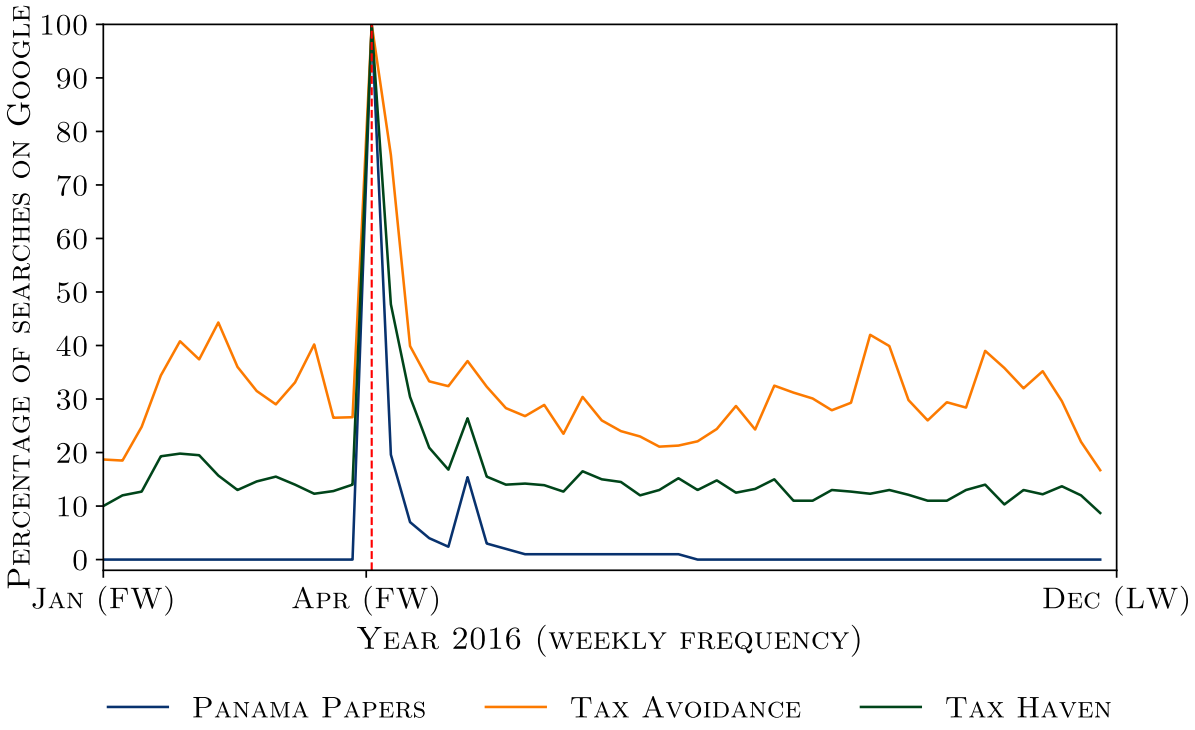
The revelations also generated immediate political consequences. During what *The Guardian* later called "*the five days that shook the world*,"[3] they triggered official investigations, par-

[1] Matthew D'Ancona described the leak as "*a hurricane of data that has blown around the globe.*" See *The New York Times*, "*The Prime Minister's Personal Finances*," 20 April 2016.

[2] Earlier investigations coordinated by the International Consortium of Investigative Journalists included Offshore Leaks in 2013, which disclosed offshore companies and trusts worldwide; LuxLeaks in 2014, which revealed confidential tax arrangements granted by Luxembourg to multinational firms; and Swiss Leaks in 2015, which exposed secret accounts held at HSBC's Swiss private bank.

[3] *The Guardian*, "*The Guardian view on the Panama Papers: five days that shook the world*," Editorial, 24

Figure 1: Global Google searches during 2016



Notes. The figure reports the weekly percentage of searches on Google for the keywords "Panama Papers," "Tax avoidance," and "Tax haven" during 2016. The red vertical dashed line highlights week 14 of 2016 (April 4-10), the week of the global disclosure of the Panama Papers. FW stands for first week; LW for last week.

liamentary inquiries, public demonstrations, and intense media scrutiny. Several implicated political leaders resigned or were subsequently removed from office, while others faced votes of no confidence, electoral losses, or substantial reputational damage. These consequences were visible in countries including Iceland, Pakistan, the United Kingdom, and Spain, as well as in several Latin American and European democracies.

In this respect, the leak demonstrated the *monitoring capacity of democracy*. Investigative journalism and international cooperation among media organizations uncovered activities that had remained concealed for years, while opposition parties, parliaments, courts, civil society, and voters helped translate these revelations into political accountability. The disclosure therefore showed that open political systems could detect and in some cases sanction elite conduct.

2.3 Implications for state legitimacy and democracy

At the same time, the Panama Papers exposed a potential failure of democratic institutions to prevent or constrain these activities. The documents showed that political leaders and senior public officials had been able to use opaque offshore structures for years, often while occupying positions in institutions responsible for taxation, financial transparency, and the enforcement of public rules. Even when these arrangements were formally legal, their persistence could indicate that powerful actors had access to secrecy, regulatory gaps, and financial opportunities unavailable to ordinary citizens.

This evidence had direct implications for the fiscal social contract. Taxation requires citizens to surrender private resources for collective purposes, and acceptance of this obligation depends

June 2016.

partly on the belief that fiscal rules are applied fairly and bind political elites as well as ordinary citizens (Luttmer and Singhal, 2014; Besley, 2020). The leak could therefore weaken fiscal reciprocity by raising doubts about whether those who designed and enforced public rules were themselves willing to respect their underlying principles. As an editorial published by *The Guardian* observed, the disclosures created the impression that "*normal rules do not apply to the global elite. In a new gilded age, taxes would—once again—appear to be for the little people.*"[4]

The reputational consequences of the scandal could consequently extend beyond the individuals directly implicated. Citizens might interpret the revelations as information about the integrity of political elites and about the capacity or willingness of governments and parliaments to uphold common rules. Exposure to the scandal should therefore reduce trust in these institutions, providing an observable manifestation of an erosion of state legitimacy.

The implications for democracy as a political system are more ambiguous. Citizens may regard the disclosure and subsequent sanctions as evidence that democratic monitoring works, thereby preserving or strengthening their support for democracy despite declining trust in incumbents. Alternatively, extensive domestic involvement may reveal a systemic failure to prevent elites from circumventing fiscal equality and public accountability. Under this interpretation, the scandal may also weaken citizens' assessments of democracy itself.

These considerations yield two main expectations about the average response. First, exposure to information about domestic political figures involved in offshore financial activities should reduce trust in governments and parliaments. Second, the effect on attitudes toward democracy should depend on the domestic scale of the scandal: disclosure may strengthen support for democracy when it demonstrates effective monitoring, but weaken it when widespread domestic involvement reveals a failure to constrain political elites.

A third implication concerns heterogeneity in the magnitude of the institutional-trust response. The same revelation of elite misconduct may have different reputational consequences depending on the strength of the democratic institutions already in place. Where political competition, checks on executive power, and mechanisms of accountability are stronger, citizens may be more likely to regard individual misconduct as the failure of particular officeholders rather than as evidence of a broader institutional failure. Such institutions may also provide greater confidence that misconduct can be investigated, sanctioned, and corrected, limiting reputational spillovers from political actors to the institutions they occupy. Conversely, where institutional constraints are weaker, revelations of elite misconduct may reinforce the perception that political institutions are unable or unwilling to constrain those who exercise public authority.

3 Data

In this section, we describe our primary data sources, the survey items used to measure institutional trust and related attitudes, and the text analysis we design to assess citizens' exposure to the scandal.

3.1 Survey data on trust and related attitudes

We opportunistically analyze three surveys that were conducted during the global outbreak of the Panama Papers scandal. These surveys cover, during the same period, different countries

[4] *The Guardian*, "*The Guardian view on the Panama Papers: secret riches and public rage*," Editorial, 4 April 2016.

Table 1: List of survey data used in the study

Surveys	Regions	Countries	Individuals
Life in Transition III	Eastern Europe and Central Asia	14	7,580
British Electoral Study 4–8	United Kingdom	1	50,142
Eurobarometer 81–88	EU and candidate countries	23	176,524

Figure 2: Countries covered in LiTS III during the Panama Papers disclosure



in various regions. We summarize the main information in Table 1 and describe each of them below.

3.1.1 Life in Transition Survey

The first survey we examine is the Life in Transition Survey (LiTS), a large, cross-national household and attitudinal survey run jointly by the European Bank for Reconstruction and Development (EBRD) and the World Bank. Its goal is to understand how people in "transition" and emerging economies experience economic and social change—both in their real living conditions and in their beliefs and attitudes. LiTS combines objective household indicators with subjective perceptions in areas that include standards of living, life satisfaction and wellbeing, governance and institutions, and political and social attitudes. This survey therefore allows us to examine respondents' attitudes while holding individual characteristics fixed.

LiTS is a repeated cross-sectional survey that has been conducted four times, in 2006 (LiTS I), 2010 (LiTS II), 2016 (LiTS III), and 2022 (LiTS IV), across Eastern Europe, Central Asia, the Caucasus, parts of the Middle East and North Africa (and sometimes a few comparator countries outside the core region). While spanning the survey in a vast area, LiTS uses common questionnaires across countries, enabling direct cross-country comparisons. The survey is

designed to be nationally representative, randomly sampling about 1,000-1,500 households per country.[5] We opportunistically focus on the third wave (LiTS III), a rolling survey conducted in 33 countries between the second half of 2015 and 2016. In our empirical strategy, which we will explain below, we take advantage of the fact that the leak occurred during the rollout of the survey in 14 countries—countries that are illustrated in the map shown in Figure 2.

The resulting sample comprises 7,580 individuals from Eastern Europe, Central Asia, and the Caucasus (Table 1). The average respondent is 46 years old (Table A1 in the Appendix). Around 40% of respondents are female, with about one child each. Approximately 42% live in a rural area, despite 61% having rural origins. Less than 5% are members of a political party, and the average educational level lies between a secondary and a post-secondary degree (Table A2 in the Appendix).

3.1.2 British Electoral Study

The second survey we analyze is the British Election Study (BES), a longitudinal panel election survey, mainly funded by the Economic and Social Research Council (ESRC). Led by a university consortium, it is one of the longest-running election studies in the world, tracking voting behavior, political attitudes, and social change in the United Kingdom continuously since the 1960s. We specifically use the Internet Panel Study (BESIP), a large longitudinal online panel that has been running since 2014. The same people are re-interviewed across many waves, a circumstance which enables us to study how individuals' political attitudes and vote choices *change over time*.

In our analysis, we follow 50,142 individuals from March 2015 (Wave 4) to June 2016 (Wave 8), a period that includes the scandal date (Table 1). Specifically, Waves 4, 5, and 6 were conducted before the Panama Papers leak; Waves 7 and 8 were surveyed after the leak. As detailed in the BES report, the retention rate is considerable, with 70% of the individuals who responded to Wave 7 also responding to Wave 6 (Fieldhouse et al., 2015).

In terms of characteristics, our BES sample is, as expected, slightly older than the LiTS III sample, with an average age of 51 (Table A3 in the Appendix). The households we examine have, on average, fewer than two members (average household size is 1.36) and an income of approximately £30,000 (Table A4 in the Appendix).

3.1.3 Eurobarometer

Third, we analyze the attitudes of individuals participating in Eurobarometer, a long-running series of public opinion surveys conducted by the European Commission to track attitudes across Europe. It has been running since 1974, making it one of the world's major continuous cross-national survey programs. The Standard Eurobarometer, the primary survey under the Eurobarometer umbrella, is conducted twice per year (generally in spring and autumn) and covers all EU member states, aligned to EU enlargements over time. Many waves also include non-EU countries in the EU neighborhood or in EBRD regions (e.g., the Western Balkans, Turkey, the United Kingdom, etc.). It is based on face-to-face interviews, usually in respondents' homes. Households and individuals are randomly selected within a Primary Sampling Unit (PSU), which is stratified by region and urbanization and selected proportionally to population size.[6]

The Standard Eurobarometer is a repeated cross-sectional survey with approximately 1,000 respondents aged 15 or older per country. Importantly, it includes a core set of repeated

[5] See, for methodological details, https://www.ebrd.com/home/news-and-events/publications/economics/special-reports/life-in-transition-survey-iii.html

[6] See, for methodological details, https://www.gesis.org/en/eurobarometer-data-service/data-and-documentation/standard-special-eb/sampling-and-fieldwork.

Table 2: Main survey items about institutional trust across surveys

Survey	Question item	Answers	Variable
	Panel A: Trust in Parliament		
EB	Please tell me if you tend to trust or tend not to trust the Parliament	(a) Tend to trust it, (b) Tend not to trust it	1 if tend to trust it
LiTS III	To what extent do you trust the Parliament?	1 = Complete distrust, 2 = Some distrust, 3 = Neither trust nor distrust, 4= Some trust, 5 = Complete trust	1 if at least some trust (≥ 4)
BES	How much trust do you have in Members of Parliament in general?	1 = No trust, ..., 7 = A great deal of trust	1 if ≥ 4
	Panel B: Trust in government		
EB	Please tell me if you tend to trust or tend not to trust the government	(a) Tend to trust it, (b) Tend not to trust it	1 if tend to trust it
LiTS III	To what extent do you trust the government?	1 = Complete distrust, 2 = Some distrust, 3 = Neither trust nor distrust, 4= Some trust, 5 = Complete trust	1 if at least some trust (≥ 4)
BES	Do you approve or disapprove of the job that the UK government is doing?	1 = Strongly disapprove, ..., 5 = Strongly approve	1 if ≥ 4

questions on EU membership, institutional trust, the economy, and life satisfaction, enabling the study of changes over time in political and economic attitudes. As the Panama Papers scandal took place in April 2016, we select for our analysis four waves that were conducted immediately after (85, 86, 87, and 88) and three before (82, 83, and 84), spanning from autumn 2013 to autumn 2017.

Overall, our study includes 176,524 individuals across 23 countries (Table 1). The resulting sample shares similarities with the BES one. The average age in the sample is around 50, and 64% have no children. The modal income level belongs to the middle class, which in the EU was between 16 and 43 thousand euros in 2019 (Dolls et al., 2023). Summary statistics for other characteristics are reported in Table A5 in the Appendix.

3.2 Information on institutional trust and related attitudes

Our main outcomes capture two related but conceptually distinct dimensions of political legitimacy. The first is citizens' trust in the institutions through which state authority is exercised. Although institutional trust does not exhaust the broader concept of state legitimacy, it provides an observable indication of whether citizens regard governments and parliaments as credible and deserving of confidence. The second dimension concerns citizens' assessment of democracy, either as a preferred political system or in terms of how it operates in practice.

We first examine trust in Parliament, which is measured in broadly comparable ways across the three surveys (Panel A of Table 2). LiTS III asks respondents, on a scale from 1 (*"Complete distrust"*) to 5 (*"Complete trust"*), "*To what extent do you trust the Parliament?*" BES asks, on a seven-point scale, "*How much trust do you have in Members of Parliament in general?*", with responses ranging from *"No trust"* to *"A great deal of trust."*[7] Eurobarometer instead records whether respondents tend to trust or tend not to trust their national Parliament. To facilitate comparisons and the interpretation of the estimates, we transform the LiTS III and BES scales into binary indicators and code all three measures so that 1 denotes trust in Parliament (column 4 of Table 2). Overall, we observe that around 40% of respondents in our LiTS III sample trust Parliament, and 34% in Eurobarometer. Not surprisingly, this share is higher in the United Kingdom, where 51% of respondents report a high level of trust in Parliament (Table A6).

We similarly examine trust in the national government (Panel B of Table 2). LiTS III measures it using the same five-point scale, whereas Eurobarometer asks whether respondents tend to trust their government. We again construct binary indicators equal to 1 when respondents express trust. Approximately 35% of respondents in our EB sample trust the government; Only 29% do in the LiTS III sample (Table A6). Unfortunately, BES does not include a directly comparable measure of trust in government. We therefore use approval of the government's performance as a related, though conceptually distinct, proxy.[8] In interpreting the results, we consequently distinguish changes in government approval in the British sample from changes in institutional trust in LiTS III and Eurobarometer.

Our measures of democratic assessment also capture two distinct attitudes. LiTS III elicits support for democracy as a political regime by asking respondents to choose among the statements "*Democracy is preferable to any other form of political system*," "*Under some circumstances, an authoritarian government may be preferable to a democratic one*," and "*For people like me, it does not matter whether a government is democratic or authoritarian*." We construct an indicator equal to 1 for respondents who state that democracy is preferable to any other political system—which are approximately 49% of respondents.

By contrast, BES and Eurobarometer measure citizens' evaluations of how democracy functions in their country. BES asks, "*On the whole, how satisfied or dissatisfied are you with the way that democracy works in the UK?*", while Eurobarometer asks whether respondents are very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the way democracy works in their country. We construct indicators equal to 1 for respondents who are satisfied with the functioning of democracy. Approximately half of respondents express satisfaction—50% in Eurobarometer and 52% in BES.[9]

3.3 Quality of democratic institutions

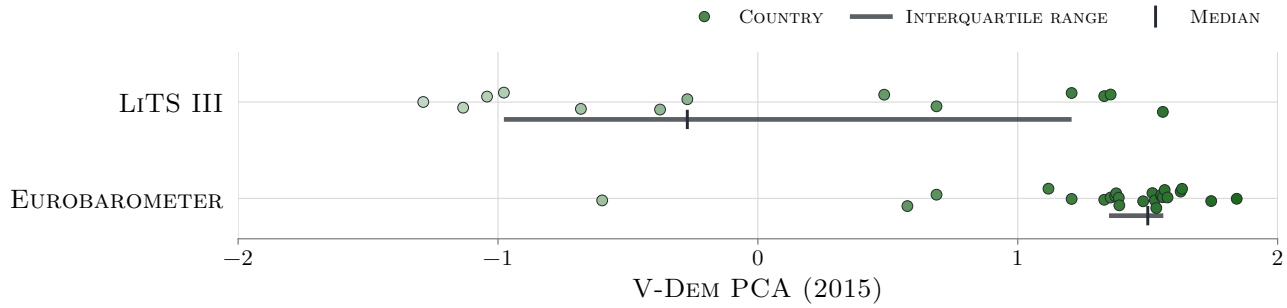
We measure the quality of democratic institutions using data from the Varieties of Democracy (V-Dem) project. V-Dem provides comparable cross-country measures of democracy along five high-level dimensions. The electoral component captures the extent to which political leaders are selected through free and fair elections under broad suffrage. The liberal component measures individual rights, the rule of law, and constraints on executive power. The participatory component captures citizens' direct involvement in political processes, while the deliberative component reflects the extent to which political decisions are based on public reasoning and respectful deliberation. Finally, the egalitarian component captures equality in access to political

[7] This item is not surveyed in Wave 5. We therefore cannot observe trust in Parliament around April and May 2015.

[8] We also note that this item is only surveyed in Waves 4 and 7, leaving us with one period before and one after the disclose of the Panama Papers in the United Kingdom.

[9] Summary statistics for these measures are reported in Online Appendix Table A7.

Figure 3: Distribution of countries according to the V-Dem PCA score



Notes. The scatterplot shows countries from LiTS III and Eurobarometer samples, plotted based on their V-Dem PCA score. Each dot represents a country, with darker shades indicating higher V-Dem scores.

power and the resources necessary for political participation.

We use the five indices measured in 2015, the year preceding the Panama Papers disclosure, from V-Dem version 16. Because these dimensions capture related aspects of democratic institutions, we combine them into a summary measure using principal component analysis (PCA). We estimate the PCA on the correlation matrix of the five indices for the 179 countries for which the required information is available, orienting each component so that higher values correspond to stronger democratic institutions. We retain the first principal component, which captures the largest share of their common variation, and standardize the resulting country score to have mean zero and unit standard deviation in the full cross-country sample. The measure is therefore determined using the worldwide distribution of democratic quality rather than the countries included in any particular survey.

Figure 3 shows the distribution of this measure across the countries covered by LiTS III and Eurobarometer. The contrast between the two samples is substantial. LiTS III spans countries with markedly different levels of democratic quality, generating considerable cross-country variation in the V-Dem score. Eurobarometer countries, by contrast, are concentrated within a considerably narrower portion of the distribution, particularly toward higher levels of democratic quality.[10] This difference is important for our subsequent heterogeneity analysis: LiTS III provides substantially more variation with which to examine whether the institutional consequences of the Panama Papers depend on the pre-existing quality of democratic institutions. We will therefore focus our main analysis of democratic resilience on the LiTS III sample.

We assign the country-level V-Dem score to each survey respondent and recenter it within the relevant empirical sample. We additionally divide respondents into relatively low- and high-democratic-quality groups using the unweighted median of the V-Dem score in the estimation sample, assigning observations at the median to the low-democracy group. In complementary specifications, we retain the continuous V-Dem measure rather than dichotomizing it.

3.4 Media coverage of the scandals

We combine information on individual attitudes with measures of the domestic political salience of the Panama Papers. As discussed above, the leak concerned political leaders and public officials from a large number of countries. Because our objective is to examine its effects on perceived state legitimacy, we focus on news reports implicating *domestic* political figures. Such reports provide citizens with information about the conduct of their own political elites

[10] Online Appendix Figure A1 also provides a map of the countries in the LiTS III and Eurobarometer samples according to this V-Dem PCA index.

and are therefore more directly relevant to their assessments of national institutions.

To construct our measures of exposure, we retrieve articles published between March 25 and May 31, 2016, that contain at least one of the search terms "*Panam**" or "*Fonseca*." We conduct the search through Factiva, a database owned by Dow Jones & Company that provides access to newspapers, news agencies, and other media sources worldwide. Given the large number of countries in our analysis, we use the BBC Monitoring Media Guides to identify reliable national news sources.[11] For the United Kingdom, we instead search the seven newspapers and tabloids with the highest readership in the British Election Study sample. Together, these outlets account for approximately 76% of declared newspaper readership.[12]

The initial collection contains 5,090 articles across the three survey settings. The survey-specific corpora overlap because some countries are included in both the LiTS III and Eurobarometer samples; their article counts therefore cannot be added to obtain the overall total. This initial sample includes some articles referring to Panama or to individuals named Fonseca but unrelated to the Panama Papers. It also includes coverage of the global scandal or of foreign political figures without mentioning domestic politicians. Because these articles do not provide direct information about the integrity of respondents' own political elites, we exclude them from our measure of exposure.

Specifically, we classify the articles using a large language model (LLM).[13] The model first determines whether an article concerns the Panama Papers and then identifies the individuals directly implicated, their nationalities, and whether they hold a political role. We retain an article when it concerns the scandal and implicates at least one domestic political figure, whom we identify as either an officeholder or a politician's close relative. Online Appendix Section B describes the classification procedure, validation exercise, and manual checks in detail.

Using the classified articles, we construct a new country-day database of media coverage. For each country and date, the database records the number of articles implicating at least one domestic politician in the Panama Papers. The resulting sample contains 1,003 relevant articles, approximately one-fifth of the initial collection. Of these, 474 belong to the Eurobarometer corpus and concern domestic political figures, as reported in Online Appendix Table B2.

Figure 4 illustrates the timing of this coverage. To make temporal patterns comparable across countries with different levels of media output, we express the number of relevant articles published on each day as a share of all relevant articles published in that country during the observation period. We report this daily distribution for the United Kingdom, Spain, and France, three of the countries with the most extensive domestic coverage, together with the corresponding average distributions for the LiTS III and Eurobarometer samples.

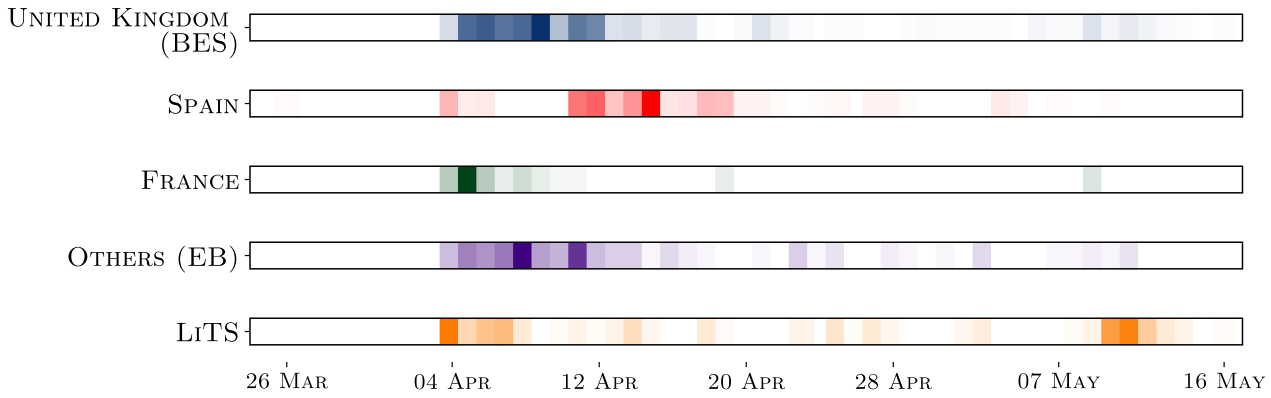
The figure shows that coverage began on April 3, with no anticipatory reporting before the coordinated disclosure. News attention was initially concentrated in the days immediately following the leak. However, the exceptionally large volume of documents meant that journalists identified the involvement of domestic politicians at different times across countries. In smaller or less internationally prominent countries, such revelations often appeared only after several days or weeks of investigation. This pattern is especially visible in the LiTS III sample, where coverage displays several distinct peaks. Exposure to information about domestic political figures was therefore staggered across countries over time, providing a central source of variation

[11] The BBC Monitoring Media Guides describe the media landscape of individual countries and identify prominent sources of political information, including in contexts where the media environment is restricted or otherwise difficult to assess. For further information, see https://monitoring.bbc.co.uk/inside-bbcm/4.

[12] The BES asks respondents which daily newspaper they read most often. Approximately one-third report that they do not read a newspaper. Among newspaper readers, around 20% report reading *The Daily Mail*, followed by *The Sun* (15%), *The Guardian* (12%), *The Mirror* (10%), *The Daily Telegraph* (7%), *The Times* (7%), and *The Independent* (3%). Detailed readership rates are reported in Online Appendix Table A8.

[13] For an overview of the development and characteristics of large language models, see Ash et al. (2025). On their applications in economic research, see Korinek (2023).

Figure 4: Media coverage of home country politicians involved in the Panama Papers scandal



Notes. The figure reports the ratio of relevant articles published on each day to all relevant articles published in that country during the observation period. Darker shades indicate higher ratios.

for our empirical analysis.

4 Empirical strategies

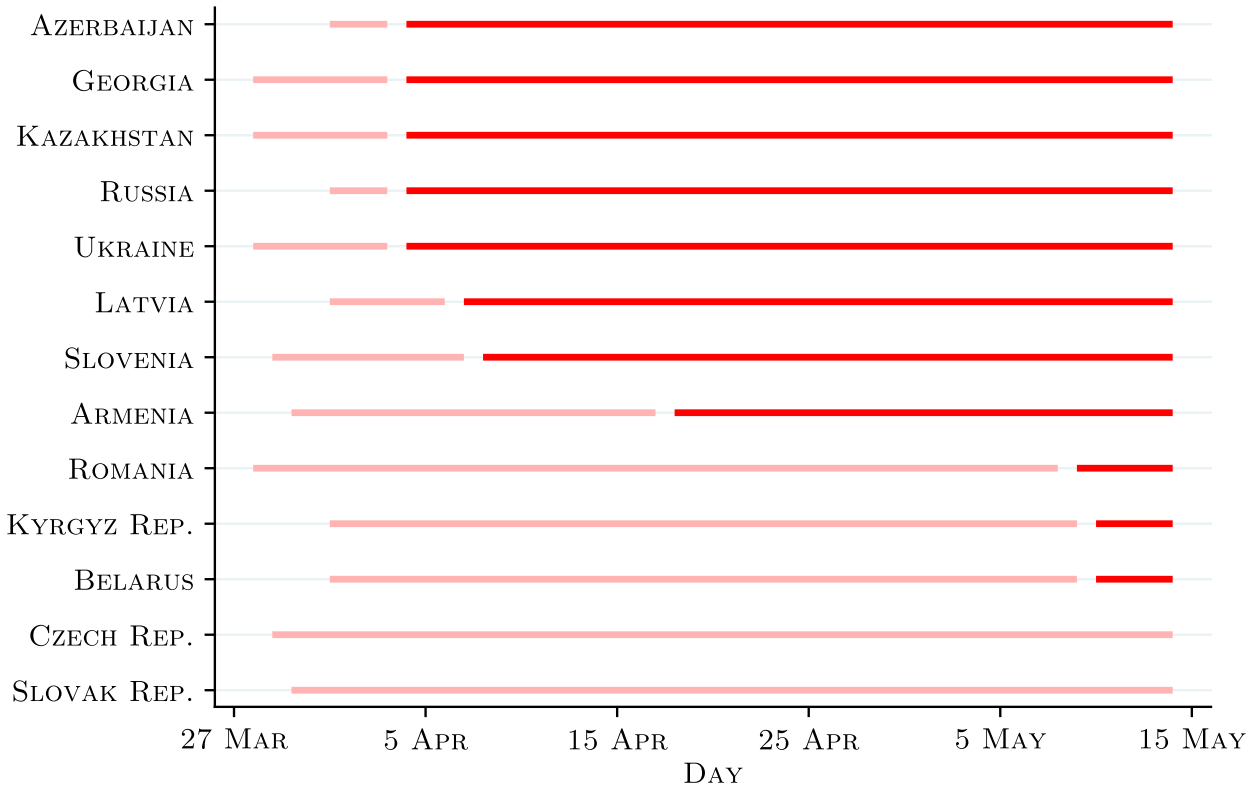
The distinct nature of the three surveys and the various institutional contexts we analyze allow us to employ diverse empirical strategies and estimators to identify different margins of the Panama Papers leak on institutional trust and democratic assessment. Below, we provide a detailed description of the specific contexts and explain the estimators employed to identify the effects. It is helpful to clarify that these sets of estimates should be interpreted as complementary evidence. In LiTS, we exploit the staggered timing of the first domestic political revelation, so the estimand is the incremental effect of the scandal acquiring domestic political relevance relative to countries where such information has not yet appeared. The BES instead provides within-individual evidence on changes surrounding the UK disclosure and contains no contemporaneous untreated comparison group. Finally, Eurobarometer exploits a continuous measure of domestic political salience and identifies whether post-disclosure changes differ with the share of Panama Papers coverage devoted to domestic political figures.

4.1 Staggered exposure across countries: LiTS III

LiTS III provides our most direct timing-based empirical design. The survey was being administered in 14 countries when the Panama Papers became public in April 2016. Importantly, we define exposure not by the global disclosure of the documents on April 3, but by the first report in the national media implicating a *domestic* political figure. For each country, respondents interviewed before this date are classified as *not yet exposed* to the domestic scandal, whereas those interviewed afterward are classified as exposed.

It is worth highlighting that *not yet exposed* here refers only to the absence of a qualifying domestic political revelation. After April 3, respondents in these comparison countries may already have been aware of the global Panama Papers leak, foreign politicians implicated in it, and the broader debate about offshore finance. The same-date comparison therefore differences out information shocks common across countries and isolates the incremental change associated with the scandal becoming domestically politically relevant. Our LiTS estimates should consequently not be interpreted as the effect of first learning about the Panama Papers.

Figure 5: Timing of the first domestic political revelation during the LiTS III rollout



Notes. The figure reports the LiTS III fieldwork dates in 15 countries over the period from March 27 to May 15. The change in color reflects the occurrence of the first domestic revelation.

Differential exposure across countries resulted from the volume and complexity of the leaked documents, which likely delayed the disclosure of some domestic cases. Figure 5 combines the LiTS III fieldwork dates with the timing of the first domestic revelation over the period from March 27 to May 15.[14] The earliest domestic revelations appeared in Azerbaijan, Georgia, Kazakhstan, Russia, and Ukraine. Reports involving political figures in other countries appeared later, while some countries remained unexposed throughout the observation window.[15] The resulting staggered timing allows us to compare respondents interviewed after a domestic revelation with respondents interviewed at the same point in time in countries where no domestic political figure had yet been implicated.

A conventional two-way fixed-effects difference-in-differences estimator may be inappropriate in this setting. When treatment timing varies across units, such an estimator can compare newly treated countries not only with countries that remain untreated, but also with countries that were exposed earlier. These comparisons may be contaminated when the effects of the scandal vary across countries or with the length of exposure (Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Roth et al., 2023).

We therefore use the difference-in-differences estimator proposed by Callaway and Sant'Anna

[14]Online Appendix Figure A2 provides additional detail on the temporal distribution of media coverage across groups of countries. The articles used to determine the first date of domestic exposure are listed in Online Appendix Table B1.

[15]Unfortunately, Factiva reports few news sources for Moldova. Using this small sample, we detect one minor scandal on April 14, 2016, involving two second-order public officers. Given the sensitivity of this information to the media landscape, we remove the country from our analysis. See Online Appendix Section B for more details.

(2021). This approach constructs group-time average treatment effects using only countries that are not yet exposed as the comparison group. It consequently avoids comparisons between countries that have already been exposed for different lengths of time and allows the effects of the scandal to vary across treatment cohorts and over time.

Formally, if we denote with $G_i = g$ the respondent i living in a country first exposed to a domestic revelation on day g and with $G_i = \infty$ the respondent i not yet exposed to it by day t, the average treatment effect on day t, for a cohort first exposed on day g, is

$$ATT(g,t) = \mathbb{E}\left[Y_{it}(g) - Y_{it}(\infty) \mid G_i = g\right], \tag{1}$$

where $Y_{it}(g)$ denotes the potential outcome on day t when domestic exposure begins on day g, and $Y_{it}(\infty)$ denotes the potential outcome if the country remains unexposed through day t. In practice, the estimator identifies $ATT(g,t)$ by comparing the change in outcomes for respondents in cohort g with the corresponding change among respondents in countries that have not yet been exposed.

Treatment is defined as binary and irreversible: once the national media report the involvement of a domestic political figure, the country remains classified as exposed. This does not require media coverage to be equally intense across countries or treatment effects to be homogeneous. Rather, the resulting estimates average over differences in the salience and diffusion of the domestic revelations. We will examine exposure intensity separately in the Eurobarometer analysis.

Identification relies on two main assumptions. First, in the absence of the domestic revelation, outcomes in countries exposed on different dates would have followed parallel trends, conditional on the observed covariates. Second, respondents do not anticipate domestic revelations before they appear in the media. To corroborate these assumptions, we will present below event-study estimates that trace their dynamics relative to the first domestic revelation. It should also be noted that the absence of relevant coverage before the country-specific exposure dates provides support for the no-anticipation assumption, while the short fieldwork window limits the scope for slow-moving country-specific factors to generate differential changes in institutional trust.

4.2 Within-individual changes in the United Kingdom: BES

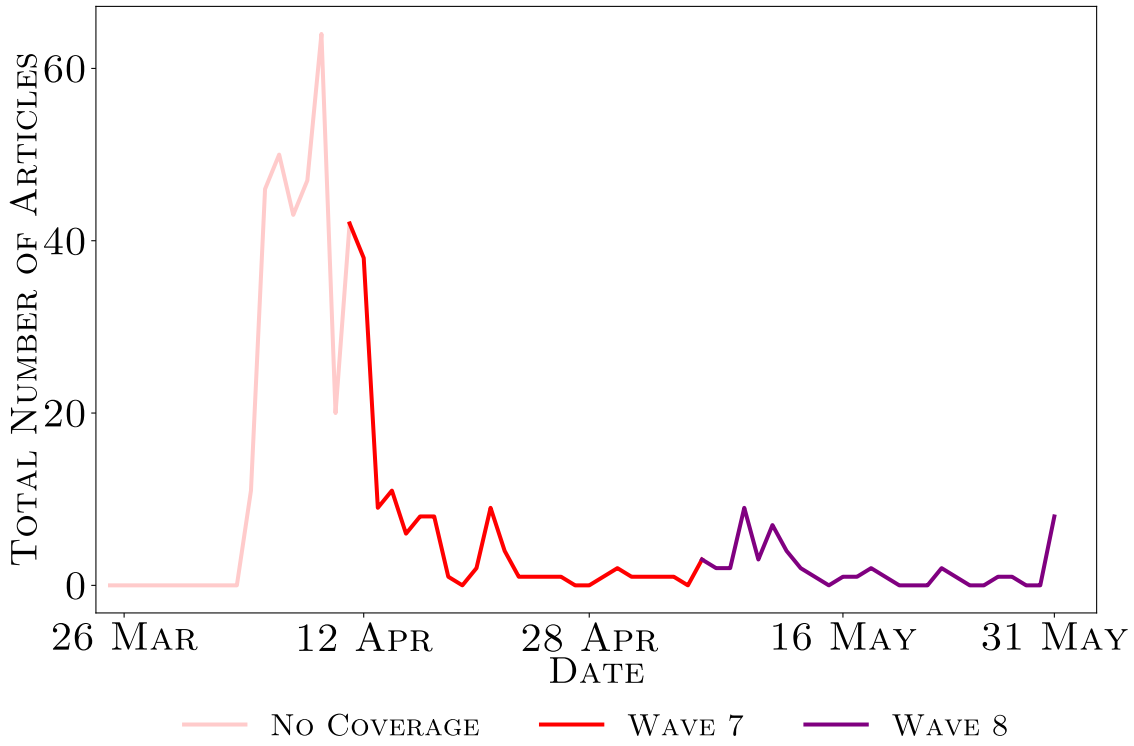
Our second empirical setting focuses on the United Kingdom, where the Panama Papers received extensive media attention and directly implicated the family finances of then-Prime Minister David Cameron. As Figure 6 shows, coverage rose sharply following the disclosure and peaked during the second week of April 2016. The seventh wave of the British Election Study (BES) was conducted shortly after this peak, while Wave 8 began after May 6. We compare responses in these post-disclosure waves with those collected from the same individuals in Waves 4, 5, and 6, which were fielded during the preceding year.

To examine within-individual changes in institutional trust surrounding the Panama Papers disclosure, we leverage the panel structure of the BES, which allows us to follow the same individuals over time. This is important because it allows us to include individual fixed effects in the regression equation, thereby absorbing individual-level heterogeneity, and to exploit within-individual variation in trust over time. We formalize this empirical setup in the following regression equation,

$$Y_{it} = \alpha_i + \beta\,\text{Post}_t + \lambda' X_{it} + \varepsilon_{it}, \tag{2}$$

where Y_{it} is the level of institutional trust reported by individual i in survey wave t. The indicator Post_t is the post-scandal dummy variable, equal to 1 for Waves 7 and 8, which were conducted after the disclosure of the Panama Papers, and 0 for Waves 4, 5, and 6. As

Figure 6: Media coverage of the scandal and BES rollout in the United Kingdom



Notes. The figure reports the total number of articles covering British political officials involved in the Panama Papers scandal.

mentioned above, we add individual fixed effects, α_i. The vector X_{it} includes time-varying characteristics—age, household income, and the number of children—while ε_{it} denotes the error term. Standard errors are clustered at the individual level.

The coefficient β measures the average within-individual change in the outcome between the pre- and post-disclosure waves. As a complementary analysis, we replace Post_t with indicators for the individual survey waves, omitting Wave 6 as the reference category:

$$Y_{it} = \alpha_i + \sum_{w \neq 6} \beta_w \mathbb{1}\{t = w\} + \lambda' X_{it} + \varepsilon_{it}. \tag{3}$$

This specification allows us to examine whether attitudes were already changing before the leak and whether the post-disclosure change is concentrated in Wave 7, which was administered closest to the peak in media coverage.

One important note is that this design does not include an untreated comparison group. It therefore cannot separately identify the effect of the Panama Papers from all other events that may have affected political attitudes between the survey waves. We consequently interpret the BES estimates as complementary within-individual evidence rather than as a stand-alone difference-in-differences estimate. Their interpretation as effects of the scandal requires that, in its absence, respondents' attitudes would not have changed discontinuously between the pre- and post-disclosure waves.

The most important contemporaneous threat to this interpretation is the referendum on the United Kingdom's membership of the European Union, held on June 23, 2016. Although the referendum occurred more than two months after the Panama Papers disclosure, the campaign may have already dominated British political debate and focused heavily on immigration and the country's relationship with Europe (e.g., Dennison and Geddes, 2018). Changes observed

in the post-disclosure waves could therefore partly reflect the referendum campaign rather than the offshore financial scandal. Below, we will show that this is unlikely, as media attention centered on taxation during April 2016.

4.3 Cross-country variation in exposure intensity: Eurobarometer

Our third empirical design uses repeated cross-sectional data from Eurobarometer (EB). We select seven survey waves spanning autumn 2013 to autumn 2017: Waves 82, 83, and 84 were conducted before the Panama Papers disclosure, while Waves 85, 86, 87, and 88 were conducted afterward, with Wave 85 starting on May 21, 2016. Our identifying variation does not come from this pre/post comparison alone. Rather, we interact the post-disclosure period with cross-country differences in the domestic political salience of the scandal. Survey-wave fixed effects absorb changes common to all countries, so the specification asks whether post-disclosure changes differ systematically with the share of national Panama Papers coverage devoted to domestic political figures.

Specifically, we measure country-level exposure intensity as the share of all articles concerning the Panama Papers that mention at least one domestic political figure implicated in the scandal. Formally, the measure is the ratio of articles covering domestic political involvement to the total number of articles covering the Panama Papers in that country. By normalizing domestic coverage by overall coverage of the leak, this measure reduces the influence of cross-country differences in media output and captures the extent to which the scandal acquired a specifically domestic political dimension.

It is important to note that the resulting index does not vary over time, but it changes only across countries as we pool media coverage from March 25 to May 18. Figure 7 illustrates the resulting variation. Darker shades indicate countries in which a larger share of Panama Papers coverage concerned domestic political figures. Exposure was highest in Spain, where approximately 69% of the relevant articles discussed domestic political involvement. Greece, Latvia, and the United Kingdom were also highly exposed. Domestic coverage of the scandal was rather close to zero in countries including Croatia, Denmark, Finland, Germany, Sweden, and Turkey. In the Online Appendix, we show the distribution, which highlights a non-symmetric functional form (Figure A3).

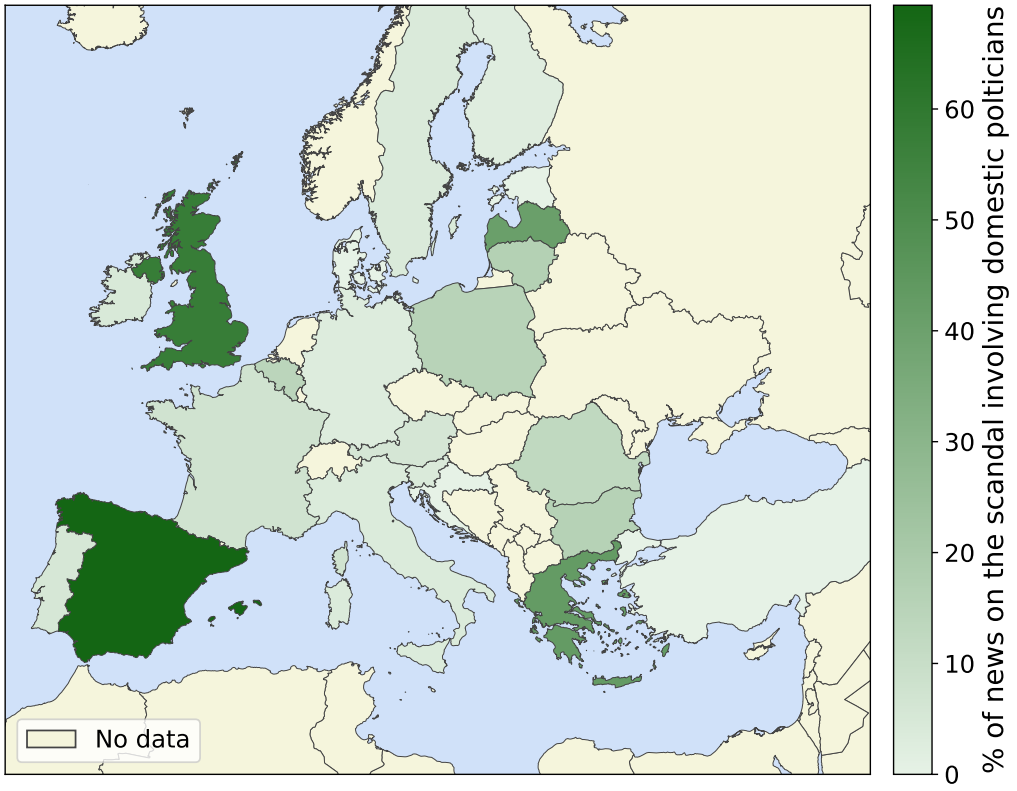
We combine the timing of the disclosure with this continuous measure of exposure in the following difference-in-differences specification:

$$Y_{ijt} = \alpha_j + \gamma_t + \beta \left(\text{Post}_t \times \text{Coverage}_j\right) + \lambda' X_{ijt} + \varepsilon_{ijt}, \tag{4}$$

where Y_{ijt} denotes the outcome reported by individual i, residing in country j, and interviewed in survey wave t. Depending on the analysis, the outcome measures trust in the national government or Parliament, or respondents' satisfaction with the functioning of democracy. The indicator Post_t equals 1 for survey waves conducted after the Panama Papers disclosure. The variable Coverage_j measures the intensity of domestic exposure in country j. Country fixed effects, α_j, absorb time-invariant differences across countries, including persistent differences in institutional trust, political culture, and media systems. Wave fixed effects, γ_t, absorb shocks common to all countries in a given survey wave. The vector X_{ijt} includes age, gender, educational attainment, rural residence, employment status, and whether respondents have children. Estimates use the Eurobarometer sampling weights.

We provide both heteroskedasticity-robust and standard errors clustered at the country level. However, it should be noted that, although the analysis contains a large number of individual respondents, the identifying variation for the interaction $Post_t \times Coverage_j$ is ultimately provided by the 23 country-level exposure values. Moreover, as shown in Figure A3, this variation is highly uneven: most countries exhibit relatively limited domestic coverage, while a

Figure 7: Domestic exposure to the Panama Papers in the Eurobarometer sample



Notes. The figure reports the ratio of articles covering domestic political involvement to the total number of articles covering the Panama Papers in that country. Darker shades indicate higher exposure of domestic political figures in the scandal.

small number of countries account for much of the variation at the upper end of the exposure distribution. Country-clustered inference therefore relies on a small and unbalanced number of independent exposure units and is correspondingly imprecise. As we will show below, the standard errors increase substantially and the estimated coefficients can no longer be statistically distinguished from zero at conventional levels when we allow for arbitrary dependence within countries. With this caveat in mind, we therefore regard the Eurobarometer estimates as informative about the direction and magnitude of the cross-country exposure gradient, but interpret their statistical precision cautiously.

The coefficient β captures an intensive-margin differential in post-disclosure changes. It indicates whether outcomes changed more after the disclosure in countries where domestic political involvement received greater media attention. It therefore does not identify the average effect of the Panama Papers disclosure common to all countries. A negative coefficient in the institutional-trust specifications, for example, would indicate that trust declined more in countries where a larger share of Panama Papers coverage implicated domestic political figures. Because Coverage_j ranges from 0 to 1, β represents the differential post-disclosure change associated with moving from no domestic coverage to exclusively domestic coverage. In presenting the results, we also report effects associated with a one-standard-deviation increase in exposure and with comparisons between specific high- and low-exposure countries.

A causal interpretation requires that, in the absence of the Panama Papers, countries with different levels of domestic exposure would have experienced similar changes in the outcomes. Country fixed effects account for persistent differences between high- and low-exposure countries, but they do not eliminate the possibility that such countries were already following

different trends. To examine the dynamics of the estimated effects and assess whether countries with different levels of exposure were already following different trends before the scandal, we complement Equation 4 with an event-study-style specification. Specifically, we interact the country-level exposure measure with indicators for each Eurobarometer wave, using Wave 84—the last survey wave conducted before the Panama Papers disclosure—as the reference period:

$$Y_{ijt} = \alpha_j + \gamma_t + \sum_{\tau \neq 84} \beta_\tau \left(\mathbb{1}\{t = \tau\} \times \text{Coverage}_j \right) + \lambda' X_{ijt} + \varepsilon_{ijt}. \tag{5}$$

As in Equation 4, α_j and γ_t denote country and survey-wave fixed effects, respectively, while X_{ijt} contains the individual-level controls. The coefficients β_τ capture the difference in the outcome associated with greater exposure intensity in wave τ, relative to the corresponding difference in Wave 84. The coefficients for Waves 82 and 83 allow us to assess whether high- and low-exposure countries exhibited differential changes before the leak.

A further concern is that the intensity of domestic coverage may be related to country characteristics that also shape post-disclosure changes in political attitudes. For example, the media may report political misconduct more extensively in countries with stronger press freedom, more competitive political systems, or more prominent domestic figures involved in the scandal. Such differences could also affect how citizens respond to the revelations. We therefore complement the full-sample analysis with a more geographically focused comparison between Galicia in Spain and Norte in Portugal.

Galicia and Norte are contiguous regions with close historical, cultural, and linguistic ties (Figure A4). These similarities culminated in the establishment of the Galicia-North Portugal Euroregion in 2008, which facilitated a further integration of the two regions through the enactment of several institutional agreements, including cultural and educational exchanges, bilateral political strategies, and ethnogastronomic accords. However, the two regions differed sharply in their exposure to the Panama Papers. The scandal received extensive domestic coverage in Spain, while comparatively little Portuguese coverage implicated national political figures. We exploit this contrast by comparing changes in political attitudes in Galicia and Norte before and after the disclosure.

For this regional analysis, we replace the continuous exposure measure with an indicator for Galicia and estimate a conventional difference-in-differences model including region and survey-wave fixed effects. The interaction between the post-disclosure indicator and the Galicia indicator captures whether institutional trust and satisfaction with democracy changed more in the highly exposed region than in the neighboring, less exposed region. The geographical comparison reduces the institutional and cultural heterogeneity present in the full European sample. It does not eliminate all differences between the two regions, but it provides another complementary test based on a narrower and more comparable setting.

5 Results

In this section, we present our regression results. We first examine whether the Panama Papers increased the public salience of taxation, thereby establishing that the leak affected an issue central to the fiscal social contract. We then turn to our main outcomes: trust in state institutions and citizens' assessments of democracy.

5.1 Taxation as a salient topic

The British Election Study asks respondents to identify the single most important issue facing the country, allowing us to examine whether taxation became more politically salient fol-

lowing the Panama Papers disclosure. Respondents provide an open-ended answer that is subsequently classified into mutually exclusive categories, including "taxation," "immigration," and "Europe."[16] The taxation category includes general references to taxation and particular taxes, as well as mentions of tax avoidance and related issues (BES, 2024, p. 20). Across the full sample, taxation was identified as the country's most important issue only in 5% of the respondent-wave pairs. Despite the low frequency, we observe that taxation experienced a pronounced increase in salience immediately after the Panama Papers revelations.

This is critical for our analysis because, as we explained above, the referendum on the United Kingdom's membership of the EU was only two and a half months away, representing a contemporaneous threat to the interpretation of our findings. This threat is corroborated by raw data which indicate that issues that were prominent during the campaign preceding the referendum received close attention. For example, topics like immigration and Europe were mentioned in 24% and 15% of the respondent-wave pairs, respectively. However, in Online Appendix Figure A5, we show that the increase in taxation, relative to its own baseline, was larger than the corresponding changes in the salience of immigration and Europe throughout the entire month of April.[17]

We formally examine this change in Table 3. Columns 1 and 2 exploit within-individual variation in the British Election Study using the specification described in Equation 2. Despite its relatively low unconditional frequency, respondents became more likely to identify taxation as the country's most important issue after the disclosure. Using all available waves, the estimated increase is 0.1 percentage points (column 1). When we exclude Wave 8, which was increasingly dominated by media coverage of the Brexit referendum, the estimated effect rises to 0.3 percentage points (column 2).[18] The latter estimate is statistically significant at the 1% level and economically meaningful: relative to the average probability of reporting taxation, it represents an increase of approximately 60%. These findings indicate that the Panama Papers entered Britons' political considerations and directed public attention toward an issue closely connected to fiscal fairness and the relationship between citizens, political elites, and the state.

We complement this finding with suggestive evidence from Eurobarometer, which poses a similar question to survey respondents. In this sample, the probability of reporting taxation as the most important issue the country is facing at the moment is 9.2 points. After the Panama Papers disclosure, the probability of identifying taxation as the most important issue increased more in countries where a larger share of media coverage concerned domestic political figures. Specifically, a 1-percentage-point increase in domestic exposure is associated with an additional increase of 0.014 percentage points in the probability of mentioning taxation (column 3). While the coefficient is imprecisely estimated when we cluster standard errors at the country level, the magnitude suggests that one-standard-deviation increase in exposure is associated with an additional increase of approximately 2.8% of the outcome mean (i.e., $\frac{0.014 \times 0.186}{0.092}$).[19]

Comparing Galicia with Norte yields a difference-in-differences coefficient of 0.066 (column 4). This estimate also suggests that, after the Panama Papers disclosure, the probability of

[16] In each wave of the British Election Study Internet Panel, respondents are asked: "*As far as you're concerned, what is the SINGLE MOST important issue facing the country at the present time?*" (capital letters in the questionnaire). Open-ended responses are subsequently classified into mutually exclusive topics. See the BES codebook for further details (BES, 2024, pp. 15–26).

[17] Specifically, in Online Appendix Figure A5, we illustrate this pattern using daily responses from Waves 7 and 8. We first calculate the daily share of respondents mentioning taxation, immigration, and Europe. We then standardize each series and subtract the standardized salience of Europe and immigration from that of taxation. Positive values therefore indicate that taxation was unusually salient relative to the comparison topic, given the typical variation in each series.

[18] The corresponding event study plot also shows that taxation received close attention exclusively during April, not in May, when topics related to the Brexit referendum became more salient (Online Appendix Figure A6).

[19] In Online Appendix Figure A7, we also illustrate the event study plot.

identifying taxation as the most important issue increased by 6.6 percentage points more in Galicia than in Norte.

5.2 Trust in state institutions

We next examine the effects of the Panama Papers on institutional trust, our main observable measure of state legitimacy, using the three empirical designs in turn. Because the designs exploit different sources of variation, their coefficients should be interpreted according to the corresponding estimand rather than compared as estimates of a common treatment effect.

We begin with our main analysis—the staggered difference-in-differences analysis based on LiTS III. Table 4 reports aggregated average treatment effects for trust in the president (column 1), the government (column 2), and Parliament (column 3). The estimates are conditional on individual characteristics and are obtained using the doubly robust estimator proposed by Callaway and Sant'Anna (2021). In the Online Appendix, we additionally show estimates based on clustered standard errors and wild bootstrap inference (Table A9). For transparency, Table A10 also reports the corresponding unconditional estimates, whilst Table A11 illustrates the results using a conventional two-way fixed-effects specification.

Because the outcomes are binary, the coefficients can be interpreted as changes in the probability of reporting trust. Exposure to news implicating a domestic political figure in the Panama Papers is estimated to reduce the probability of trusting the president by 23.8 percentage points, relative to the change observed among respondents in countries not yet exposed to a domestic revelation (column 1). The corresponding estimates indicate declines of 34.4 percentage points in trust in the government (column 2) and 23.3 percentage points in trust in Parliament (column 3). All three effects are statistically significant and large in magnitude.[20] Taken together, the results suggest that the consequences of the scandal extended beyond the political figures directly implicated. The revelations also weakened citizens' confidence in the central institutions through which political authority is exercised, consistent with an erosion of perceived state legitimacy.

We next examine the dynamics of these effects using the event-study aggregation proposed by Callaway and Sant'Anna (2021). Online Appendix Figures A8, A9, and A10 show no systematic differences in trust before the first domestic revelation, relative to countries in which no domestic political revelation had yet occurred. After exposure, however, trust begins to diverge, with the clearest and most persistent decline observed for national governments. This temporal pattern supports the interpretation that the revelations altered citizens' assessments of the legitimacy of incumbent political institutions.

We next turn to the British Election Study, which allows us to examine within-individual changes in political attitudes in the United Kingdom. Table 5 reports estimates from Equation 2. We begin with evaluations of the political actors most directly associated with the scandal. Following the disclosure, support for then-Prime Minister David Cameron declined by 7.7 percentage points (column 1), while support for the Conservative Party fell by 4.1 percentage points (column 2). The larger response for Cameron is consistent with his particularly prominent role in British coverage of the Panama Papers. The estimated decline corresponds to approximately 17% of the average level of support for him.

The reputational consequences were not confined to the political actors most directly associated with the scandal but extended to broader political institutions. Trust in Members of

[20] Their magnitudes correspond to approximately 56% of the sample mean for trust in the president (i.e., 0.238/0.428), 110% of the sample mean for trust in the government (i.e., 0.344/0.314), and 86% of the sample mean for trust in Parliament (i.e., 0.233/0.271). It is worth highlighting, however, that these comparisons are intended only to convey the scale of the estimates, since the full-sample means do not coincide with the counterfactual levels of trust for treated respondents.

Parliament declined by 12.1 percentage points (column 3), an estimate statistically significant at the 1% level and equivalent to approximately 26% of the outcome mean. Notably, this institutional effect is larger than the estimated declines in support for Cameron and the Conservative Party, suggesting that the revelations affected citizens' assessments of the political class more broadly rather than only those directly implicated. Government approval also fell by 5.4 percentage points (column 4), corresponding to around 12% of the average approval rate. However, because the BES does not contain a directly comparable measure of trust in government, this outcome should be interpreted as an assessment of government performance rather than as a direct measure of institutional trust.[21]

It should be noted that our baseline estimates must be interpreted as intention-to-treat effects, since we do not directly observe whether individual respondents were exposed to news about the Panama Papers. However, the British Election Study allows us to moderate this concern, as it collects self-reported respondents' media consumption. We therefore use this information from Wave 6, the round administered just before the Panama Papers disclosure, and construct two binary indicators of pre-disclosure news consumption. The first captures general news exposure and equals one if respondents report spending any time following politics or current affairs through television, newspapers, radio, or the internet. The second captures newspaper readership and equals one if respondents report reading any newspaper, including local ones. We then interact each indicator with the post-disclosure dummy and re-estimate Table 5 in order to assess whether the estimated effects are concentrated among respondents who were more likely to come across political news. The estimated decline in institutional trust is generally larger among respondents with greater pre-disclosure news consumption, both when exposure is measured using general political-news consumption and newspaper readership (Tables A12 and A13). This pattern is consistent with the interpretation that exposure to information about the Panama Papers contributes to the observed decline in institutional trust.

Finally, Table 6 reports the difference-in-differences estimates obtained using Eurobarometer data.[22] Columns 1 and 2 use trust in the national government and Parliament as outcomes, respectively. In both cases, the interaction between the post-disclosure indicator and exposure intensity is negative. The point estimates thus indicate that trust declined more in countries with greater domestic exposure to domestic political figures. While the estimates are statistically significant under individual-level inference, uncertainty is substantial once inference is conducted at the country level, reflecting the limited and uneven cross-country variation in exposure.[23]

Focusing on the point estimates, we obtain an estimated coefficient for trust in government of -0.042 (column 1). Because exposure intensity ranges from 0 to 1, a one-standard-deviation increase in exposure—equal to 0.186—is associated with an additional post-disclosure decline of approximately 0.8 percentage points in the probability of trusting the government (i.e.,

[21] Online Appendix Figures A11, A12, and A13 report event study plots for approval rates for Cameron and the Conservative Party as well as for trust in members of Parliament. As we remarked above, approval rate for the government is only surveyed in Waves 4 and 7. We therefore do not present an event study plot for this item.

[22] In the Online Appendix, we also show graphically that trust in government and in the Parliament both declined immediately after the Panama Papers disclosure across the whole sample (Figures A14 and A15).

[23] As an additional sensitivity exercise, in the Online Appendix we re-estimate the Eurobarometer specification sequentially excluding each country from the sample (Figures A16 and A17). The institutional-trust estimates are similar under most exclusions, but are sensitive to the composition of the country sample. In particular, excluding either Greece or Portugal substantially attenuates the estimated exposure gradient for trust in government and Parliament, with the government-trust coefficient becoming close to zero when Greece is omitted. This sensitivity is consistent with the limited and uneven cross-country variation underlying the Eurobarometer design and reinforces our interpretation of these estimates as complementary evidence on the intensive margin rather than as a stand-alone source of identification.

-0.042×0.186). This corresponds to approximately 2.3% of the outcome mean. To illustrate the magnitude using observed differences in exposure, Spain had an exposure measure of 0.693, whereas Denmark had virtually no coverage implicating domestic political figures. The estimates therefore imply that the post-disclosure decline in trust in government was approximately 2.9 percentage points larger in Spain than in Denmark (i.e., -0.042×0.693), equivalent to around 8.4% of the average level of government trust.

The effect on trust in Parliament is somewhat smaller. The estimated coefficient of -0.032 implies that a one-standard-deviation increase in exposure is associated with an additional decline of approximately 0.6 percentage points in the probability of trusting Parliament (i.e., -0.032×0.186). Relative to Denmark, the implied post-disclosure decline in Spain is approximately 2.2 percentage points. Taken together, these further findings add suggestive evidence that the reputational consequences of the scandal extend to the principal institutions through which political authority is exercised, consistent with an erosion of perceived state legitimacy.[24]

We next turn to the separate regional comparison between Galicia and Norte. This specification identifies a different contrast from the continuous-exposure Eurobarometer model and its coefficient is therefore not directly comparable in magnitude. Following the Panama Papers disclosure, the probability of trusting the government and Parliament declined by 25.4 and 12 percentage points more, respectively, in Galicia, where domestic political involvement received extensive media coverage, than in the comparatively less exposed region of Norte.[25] These differential changes are equivalent to approximately 78% and 47% of the average level of government trust and Parliament trust in the regional sample, respectively. The magnitude of the estimates suggests that, where the scandal acquired a particularly salient domestic dimension, the reputational consequences extended substantially to the institution exercising national political authority, consistent with a pronounced erosion of perceived state legitimacy.

5.3 Consequences for democratic assessment

We finally examine whether the decline in institutional trust documented above translated into a broader deterioration in citizens' assessments of democracy. Table 7 reports the results. As explained above, the outcomes differ conceptually across the three surveys. LiTS III measures support for democracy as a political regime, whereas the BES and Eurobarometer measure satisfaction with the way democracy functions in respondents' countries. This distinction allows us to assess whether citizens separated the misconduct of political elites and incumbent institutions from their evaluation of the democratic system itself.

Column 1 reports the staggered difference-in-differences estimate for LiTS III. Exposure to news implicating domestic political figures increased the probability of identifying democracy as the preferred political system by 18.9 percentage points. The estimate is statistically significant at the 5% level and amounts to approximately 39% of the outcome mean.[26] Thus, although the revelations substantially reduced trust in presidents, governments, and parliaments, they

[24] Event study plots are reported in Online Appendix Figures A18 and A19. The dynamic estimates call for some caution in interpreting the Eurobarometer estimates as an immediate discontinuous response in trust in government to the disclosure. The negative differential does not seem to emerge immediately after the disclosure but becomes pronounced in later survey rounds. We therefore interpret the Eurobarometer evidence as indicating a gradual post-disclosure deterioration in relative trust in government rather than a sharp contemporaneous break.

[25] In Online Appendix Figures A20 and A21, we show the corresponding event study plots. They provide support for the parallel-trends assumption underlying the difference-in-differences design because the coefficients for Waves 82 and 83 are close to zero. The coefficients for Waves 85–88 instead indicate the emergence of a differential post-disclosure change.

[26] This estimate retains statistical precision when we cluster standard errors at the country level and when we use wild bootstrap inference (Table A9).

did not produce a generalized rejection of democracy. On the contrary, respondents became more likely to regard democracy as preferable to authoritarian alternatives.

The evidence from the United Kingdom points in the same direction. Following the disclosure, the respondents' satisfaction with the functioning of democracy increased by 3.8 percentage points (column 2). The estimate is statistically significant at the 1% level and corresponds to approximately 7.6% of the average satisfaction rate. Given the absence of an untreated comparison group in the BES design, this result should be interpreted as complementary within-individual evidence. Nevertheless, together with the LiTS III findings, it is consistent with a *monitoring effect*: the disclosure may have demonstrated that democratic systems, through a free press and public scrutiny, are capable of uncovering elite misconduct.

The Eurobarometer results reveal an important qualification. In the full sample, we estimate a negative coefficient of the interaction between the post-disclosure indicator and exposure intensity (column 3). The coefficient of -0.072 suggests that satisfaction with democracy declined more after the leak in countries where a larger share of Panama Papers coverage implicated domestic political figures. The Eurobarometer setup therefore suits an analysis that is informative about the magnitude and direction of the intensive margin of the effect. While the estimated coefficient becomes statistically imprecise when we cluster standard errors at the country level, our finding suggests that a one-standard-deviation increase in exposure intensity, equal to 0.186, is associated with an additional decline of approximately 1.3 percentage points in satisfaction with democracy (i.e., -0.072×0.186).[27] Similarly, the estimates imply that the post-disclosure decline was approximately 5 percentage points larger in Spain, the most intensely exposed country, than in a country with virtually no domestic coverage.

The comparison between Galicia and Norte reinforces this conclusion. Following the disclosure, satisfaction with democracy declined by 20 percentage points more in Galicia than in the substantially less exposed region of Norte (column 4). This differential change amounts to approximately 51% of the average satisfaction rate in the regional sample. Although the narrower comparison cannot eliminate all differences between the two regions, the magnitude of the estimate indicates that extensive domestic involvement can weaken citizens' evaluations of how democracy functions.

Taken together, the results point to two competing effects of the Panama Papers. The disclosure itself may strengthen support for democracy by demonstrating the capacity of democratic institutions and investigative journalism to uncover misconduct. When the revelations instead expose extensive involvement among domestic political elites, citizens may interpret them as evidence that democratic institutions had failed to prevent or constrain such behavior. Exposure to a domestic scandal therefore appears to have reinforced support for democracy as an accountability mechanism while reducing satisfaction with its performance where the failure of prevention was most visible. However, because the designs identify different empirical objects, we do not interpret these patterns as separate estimates of a single underlying treatment effect.

5.4 Democratic institutions as a buffer

An important question is whether the institutional consequences of the Panama Papers depended on the quality of the democratic institutions in place when the scandal broke. More consolidated democratic systems may be better able to absorb revelations of misconduct without allowing the reputational consequences of individual political scandals to translate into a broader loss of institutional legitimacy. Institutions that provide effective mechanisms of accountability, political competition, and public scrutiny may, in this sense, make democracies

[27] The magnitude remains stable to the omission of most of the countries in our sample, while it increases when we remove Spain and decreases when we remove either Greece or Portugal. In all specifications, presented in Online Appendix Figure A22, the estimated coefficient remains statistically significant.

more resilient to negative information about political elites. We investigate this possibility using pre-scandal measures of democratic quality from V-Dem as described in Section 3.3. In this analysis, we will focus on LiTS III, where there is substantially greater cross-country variation in democratic quality than in the Eurobarometer sample, which consists predominantly of EU member states and therefore offers more limited variation along this dimension.

Because the first national-media revelation implicating a domestic political figure occurs at different times across countries in the LiTS sample, our main heterogeneity analysis also relies on the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021). We therefore divide countries according to their pre-scandal V-Dem PCA score and estimate group-time average treatment effects, $ATT(g,t)$, separately for countries with relatively low and high levels of democratic quality, defined as those below and above the median. For each treatment cohort g and period t, the estimator compares treated observations with observations in countries that have not yet been treated, using the same covariates and survey weights as in our baseline specification. We subsequently aggregate these group-time effects over the post-treatment period and report both the subgroup-specific ATTs and their difference. As explained above, this approach accommodates variation in treatment timing without imposing the homogeneous treatment-effect restrictions underlying the conventional two-way fixed-effects estimator in staggered-adoption settings.

Table 8 provides evidence consistent with the idea that stronger democratic institutions can attenuate the adverse consequences of the scandal, although the pattern is not uniform across political institutions. The clearest result concerns trust in government. Among respondents in the low-V-Dem group, the domestic-revelation treatment reduces the probability of reporting high trust in government by 24.8 percentage points. By contrast, the estimated effect in the high-V-Dem group is positive but statistically indistinguishable from zero. The difference between the two estimates is 40.1 percentage points and is statistically significant at the 5 percent level. A similar pattern emerges for trust in the president: the estimated treatment effect is substantially more negative in the low-V-Dem group, and the high-minus-low difference is positive and statistically significant. For trust in parliament, instead, the treatment effect is negative in both groups and the difference between them is statistically insignificant.[28] Taken together, these results provide suggestive evidence that pre-existing democratic quality moderates the institutional consequences of the scandal, as a specific form of democratic resilience.

6 Conclusion

Political legitimacy depends not only on the state's capacity to provide public goods and enforce collective rules, but also on citizens' belief that those rules apply fairly to political elites. This concern is especially important in taxation, which lies at the core of the fiscal relationship between citizens and the state. Revelations that political leaders have benefited from opaque offshore arrangements can therefore affect more than evaluations of the individuals involved: they may call into question the fairness of the tax system, the integrity of representative

[28] As a complementary exercise, Online Appendix Table A14 retains the continuous variation in democratic quality and estimates a conventional two-way fixed-effects specification in which treatment is interacted with the 2015 V-Dem PCA index. The interaction coefficients are positive for all three measures of institutional trust, implying that the negative post-scandal association becomes less pronounced as pre-existing democratic quality increases. The interaction is statistically significant for trust in government and parliament, at the 5 and 10 percent levels respectively, while it is smaller and imprecisely estimated for trust in the president. Thus, although the precise pattern across individual institutions differs from the subgroup estimates in Table 8, the TWFE exercise provides complementary evidence of a positive gradient between democratic quality and institutional resilience.

institutions, and the legitimacy of the political order itself. At the same time, such revelations expose an important tension. Investigative journalism and public scrutiny demonstrate the capacity of democratic systems to uncover misconduct, but the misconduct revealed may also provide evidence that those systems previously failed to prevent or constrain elite behavior.

This paper studies these questions using the Panama Papers disclosure and an original country-level database measuring the timing and intensity of media coverage implicating domestic political figures. The empirical exercises exploit different margins of the scandal. In LiTS III, we use cross-country differences in the timing of the first domestic political revelation, thereby estimating the incremental effect of the scandal acquiring domestic political relevance relative to countries where such information had not yet appeared. In the British Election Study, we document within-individual changes surrounding the UK disclosure. In Eurobarometer, we ask whether post-disclosure changes were more pronounced in countries where domestic political involvement accounted for a larger share of Panama Papers coverage, and we complement this analysis with a separate regional comparison between Galicia and Norte. Although these designs do not estimate a common causal parameter, their results point in a consistent substantive direction: when revelations become domestically politically salient, confidence in political representatives and institutions deteriorates. The evidence on democratic attitudes is more nuanced. Disclosure can underscore the monitoring capacity of democratic institutions, while particularly salient domestic involvement can worsen citizens' evaluations of how democracy functions in practice.

These findings have broader implications for how we think about political accountability and state legitimacy. Accountability is commonly understood as beneficial because information allows citizens to identify and sanction misconduct. Our results point to a complementary consequence: the information that makes accountability possible can itself generate reputational spillovers from political actors to the institutions they occupy. Our analysis suggests that whether such spillovers become a broader crisis of legitimacy depends, at least in part, on the institutional environment in which the revelation occurs. Stronger democratic institutions may give citizens greater reason to distinguish the misconduct of individual officeholders from the political system itself, thereby limiting the transmission of distrust across levels of political authority. Democratic resilience may therefore have an attitudinal dimension in addition to its institutional one: robust institutions may not only constrain political actors and facilitate accountability, but also help preserve confidence in the political order when those actors violate citizens' expectations.

This perspective also suggests several avenues for future research. One priority is to identify more precisely what citizens learn from scandals of this kind. The Panama Papers combined allegations ranging from formally legal tax avoidance to potentially unlawful conduct, making it difficult to separate reactions to illegality from reactions to privilege, unequal access to financial secrecy, or violations of norms of fiscal reciprocity. Future work could exploit variation in the legal status and substantive content of revelations to distinguish these mechanisms. A second question concerns dynamics. The short-run loss of trust documented here may fade as media attention recedes, persist as citizens update their beliefs about political institutions, or depend on the subsequent capacity of institutions to investigate and sanction misconduct. Following comparable information shocks over longer horizons would help establish when accountability shocks leave durable political legacies. Finally, an important next step is to connect changes in perceived legitimacy to behavior. Linking political-information shocks to administrative records or behavioral outcomes could reveal whether lower institutional trust ultimately affects tax compliance, electoral participation, protest, or other forms of engagement with the state.

More generally, our analysis indicates that political scandals should be evaluated not only by whether they expose wrongdoing and improve electoral accountability, but also by how they reshape citizens' beliefs about the institutions through which political authority is exercised.

Understanding when transparency strengthens democratic accountability without eroding institutional legitimacy remains an important challenge for future research.

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Table 3: Panama Papers scandal and salience of taxation

	Taxation is a priority issue			
	BES		EB	
	Full sample	Wave 8 removed	Full sample	Galicia vs Norte
	(1)	(2)	(3)	(4)
Post disclosure	0.001*	0.003***		
	(0.000)	(0.001)		
Post × Domestic-coverage share			0.014	
			(0.007)*	
			[0.033]	
Post × Galicia				0.066
				(0.020)***
Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	No	No
Country FE	No	No	Yes	Yes
Wave FE	No	No	Yes	Yes
DV mean	0.004	0.005	0.092	0.109
Obsrevations	129,441	99,702	170,216	3,655

Column 1 includes British Election Study's Waves 4 to 8, while in column 2 we remove Wave 8. Both columns include respondents' income, age, and whether respondents have children as time-variant control variables, and report standard errors clustered at the respondent level. Column 3 uses the full Eurobarometer sample, which comprises 24 European countries. Heteroskedasticity-robust standard errors are reported in rounded parentheses. Standard errors clustered at the country level are shown in squared brackets. Column 4 restricts the sample to the regions of Galicia and Norte and only reports heteroskedasticity-robust standard errors. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls include respondents' income, age, sex, rural residence, unemployment, education, and whether respondents have children.

Table 4: Panama Papers scandal and institutional trust in the LiTS III sample

	Dependent variable is trust in:		
	President	Government	Parliament
	(1)	(2)	(3)
Post first domestic revelation (Avg. ATT)	-0.238**	-0.344***	-0.233**
	(0.105)	(0.081)	(0.097)
Country FE	Yes	Yes	Yes
Day FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	6135	6085	6056

The table reports estimates based on the estimator developed by Callaway and Sant'Anna (2021) using data from the LiTS III sample. Control variables are: age, gender, educational level, whether respondents have children, whether they are members of a political party, whether they live in a rural area, and whether the respondents' parents were raised in a rural area. Robust standard errors are reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Alternative standard errors are reported in Online Appendix Table A9.

Table 5: Panama Papers scandal and institutional trust in the BES sample

	Dependent variable is:			
	Like Cameron	Like Conserv.	Trust MPs	Gov. Approval
	(1)	(2)	(3)	(4)
Post disclosure	-0.077***	-0.041***	-0.121***	-0.054***
	(0.005)	(0.005)	(0.008)	(0.013)
Individual FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
DV mean	0.441	0.444	0.461	0.432
Observations	103,967	68,771	59,885	28,658

The table reports within-respondent estimates using data from the British Election Study. Time-variant control variables are: income, age, and whether respondents have children. Standard errors are clustered at the individual level and reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Panama Papers scandal and institutional trust in the EB sample

	Dependent variable is trust in:			
	Full sample		Galicia vs Norte PT	
	(1)	(2)	(3)	(4)
	Government	Parliament	Government	Parliament
Post × Domestic-coverage share	−0.042	−0.032		
	(0.011)***	(0.011)***		
	[0.070]	[0.052]		
Post × Galicia			−0.254	−0.120
			(0.034)***	(0.032)***
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Survey FE	Yes	Yes	Yes	Yes
DV mean	0.351	0.344	0.325	0.258
Observations	160,688	159,649	3,523	3,501

Columns 1 and 2 use the full Eurobarometer sample, which comprises 24 European countries. Heteroskedasticity-robust standard errors are reported in rounded parentheses. Standard errors clustered at the country level are shown in squared brackets. Columns 3 and 4 restrict the sample to the regions of Galicia and Norte and only report heteroskedasticity-robust standard errors. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls include respondents' income, age, sex, rural residence, unemployment, education, and whether respondents have children.

Table 7: Panama Papers scandal and democracy assessment

	Dependent variable is:			
	Democracy is the most preferred political system	Satisfaction with democracy		
	LiTS	BES	EB	
			Full sample	Galicia vs Norte
	(1)	(2)	(3)	(4)
Post first domestic revelation (Avg. ATT)	0.189**			
	(0.076)			
Post disclosure		0.038***		
		(0.008)		
Post × Domestic-coverage share			−0.072	
			(0.013)***	
			[0.065]	
Post × Galicia				−0.200
				(0.041)***
Country FE	Yes	No	Yes	Yes
Day FE	Yes	No	No	No
Individual FE	No	Yes	No	No
Wave FE	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes
DV mean	0.487	0.501	0.526	0.396
Observations	5,894	77,356	159,848	3,627

Column 1 reports estimates based on the estimator developed by Callaway and Sant'Anna (2021) using data from the LiTS III sample. Control variables are: age, gender, educational level, whether respondents have children, whether they are members of a political party, whether they live in a rural area, and whether the respondents' parents were raised in a rural area. Robust standard errors are reported in parentheses. Alternative standard errors are reported in Online Appendix Table A9's row 4. Column 2 reports within-respondent estimates using data from the British Election Study. Time-variant control variables are: income, age, and whether respondents have children. Standard errors are clustered at the individual level and reported in parentheses. Column 3 uses the full Eurobarometer sample, which comprises 24 European countries. Heteroskedasticity-robust standard errors are reported in rounded parentheses. Standard errors clustered at the country level are shown in squared brackets. Column 4 restricts the sample to the regions of Galicia and Norte and only reports heteroskedasticity-robust standard errors. Controls include respondents' income, age, sex, rural residence, unemployment, education, and whether respondents have children. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Panama Papers scandal and democratic resilience: LiTS III

	V-Dem level		
	Low	High	Difference
Panel A: Trust in President			
Post first domestic revelation (Avg. ATT)	-1.229***	-0.356	0.873***
	(0.063)	(0.221)	(0.230)
Observations	1162	3218	4380
Panel B: Trust in Government			
Post first domestic revelation (Avg. ATT)	-0.248***	0.153	0.401**
	(0.074)	(0.156)	(0.173)
Observations	1162	3203	4365
Panel C: Trust in Parliament			
Post first domestic revelation (Avg. ATT)	-0.259***	-0.486*	-0.227
	(0.060)	(0.260)	(0.267)
Observations	1152	3206	4358

The table reports estimates based on the estimator developed by Callaway and Sant'Anna (2021) using data from the LiTS III sample. Regressions are estimated separately for countries with a high V-Dem score (above the unweighted respondent median) and with a low V-Dem score (below the unweighted respondent median). Control variables are: age, gender, educational level, whether respondents have children, whether they are members of a political party, whether they live in a rural area, and whether the respondents' parents were raised in a rural area. Robust standard errors are reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

Offshore Wealth, Onshore Distrust: Evidence from the Panama Papers Scandal

Alessandro Belmonte, Giuseppe Spataro, and Davide Ticchi

A Additional tables and figures

Table A1: Descriptive Statistics — LiTS III

	N	Mean	Median	St. dev.	Min	Max
Age	6751	47.000	46	16.673	18	91
Sex	6751	0.401	0	0.490	0	1
Number of Children	6751	0.954	1	1.072	0	8
Education	6751	4.770	5	1.390	1	8
Rural	6751	0.423	0	0.494	0	1
Mother is rural	6751	0.616	1	0.486	0	1
Father is rural	6751	0.624	1	0.484	0	1
Party member	6751	0.041	0	0.198	0	1

Table A2: Education — Categorical variable (LiTS III)

Value	Category
1	No degree/No education
2	Primary education
3	Lower secondary education
4	(Upper) secondary education
5	Post-secondary non-tertiary education
6	Tertiary education (not a university diploma)
7	Bachelor's degree or more
8	Master's degree or PhD

Table A3: Descriptive Statistics — BES

	N	Mean	Median	St. dev.	Min	Max
Age	70595	51.483	54	15.690	16	97
Income level	70595	6.909	6	3.555	1	15
Children proxy	70595	1.378	1	0.804	1	6

Table A4: Income level — Categorical variable (BES)

Value	Category
1	under £5,000 per year
2	£5,000 to £9,999 per year
3	£10,000 to £14,999 per year
4	£15,000 to £19,999 per year
5	£20,000 to £24,999 per year
6	£25,000 to £29,999 per year
7	£30,000 to £34,999 per year
8	£35,000 to £39,999 per year
9	£40,000 to £44,999 per year
10	£45,000 to £49,999 per year
11	£50,000 to £59,999 per year
12	£60,000 to £69,999 per year
13	£70,000 to £99,999 per year
14	£100,000 to £149,999 per year
15	£150,000 and over

Table A5: Descriptive statistics — Eurobarometer

Panel A: Income		
	Frequency	Percentage
The working class of society	47,708	29.883
The lower middle class of society	25,940	16.248
The middle class of society	72,823	45.614
The upper middle class of society	10,057	6.299
The higher class of society	1,092	0.684
Other	344	0.215
None	1,685	1.055

Panel B: Age		
	Mean	Median
Age	50.205	50.000

Panel C: Rural		
	Frequency	Percentage
Rural area or village	47,171	29.547
Small or middle sized town	66,882	41.893
Large town	45,596	28.560

Panel D: Unemployed		
	Frequency	Percentage
Employed	79,072	49.529
Unemployed	21,049	13.185
Inactive	59,528	37.287

Panel E: Education		
	Frequency	Percentage
Up to 14 years	15,292	9.579
15 years	9,358	5.862
16 years	10,484	6.567
17 years	9,136	5.723
18 years	31,321	19.619
19 years	16,657	10.434
20 years	8,321	5.212
21 years	6,632	4.154
22 years and older	37,606	23.555
Other	14,842	9.297

Panel F: Children		
	Frequency	Percentage
No	101,112	63.334
Yes	57,618	36.090
Other	919	0.576

Table A6: Descriptive statistics — Trust in institutions

Panel A: LITS

Trust in President

	Frequency	Percentage
No	3,915	57.212
Yes	2,928	42.788

Trust in Parliament

	Frequency	Percentage
No	4,920	72.878
Yes	1,831	27.122

Trust in Government

	Frequency	Percentage
No	4,654	68.613
Yes	2,129	31.387

Panel B: BES

Trust in MPs

	Frequency	Percentage
No	71,590	76.550
Yes	21,930	23.450

Panel C: EB

Trust in Parliament

	Frequency	Percentage
Tend not to trust	104,735	65.603
Tend to trust	54,914	34.397

Trust in Government

	Frequency	Percentage
Tend not to trust	104,364	64.948
Tend to trust	56,324	35.052

Table A7: Descriptive statistics — Democratic assessment

	N	Mean	Median	St. dev.	Min	Max
			Panel A: LiTS			
Democracy preferable	5894	0.487	0.000	0.500	0.000	1.000
			Panel B: BES			
Satisfied with democracy	88075	0.500	1.000	0.500	0.000	1.000
			Panel C: Eurobarometer			
Satisfied with democracy	159848	0.526	1.000	0.499	0.000	1.000

Table A8: Newspaper readership rates in the BES sample (among the readers)

Newspaper	**Readership share**	**Cumulative share**
The Daily Mail	20.30	20.30
The Sun	15.93	36.23
The Guardian	12.07	48.30
The Mirror	10.21	58.51
The Daily Telegraph	7.35	65.86
The Times	7.06	72.92
The Independent	3.12	76.04

Table A9: Alternative clustering approach to estimates using the LiTS III sample

	ATT	p Robust	p Country-clustered	Wild-cluster 95% CI
Trust in President	-0.238	0.024	0.068	$[-0.464, -0.013]$
Trust in Government	-0.344	< 0.001	< 0.001	$[-0.478, -0.210]$
Trust in Parliament	-0.233	0.016	0.095	$[-0.504, 0.038]$
Democracy is the most preferred political system	0.189	0.013	< 0.001	$[0.095, 0.283]$

ATT estimates using the Callaway–Sant'Anna `cs-did` command with individual controls, survey weights, doubly robust inverse-probability weighting, and not-yet-treated controls. The first three rows report the estimates displayed in Table 4's columns 1 to 3. The last row reports the estimate presented in Table 7's column 1. Robust and country-clustered p-values use asymptotic inference. The 95% intervals use 999 Rademacher wild-cluster bootstrap repetitions with country-level clustering (12 clusters).

Table A10: Panama Papers scandal and institutional trust in the LiTS III sample — Excluding control variables

	Dependent variable is trust in:		
	President	Government	Parliament
	(1)	(2)	(3)
Post first domestic revelation (Avg. ATT)	-0.243**	-0.298***	-0.174*
	(0.103)	(0.083)	(0.093)
Country FE	Yes	Yes	Yes
Day FE	Yes	Yes	Yes
Controls	No	No	No
Observations	6,659	6,606	6,576

The table reports estimates based on the estimator developed by Callaway and Sant'Anna (2021) using data from the LiTS III sample. Robust standard errors are reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Panama Papers scandal and institutional trust in the LiTS III sample — TWFE estimates

	Dependent variable is trust in:		
	President	Government	Parliament
	(1)	(2)	(3)
Post first domestic revelation	-0.110***	-0.088***	-0.080***
	(0.032)	(0.031)	(0.029)
Country FE	Yes	Yes	Yes
Day FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
DV Mean	0.428	0.314	0.271
Observations	6,843	6,783	6,751

The table reports estimates based on the Two-Way Fixed Effects (TWFE) estimator using data from the LiTS III sample. Control variables are: age, gender, educational level, whether respondents have children, whether they are members of a political party, whether they live in a rural area, and whether the respondents' parents were raised in a rural area. Robust standard errors are reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Panama Papers scandal and institutional trust in the BES sample: getting informed

	Dependent variable is:			
	Like Cameron	Like Conserv.	Trust MPs	Gov. Approval
	(1)	(2)	(3)	(4)
Post disclosure	-0.071***	-0.053***	0.013	0.003
	(0.013)	(0.015)	(0.019)	(0.020)
Post disclosure × Got informed	-0.016	0.014	-0.035***	-0.065***
	(0.013)	(0.014)	(0.013)	(0.016)
Individual FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
DV Mean	0.444	0.442	0.464	0.286
Observations	89,379	58,795	57,892	34,569

The table reports within-respondent estimates using data from the British Election Study. Time-variant control variables are: income, age, and whether respondents have children. *Got informed* is a binary variable equal to 1 for respondents reporting spending any time following politics or current affairs through television, newspapers, radio, or the internet. Standard errors are clustered at the individual level and reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Panama Papers scandal and institutional trust in the BES sample: reading

	Dependent variable is:			
	Like Cameron	Like Conserv.	Trust MPs	Gov. Approval
	(1)	(2)	(3)	(4)
Post disclosure	-0.076***	-0.037***	-0.011	-0.043***
	(0.007)	(0.007)	(0.015)	(0.013)
Post disclosure × Readership	-0.015***	-0.004	-0.016**	-0.022***
	(0.005)	(0.006)	(0.007)	(0.007)
Individual FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
DV Mean	0.443	0.440	0.463	0.284
Observations	92,686	60,954	60,163	35,838

The table reports within-respondent estimates using data from the British Election Study. Time-variant control variables are: income, age, and whether respondents have children. *Readership* is a binary variable equal to 1 for respondents reporting reading any newspaper, including local ones. Standard errors are clustered at the individual level and reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Panama Papers scandal and democratic resilience: LiTS III — TWFE estimates

	Dependent variable is trust in:		
	President	Government	Parliament
	(1)	(2)	(3)
Post first domestic revelation	-0.092**	-0.034	-0.044
	(0.042)	(0.037)	(0.039)
Post first domestic revelation × V-Dem PCA 2015	0.023	0.058**	0.054*
	(0.031)	(0.029)	(0.030)
Country FE	Yes	Yes	Yes
Day FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
DV Mean	0.428	0.271	0.314
Observations	6,843	6,751	6,783

The table reports estimates based on the Two-Way Fixed Effects (TWFE) estimator using data from the LiTS III sample. Control variables are: age, gender, educational level, whether respondents have children, whether they are members of a political party, whether they live in a rural area, and whether the respondents' parents were raised in a rural area. Robust standard errors are reported in parentheses. Symbols: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: V-Dem principal component in LiTS III and Eurobarometer samples

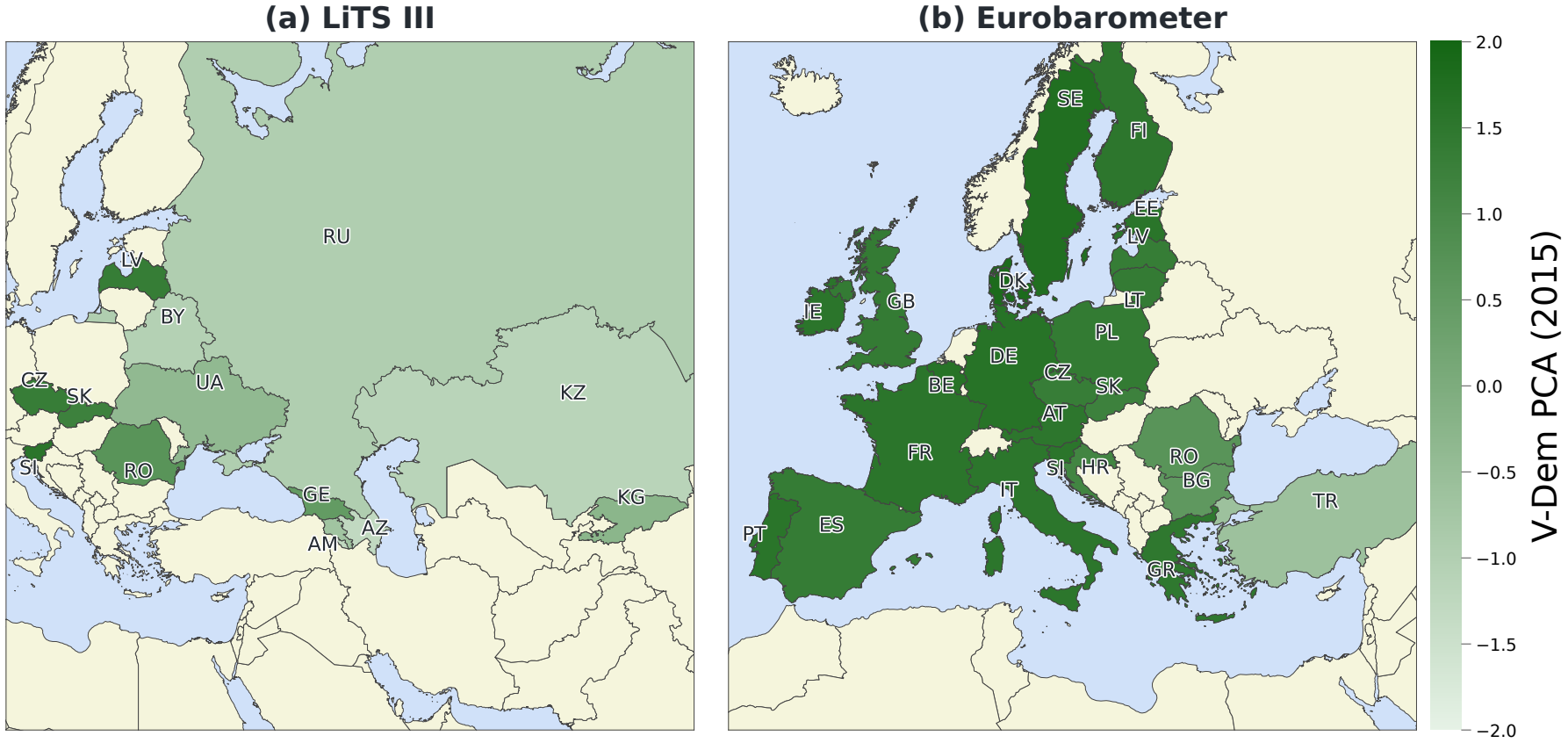
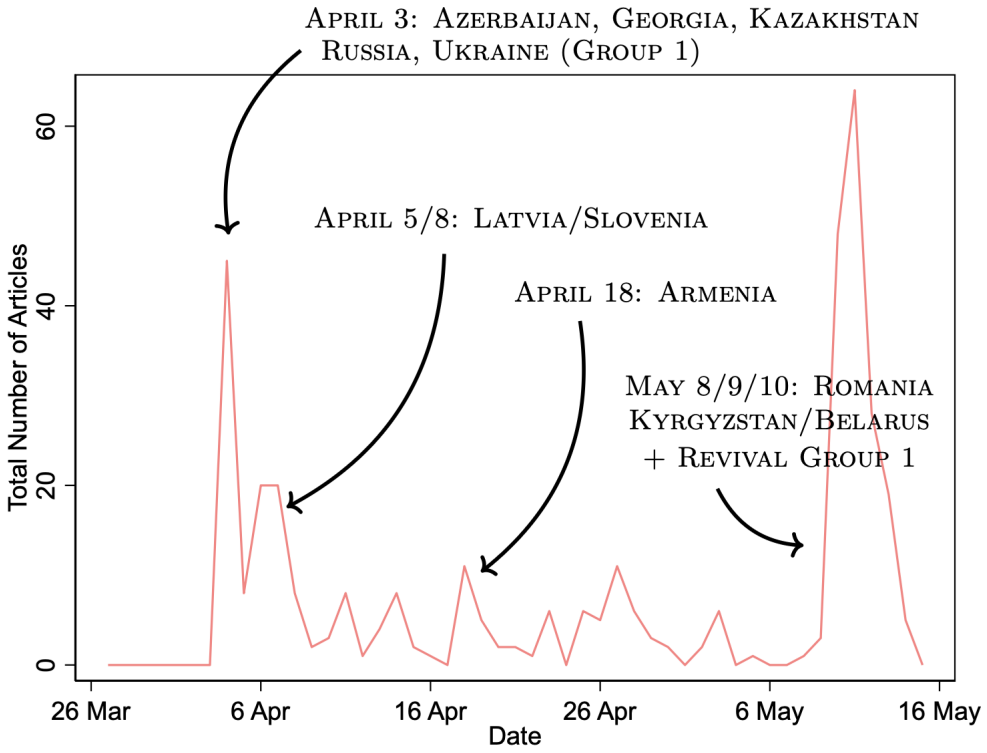
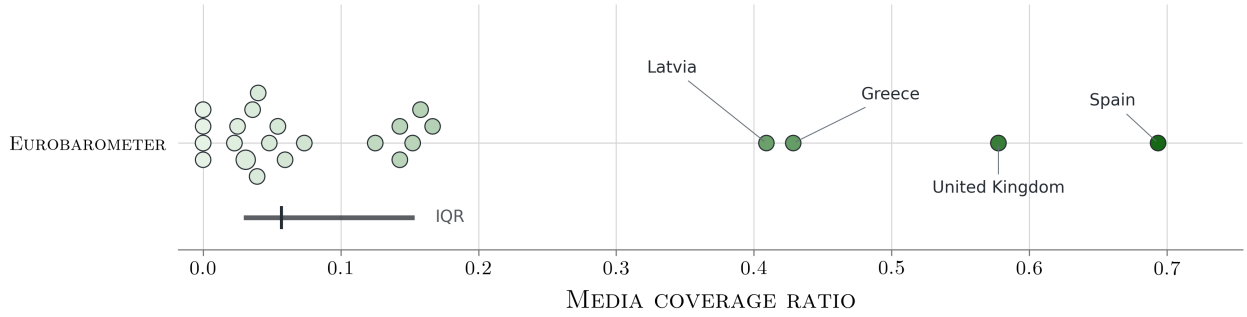


Figure A2: Media coverage of the scandal in the LiTS III sample



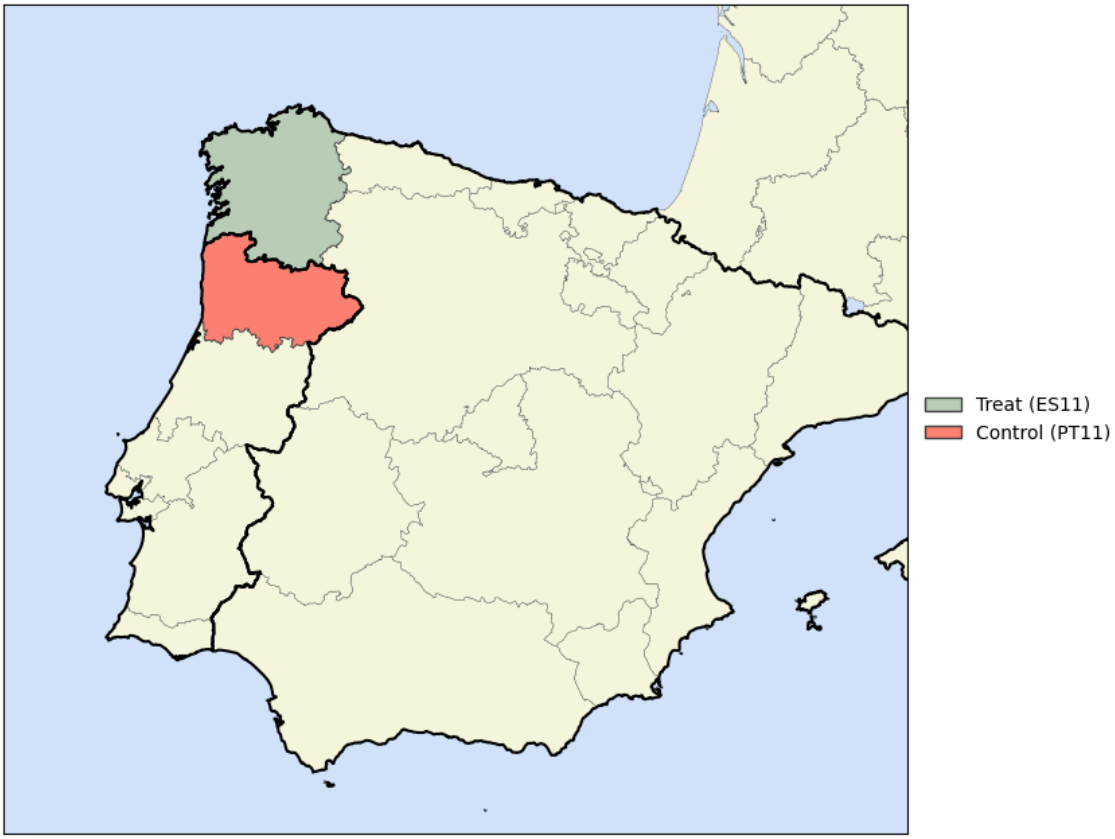
Notes. The figure reports the total number of articles covering domestic political officials involved in the Panama Papers scandal in countries that are in the LiTS III sample.

Figure A3: Cross-country distribution in the media coverage index — Eurobarometer



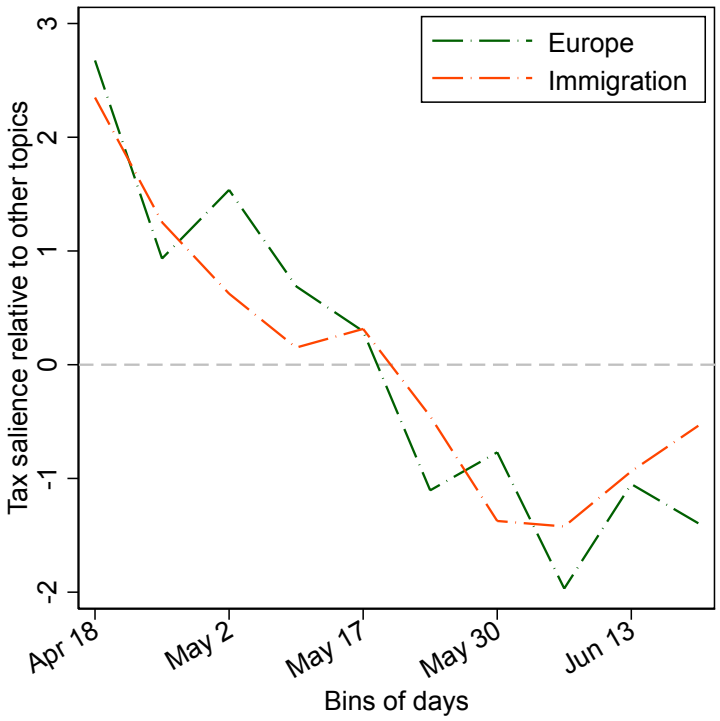
Notes. The figure reports the distribution of countries in the Eurobarometer sample according to the ratio of articles covering domestic political involvement to the total number of articles covering the Panama Papers in that country. Darker shades indicate higher exposure of domestic political figures in the scandal.

Figure A4: Comparing the regions of Galicia (Spain) and Norte (Portugal)



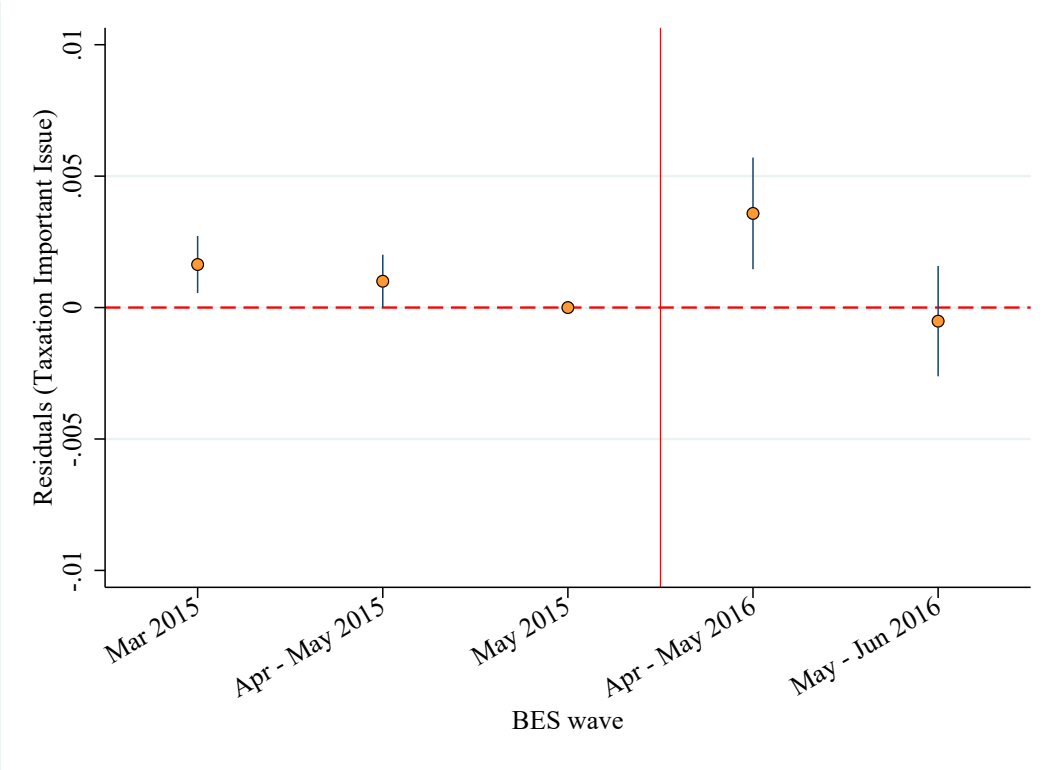
Notes. The figure illustrates the Spanish region of Galicia in green and the Portuguese region of Norte in red.

Figure A5: Relative salience of taxation — BES



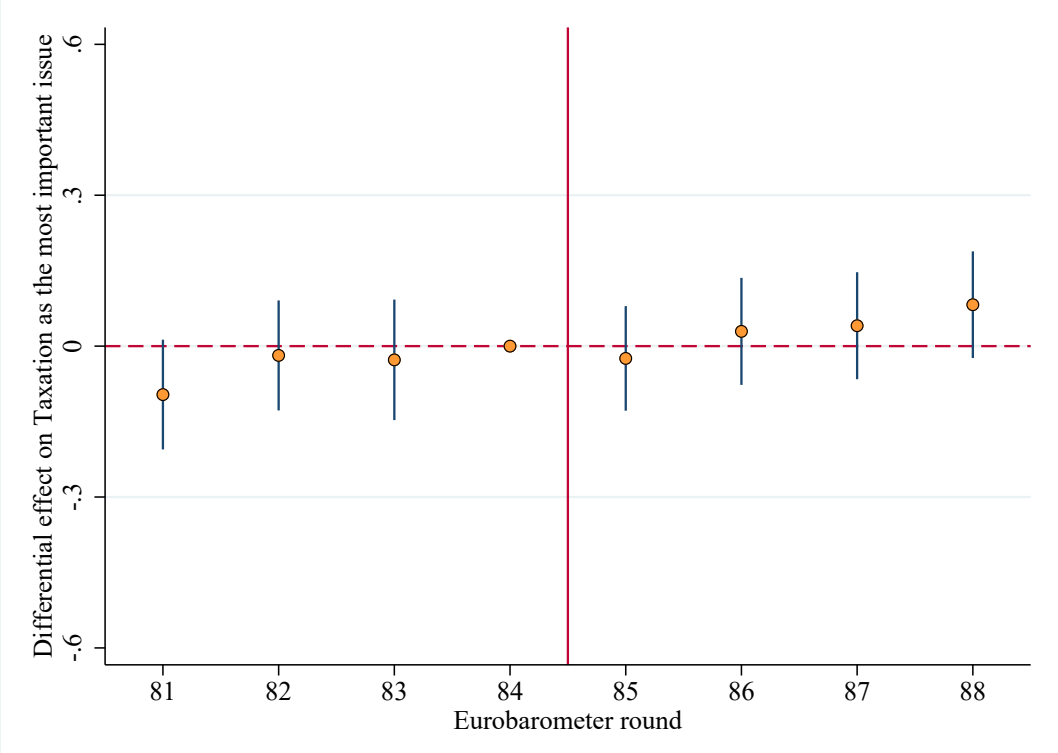
Notes. The figure displays the daily differences collected during BES Waves 7 and 8 of the standardized share of respondents mentioning taxation and of mentioning Europe (in green) and immigration (in orange), respectively. Positive values indicate that taxation was unusually salient relative to the comparison topic, given the typical variation in each series.

Figure A6: Event study estimates for salience of taxation — BES



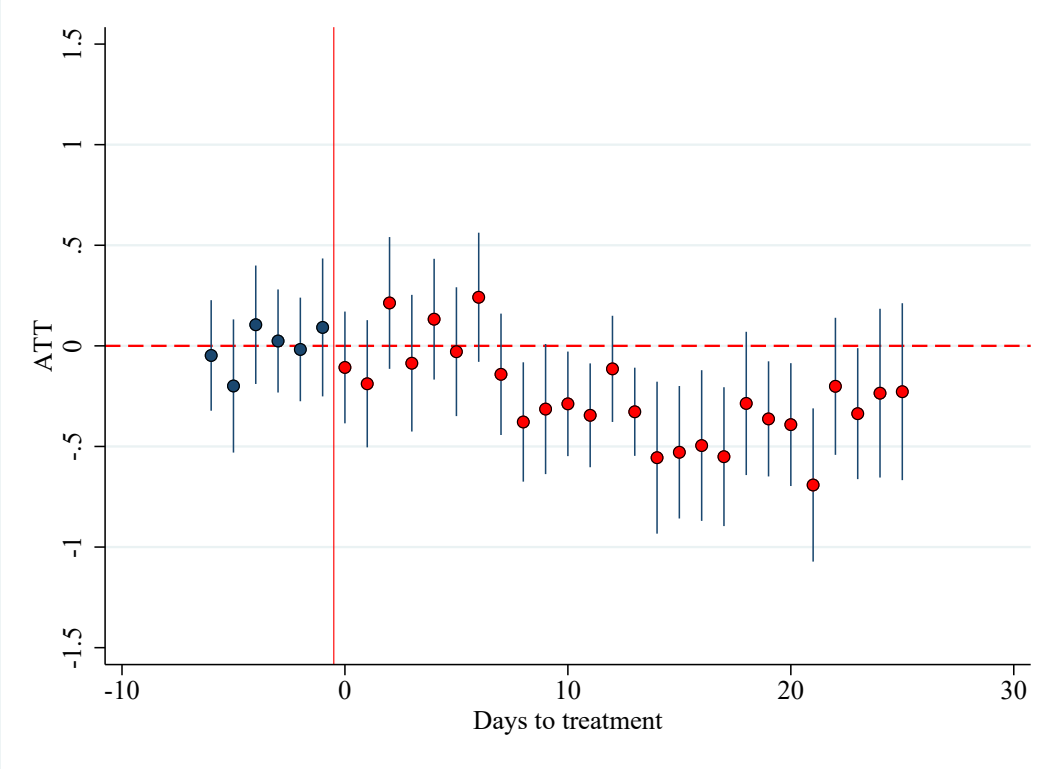
Notes. The figure reports dynamic estimations, as described in Equation 3, based on Table 3's column 1 model.

Figure A7: Event study estimates for salience of taxation — Eurobarometer



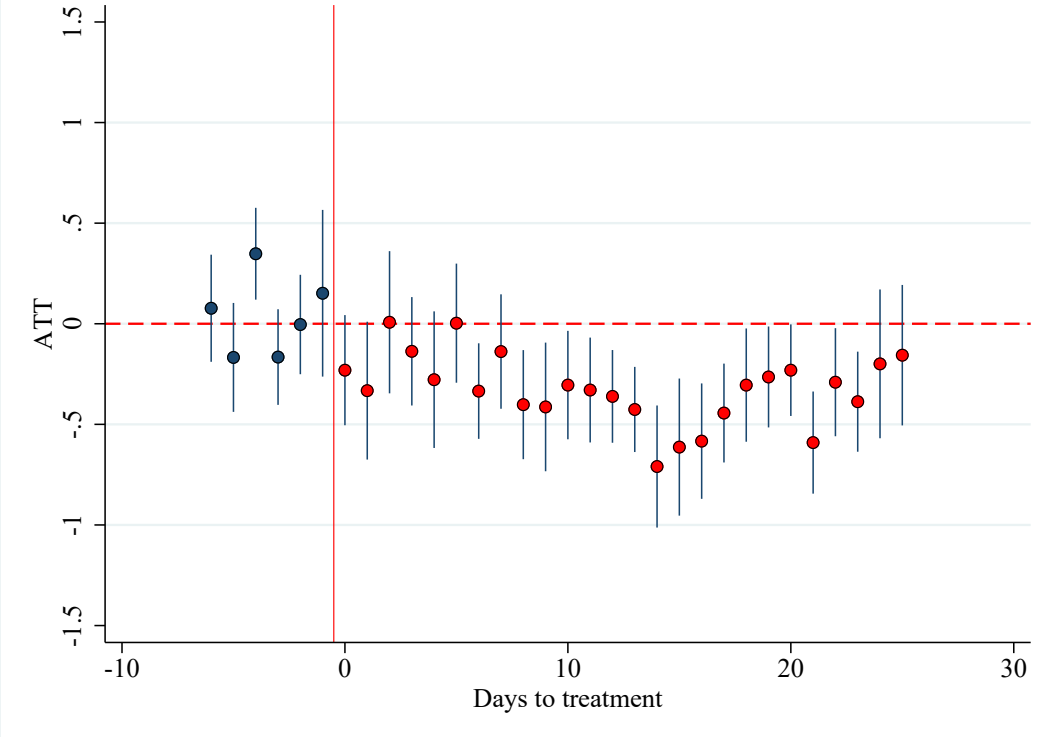
Notes. The figure reports dynamic estimations, as described in Equation 5, based on Table 3's column 3 model.

Figure A8: Event study estimates of the dynamic effects on trust in the president — LiTS III



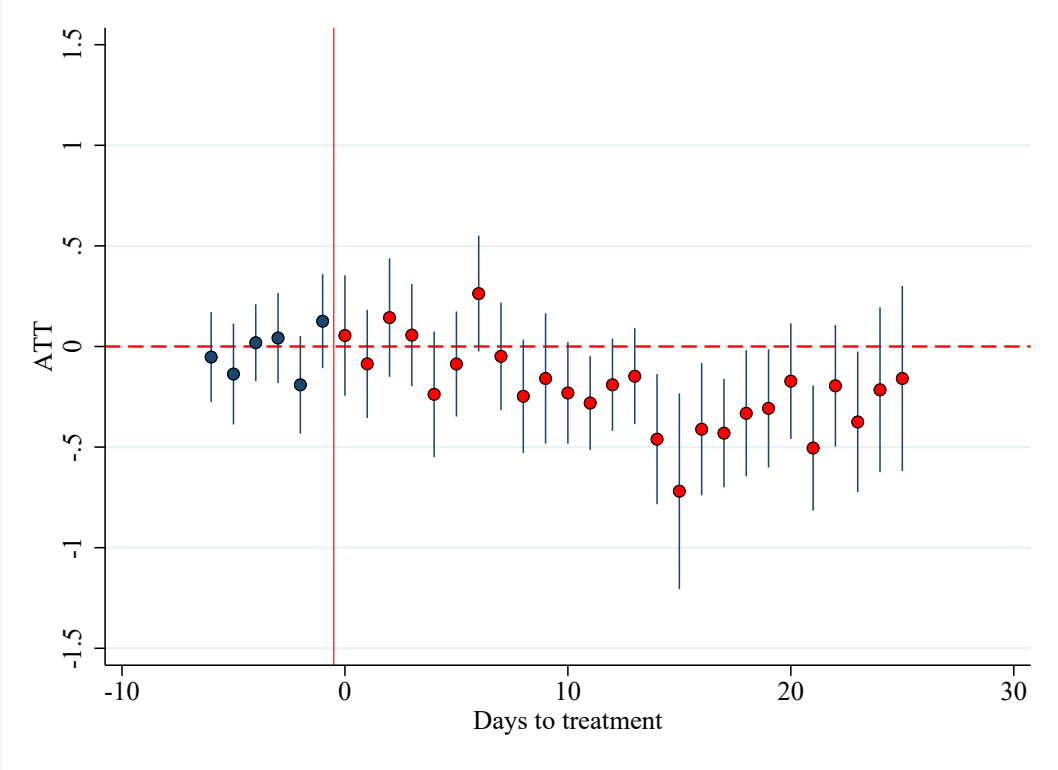
Notes. The figure reports dynamic estimates based on Table 4's column 1 model.

Figure A9: Event study estimates of the dynamic effects on trust in the government — LiTS III



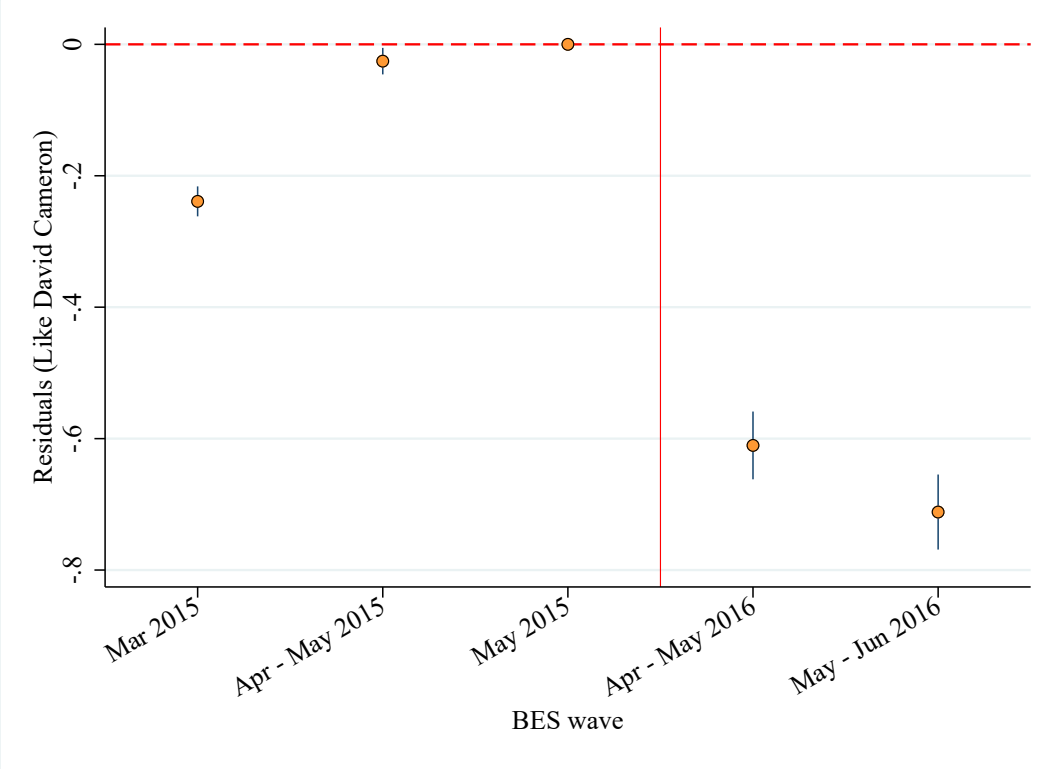
Notes. The figure reports dynamic estimates based on Table 4's column 2 model.

Figure A10: Event study estimates of the dynamic effects on trust in Parliament — LiTS III



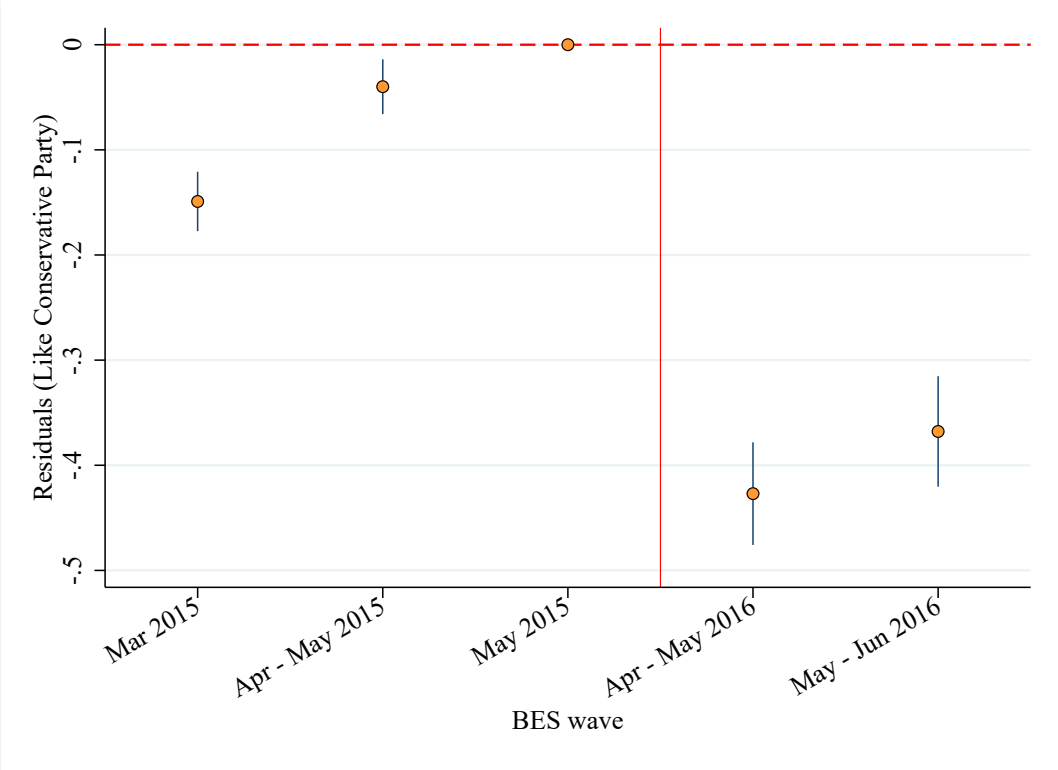
Notes. The figure reports dynamic estimates based on Table 4's column 3 model.

Figure A11: Event study estimates for Cameron approval — BES



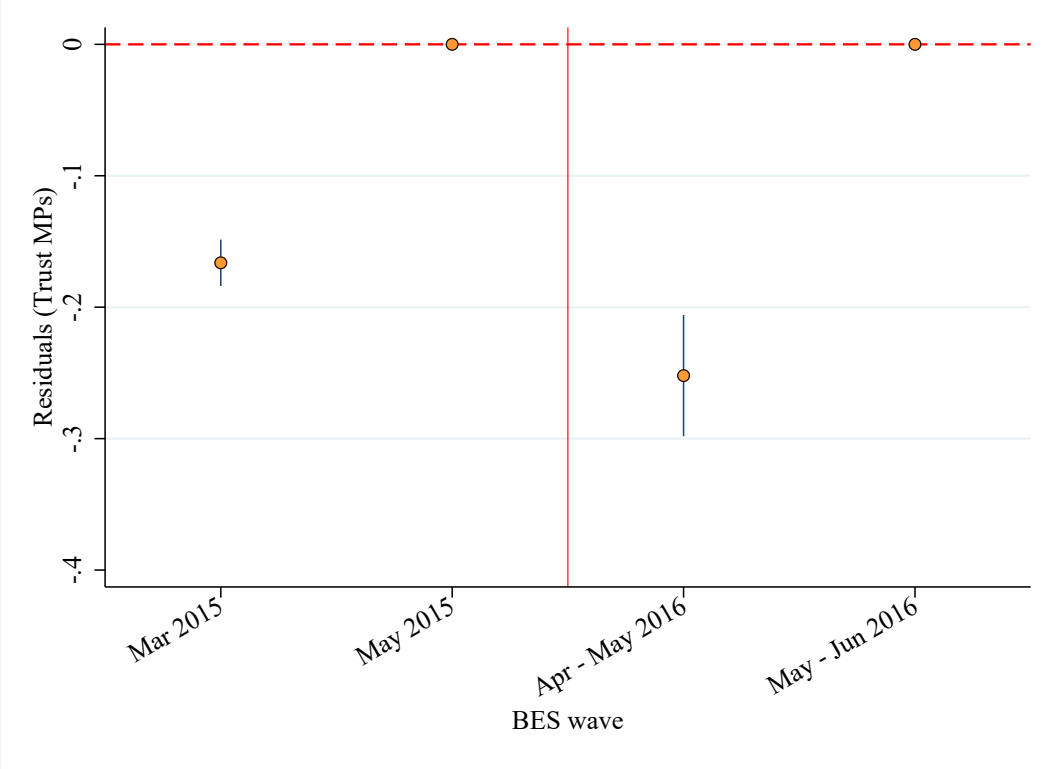
Notes. The figure reports dynamic estimations, as described in Equation 3, based on Table 5's column 1 model.

Figure A12: Event study estimates for the Conservative Party approval — BES



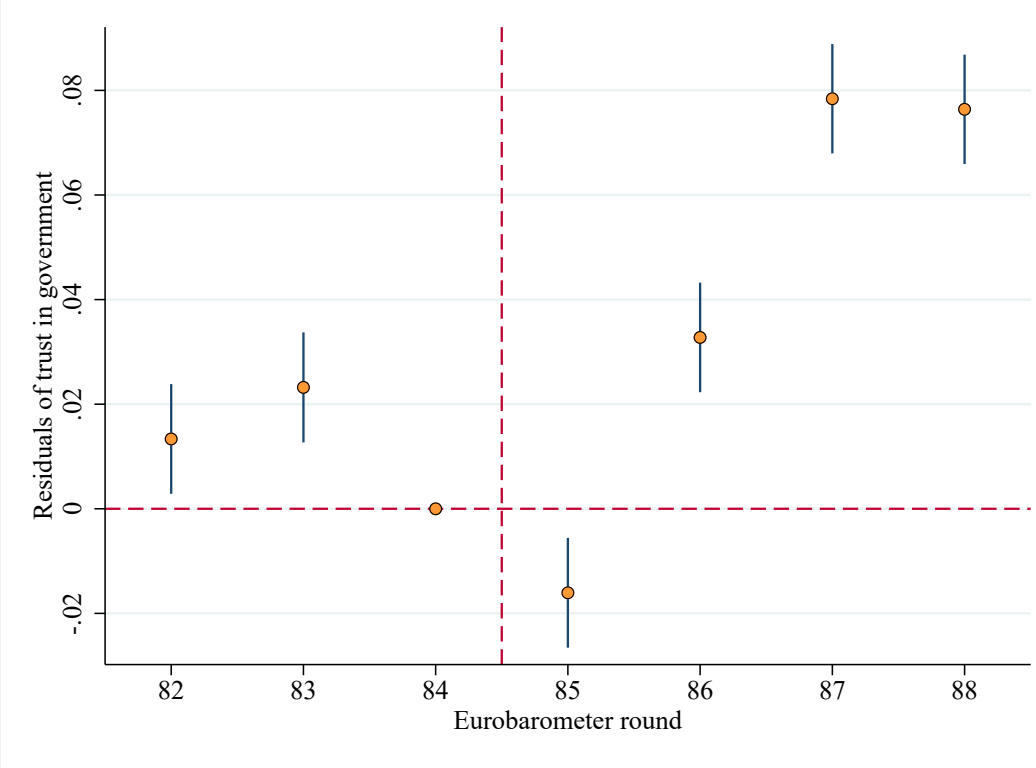
Notes. The figure reports dynamic estimations, as described in Equation 3, based on Table 5's column 2 model.

Figure A13: Event study estimates for trust in members of Parliament — BES



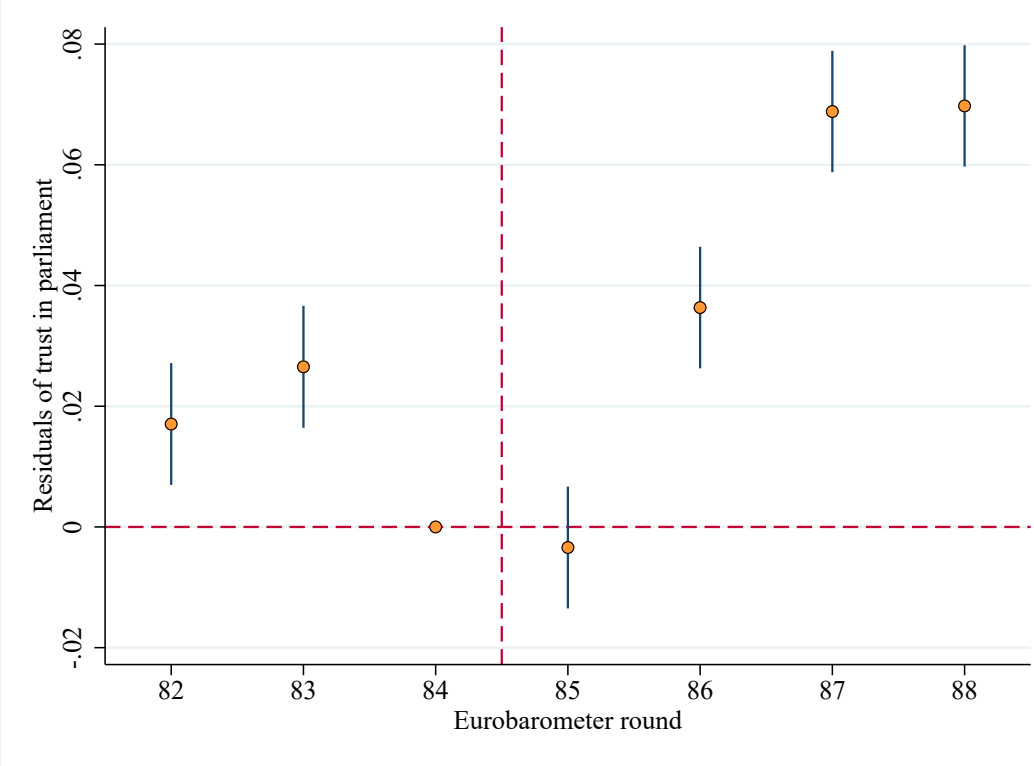
Notes. The figure reports dynamic estimations, as described in Equation 3, based on Table 5's column 3 model.

Figure A14: Event study estimates (residuals) for trust in the government — Eurobarometer



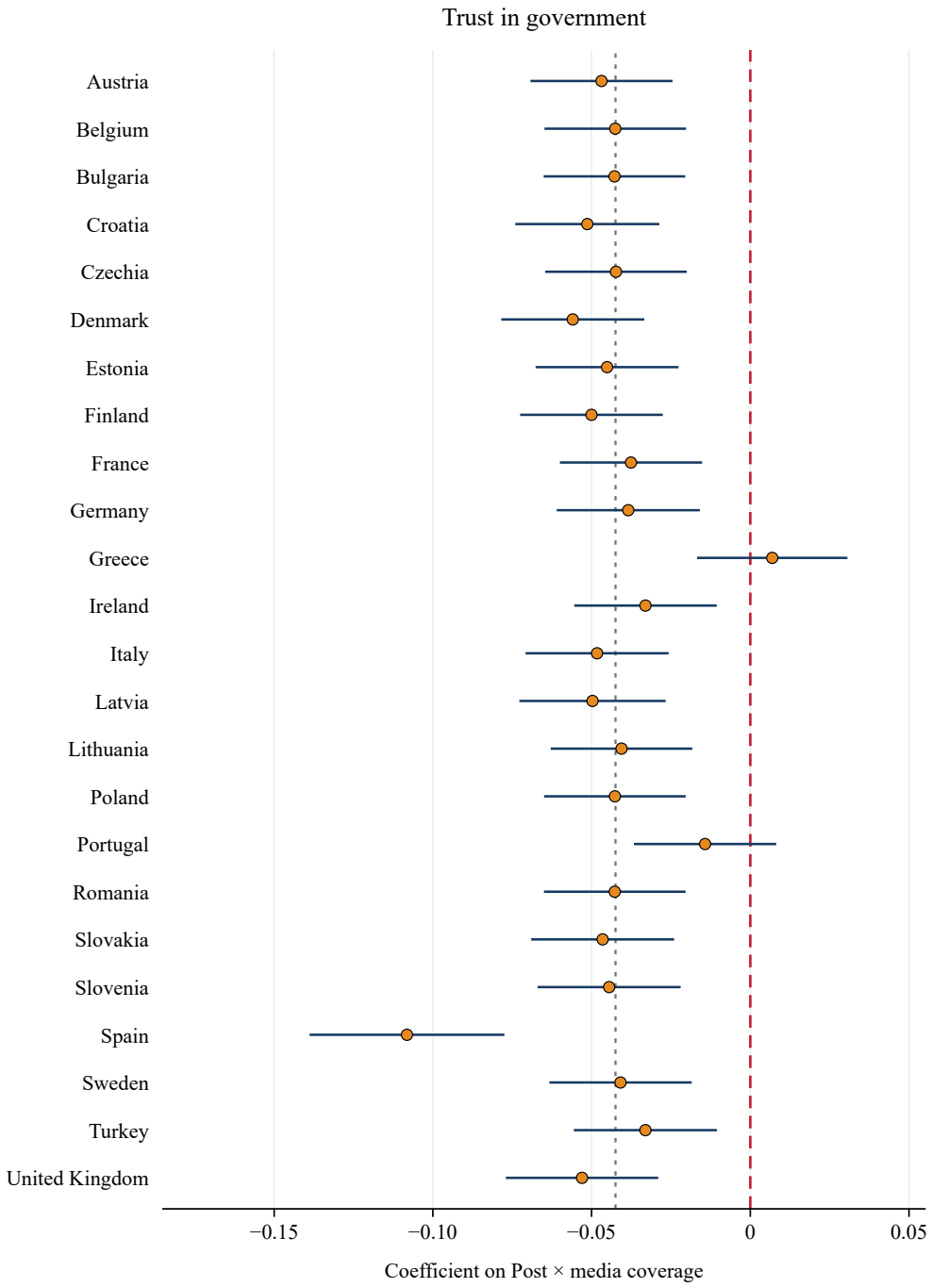
Notes. The figure reports dynamic estimations, as described in Equation 5, using only Eurobarometer survey round dummies as explanatory variables. The remainder is equivalent to column 1 of Table 6.

Figure A15: Event study estimates (residuals) for trust in Parliament — Eurobarometer



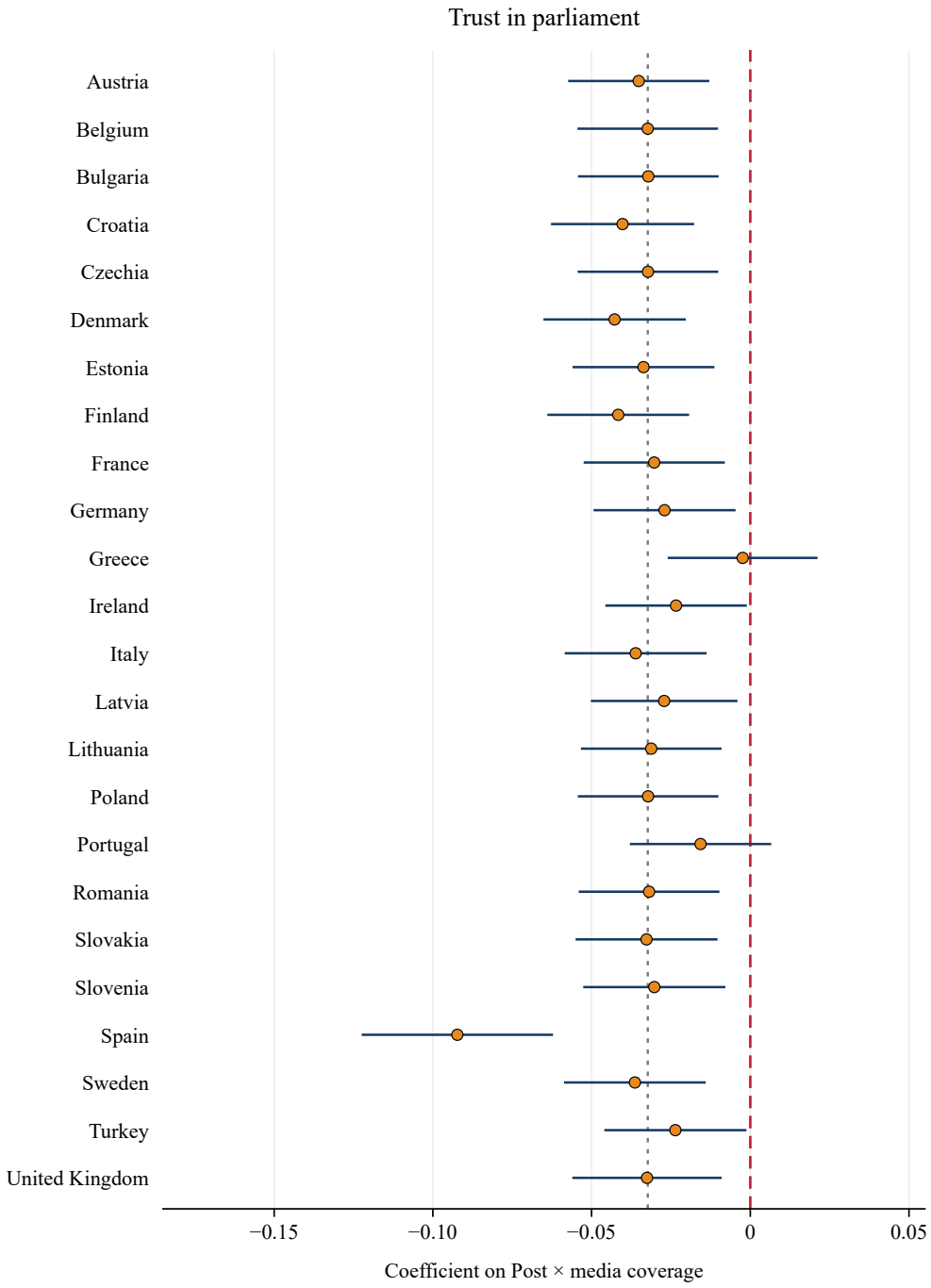
Notes. The figure reports dynamic estimations, as described in Equation 5, using only Eurobarometer survey round dummies as explanatory variables. The remainder is equivalent to column 2 of Table 6.

Figure A16: Leave-one-country-out sensitivity of the estimates for trust in the government — Eurobarometer



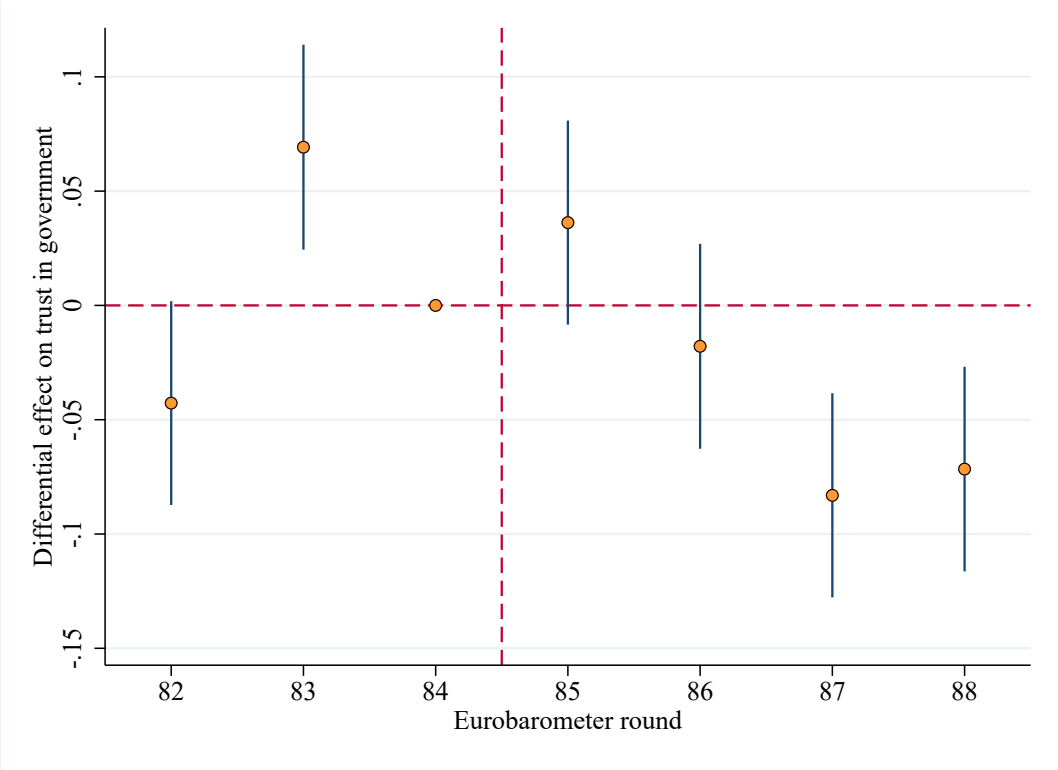
Notes. The figure reports diagnostics of the estimated coefficient reported in column 1 of Table 6. The y-axis indicates the omitted country.

Figure A17: Leave-one-country-out sensitivity of the estimates for trust in Parliament — Eurobarometer



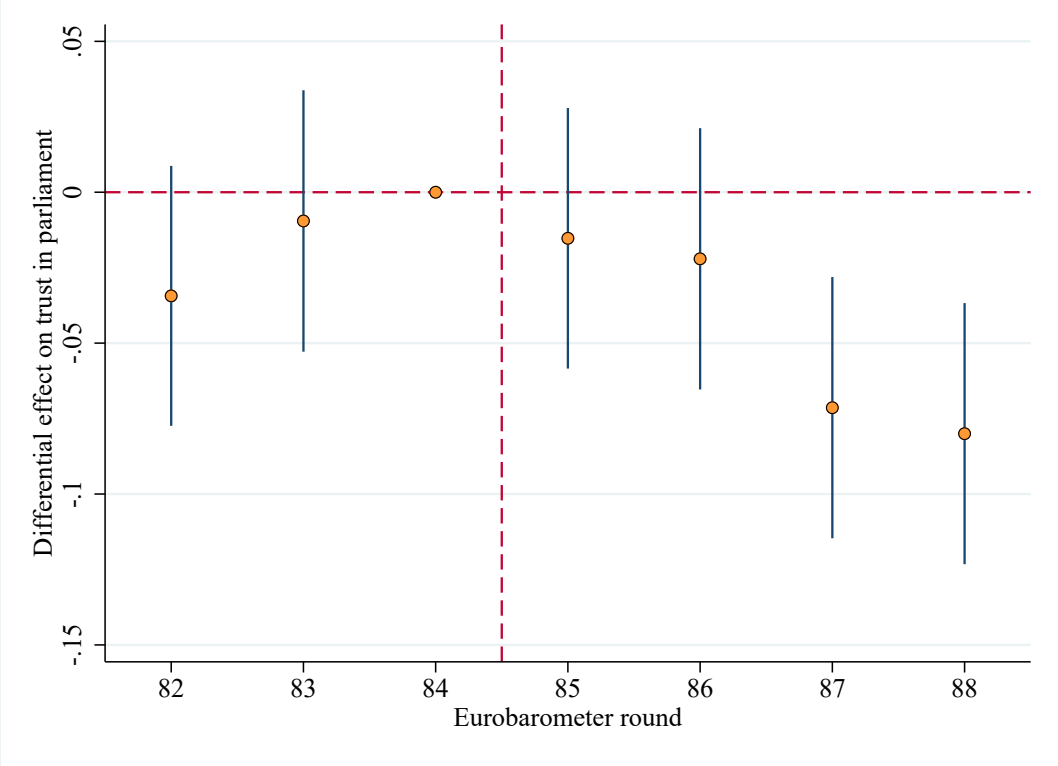
Notes. The figure reports diagnostics of the estimated coefficient reported in column 2 of Table 6. The y-axis indicates the omitted country.

Figure A18: Event study estimates for trust in the government — Eurobarometer



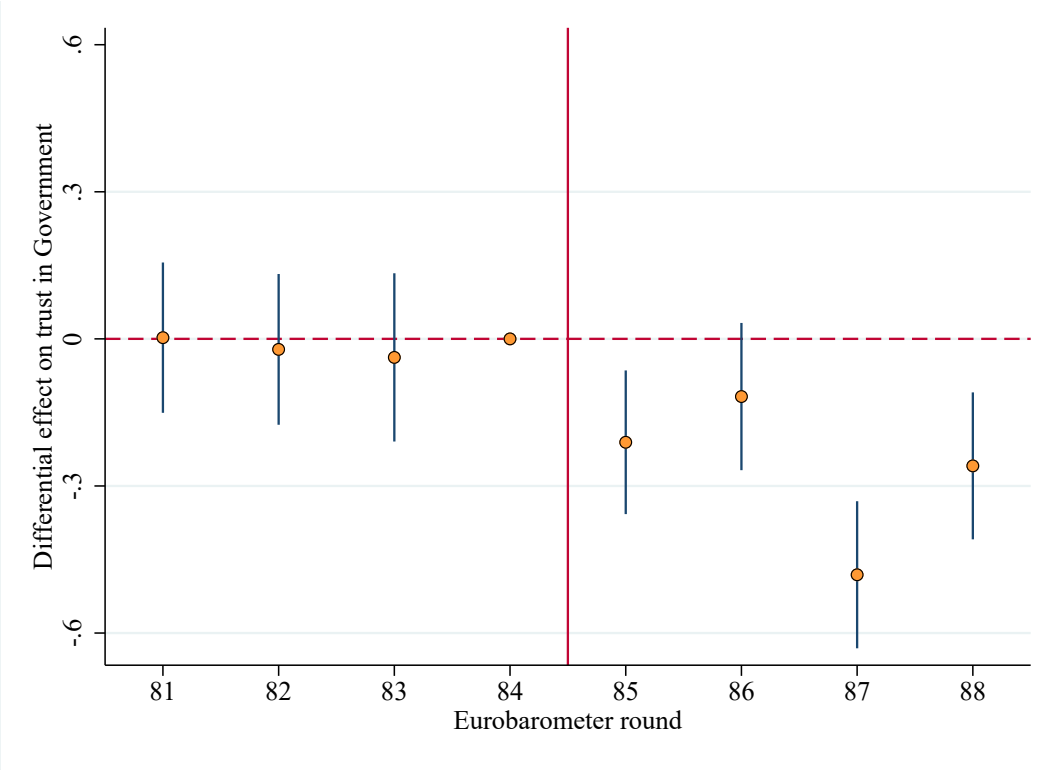
Notes. The figure reports dynamic estimations, as described in Equation 4, based on Table 6's column 1 model.

Figure A19: Event study estimates for trust in Parliament — Eurobarometer



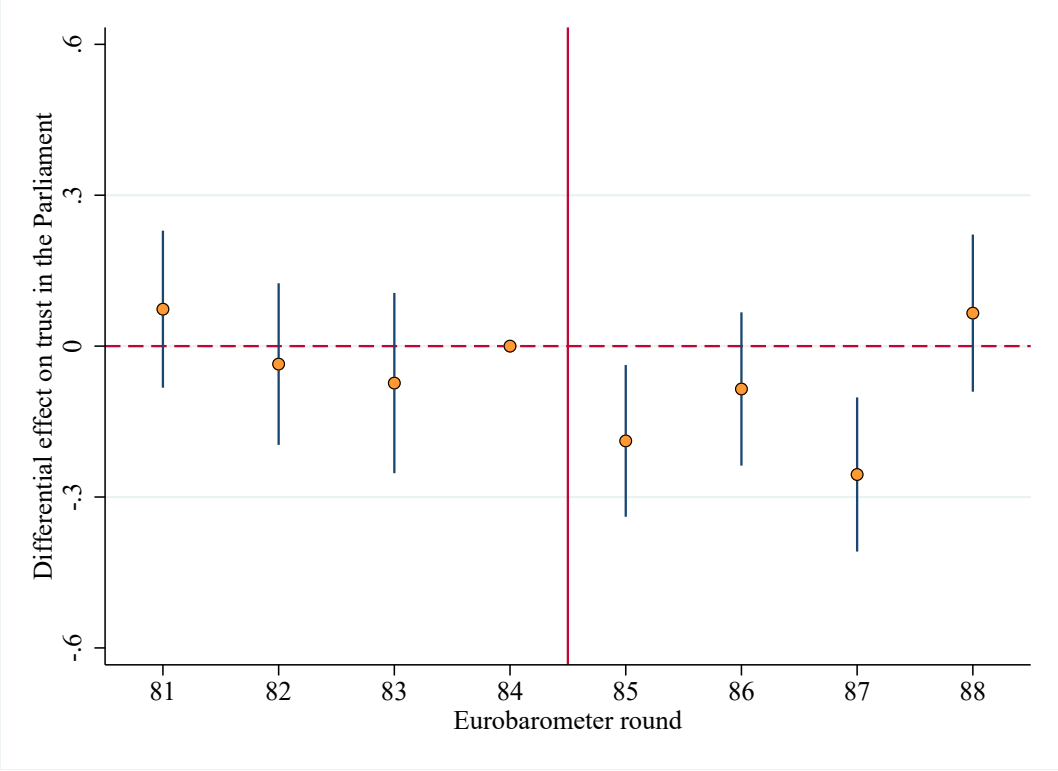
Notes. The figure reports dynamic estimations, as described in Equation 4, based on Table 6's column 2 model.

Figure A20: Event study estimates for trust in the government — Galicia vs Norte



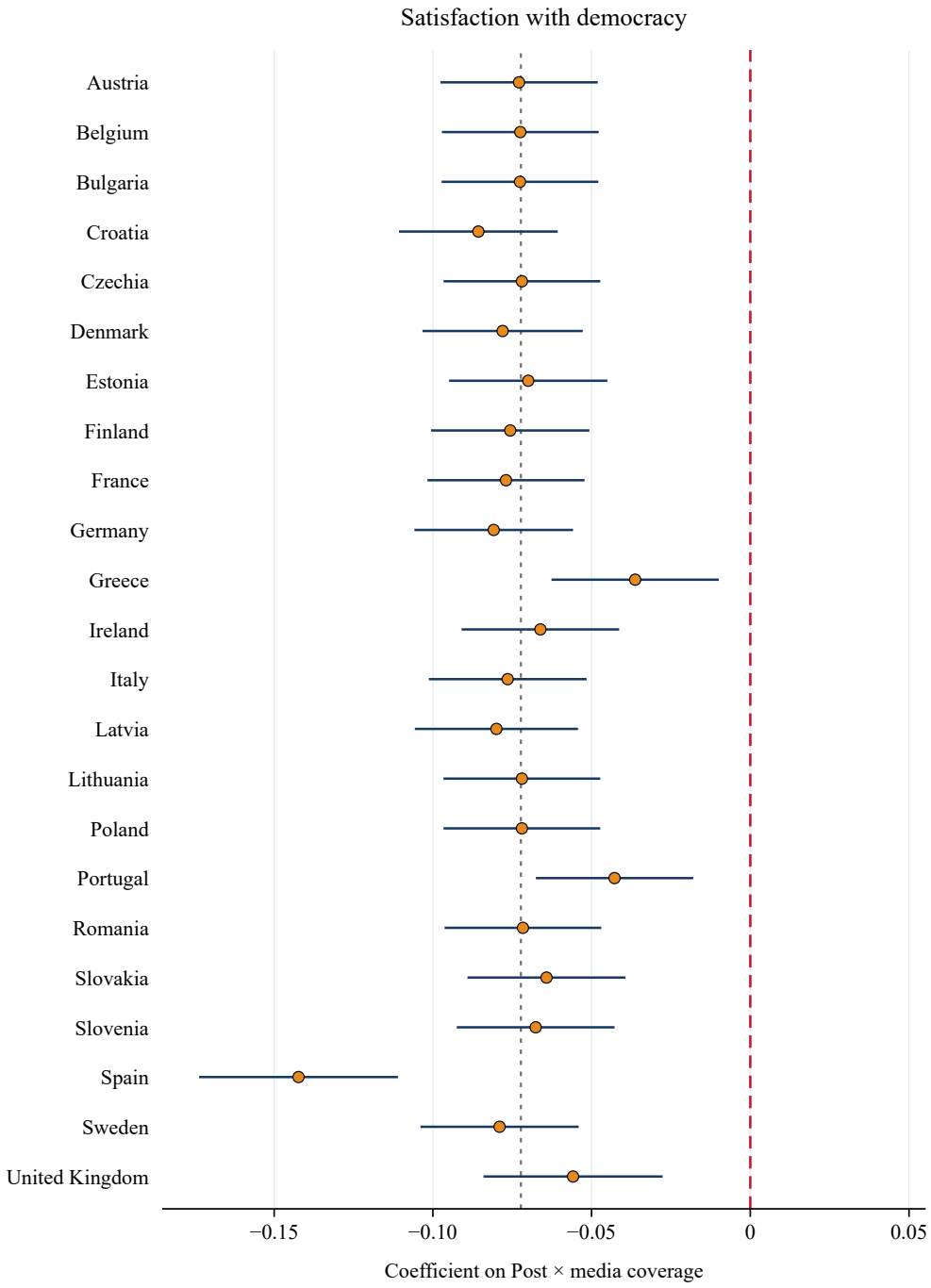
Notes. The figure reports dynamic estimations, as described in Equation 4, based on Table 6's column 3 model.

Figure A21: Event study estimates for trust in Parliament — Galicia vs Norte



Notes. The figure reports dynamic estimations, as described in Equation 4, based on Table 6's column 4 model.

Figure A22: Leave-one-country-out sensitivity of the estimates for democratic assessment — Eurobarometer



Notes. The figure reports diagnostics of the estimated coefficient reported in column 2 of Table 7. The y-axis indicates the omitted country.

B Technical details on media-data collection and classification

As described in Section 3.4, we use news coverage to measure both the country-specific timing and the intensity of exposure to the Panama Papers scandal. This appendix provides additional details on the selection of media sources, the retrieval and classification of articles, the construction of the exposure measures used in the empirical analysis, and the validation of the classification procedure.

B.1 Media sources and article retrieval

We retrieve news articles from Factiva, a database providing access to newspapers, news agencies, and other media sources worldwide. For the LiTS III and Eurobarometer countries, we use the BBC Monitoring Media Guides to identify reliable national sources of political information. Where Factiva provides sufficiently extensive coverage from a national news agency, we use that agency as our primary source. Where such coverage is limited, typically in smaller countries, we instead use a leading national newspaper available through Factiva. Countries are retained only when a suitable national source provides sufficiently regular coverage over the period of interest. As Moldova does not, we remove this country from our sample.

For the United Kingdom, we adopt a separate source-selection criterion tailored to the British Election Study. We search the seven newspapers and tabloids with the highest reported readership in the BES sample: *The Daily Mail*, *The Sun*, *The Guardian*, *The Mirror*, *The Daily Telegraph*, *The Times*, and *The Independent*. Together, these outlets account for approximately 76 percent of declared newspaper readership.

The search window extends from March 25 to May 31, 2016. This interval allows us both to verify the absence of relevant coverage before the coordinated disclosure of the Panama Papers on April 3 and to capture domestic revelations that emerged only after journalists had examined the leaked documents for several days or weeks.

For LiTS III countries, some of whose selected sources publish in Russian, we use the following Factiva query:

'(панам* or panam* or fonseca) and rst= [COUNTRY OR NEW AGNECY CODE]'

For the remaining countries, we use:

'(panam* or fonseca) and rst= [COUNTRY OR NEW AGNECY CODE]'

The initial collection contains 5,090 articles across the three survey settings. The LiTS III and Eurobarometer corpora contain 1,148 and 4,127 articles, respectively. These survey-specific counts are not additive because some countries and media sources contribute to more than one survey-specific corpus. The initial collection is deliberately broad: it includes articles referring to Panama or to individuals named Fonseca but unrelated to the Panama Papers, as well as coverage of the global scandal or foreign political figures without any reference to domestic political involvement. We therefore classify the retrieved articles before constructing our exposure measures.

B.2 LLM-assisted article classification

We classify the retrieved articles using GPT-4o. The classification procedure has two objectives. First, we determine whether an article concerns the Panama Papers. Second, conditional on the article being related to the scandal, we identify the individuals directly implicated, their nationalities, and whether they have a political role.

To assess the stability of the classification, we process each article twice using the same prompt and a temperature parameter of 1. Disagreement between the two classifications is used as a trigger for manual review. Articles for which the two model outputs are inconsistent are inspected and assigned a final classification manually.

The prompt supplied to the model is:

> *Analyze the article and determine if it is related to the Panama Papers scandal. If so, extract and list the names of individuals mentioned, their nationality, and specify if they hold a political role. Only report those who are directly implicated in the scandal. Provide all information in English.*

We request the response in structured JSON format:

```
{
        "panama_papers": boolean,
        "people_implicated": list of strings,
        "nationality": list of strings,
        "political_role": list of boolean
}
```

The LLM output is used as a screening device rather than as an entirely automated final classification. We retain articles that concern the Panama Papers and implicate at least one domestic political figure. Consistent with the definition used in Section 3.4, domestic political involvement includes both political officeholders and politicians' close relatives when they are directly implicated in the reported offshore activities. The individual's nationality, political role, and relationship to domestic political figures are assessed using the information contained in the article. Ambiguous cases, as well as cases in which the two LLM classifications disagree, are resolved through manual inspection.

B.3 Construction of the exposure measures

We use the classified articles differently across the three empirical settings.

For LiTS III, our objective is to identify the first date on which the national media report the involvement of a domestic political figure. We therefore manually review the retained articles in chronological order and identify the first qualifying domestic revelation in each country. This date determines treatment onset in the staggered difference-in-differences analysis. Respondents interviewed before that date are classified as not yet exposed, whereas respondents interviewed on or after it are classified as exposed. If no qualifying article appears during the LiTS III fieldwork window, the country remains untreated during the estimation period. Table B1 reports the first qualifying article identified for each LiTS III country.

For Eurobarometer, our objective is instead to measure the intensity with which the scandal acquired a domestic political dimension. We therefore aggregate the classified articles by country and construct the exposure measure described in Section 4.3: the number of Panama Papers articles implicating at least one domestic political figure divided by the total number of articles concerning the Panama Papers in that country. Table B2 reports, by media source, the number of retrieved articles, the number classified as concerning the Panama Papers, and the number additionally implicating a domestic political figure. The latter category contains 474 articles, all of which are included in the overall sample of 1,003 articles classified as relevant across the three survey settings.

For the British Election Study, we aggregate relevant articles across the seven newspapers described above. We use the resulting daily series to characterize the timing and intensity of British media attention to domestic political involvement in the scandal. Figure 6 in the main text reports the daily average number of relevant articles per newspaper.

B.4 Validation of the classification procedure

We evaluate the accuracy of the LLM-assisted procedure using a validation sample of 100 articles from the Eurobarometer corpus. These articles are manually coded without consulting the corresponding LLM classifications. We assess performance separately for the two stages of the classification procedure.

For the first task—determining whether an article concerns the Panama Papers—the procedure achieves an overall accuracy of 92 percent. The class-specific F1 scores are 0.85 for articles not concerning the scandal and 0.95 for articles concerning the Panama Papers.

For the second task—determining whether an article implicates a domestic political figure—the procedure achieves an overall accuracy of 95 percent. The corresponding F1 scores are 0.97 for articles without domestic political involvement and 0.78 for articles with at least one domestic political figure implicated in the scandal. The lower F1 score for the latter category reflects the substantially smaller number of positive cases in the validation sample. Figure B1 reports the complete confusion matrices for both classification tasks.

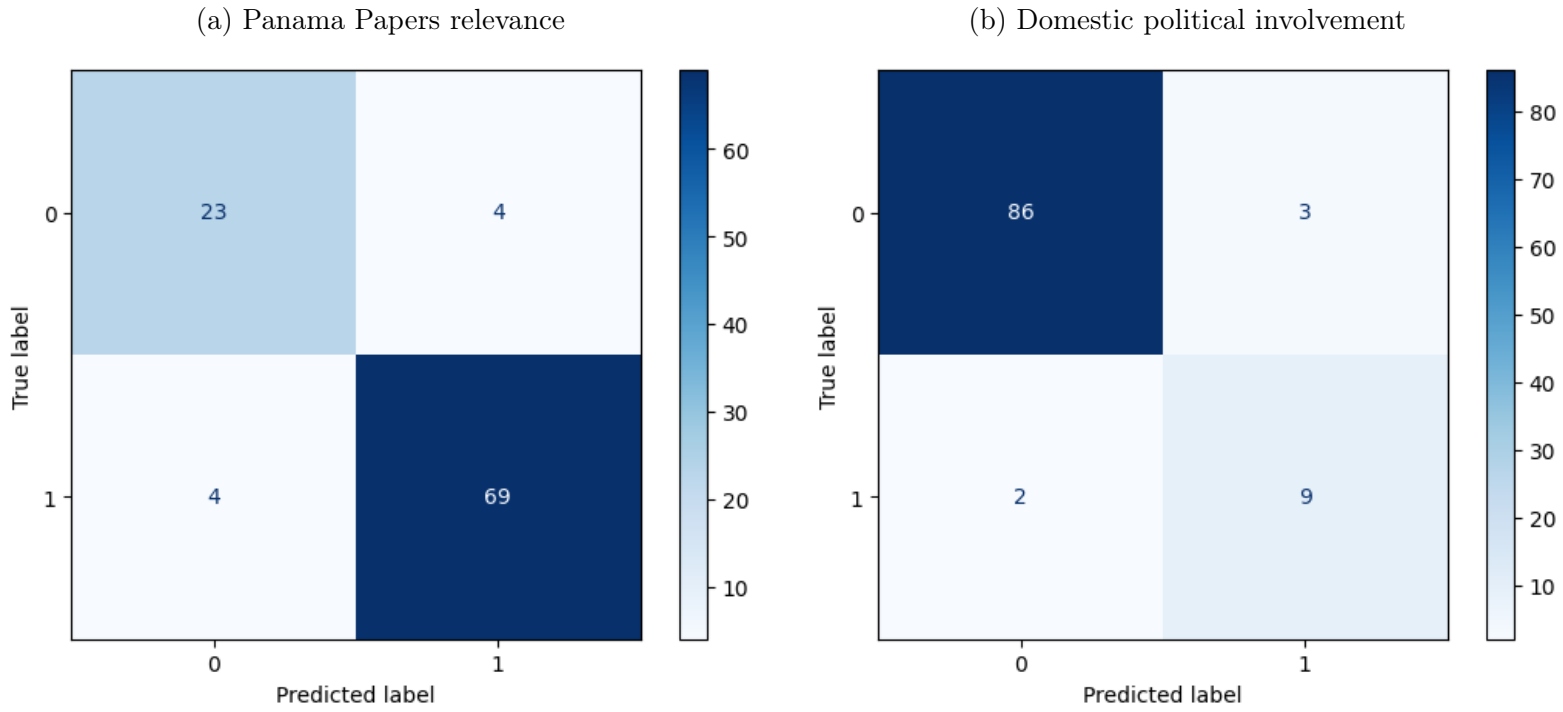
Table B1: First qualifying domestic revelation in LiTS III countries

Country	Date	Article ID	People involved	Role
Azerbaijan	04 April 2016	TURNEW0020160404ec440003a	Ilham Aliyev	President and his family
Georgia	04 April 2016	PRIMNE0020160405ec4400006	Bidzina Ivanishvili	Former PM
Kazakhstan	04 April 2016	KZPOL00020160405ec440006c	Nurali Aliyev	Grandson of Pres
Russia	04 April 2016	TASSRU0020160404ec44002k2	Dmitry Ulyukayev	Son of Economic Development Minister
Ukraine	04 April 2016	UKRARU0020160404ec440012x	Pavlo Lazarenko, Petro Poroshenko	Former PM, Former President
Latvia	07 April 2016	BNNEN00020160407ec470000b	Aivars Lembergs	Mayor and former oligarch
Slovenia	08 April 2016	STA0000020160408ec480005l	Two Slovenian honorary consuls	
Armenia	18 April 2016	ARMFNR0020160418ec4i0002y	Migran Poghosyan	Head - Compulsory Enforcement Service
Romania	8 May 2016	ROMPES0020160508ec58000p1	Corneliu Iacobov	Social-democrat (PSD) politician
Belarus	10 May 2016	BELAEN0020160510ec5a0008d	Uladzimir Shyshko	Officer of the Investigative Committee
Kyrgyzstan	10 May 2016	AKIBUS0020160511ec5a002gx	Saikal E. Akaeva	Daughter in law of former president
Slovakia	27 May 2016	SITA000020160627ec5r0007k	Tomas Borec	Former Justice Minister
Czechia	None	None	None	None

Table B2: Article retrieval and classification in the Eurobarometer sample

Country	News Agency	CODE	Language	Retrieved articles	Panama Papers	Panama Papers + domestic political figure
Austria	Austria Presse Agentur-OTS	AUPAG	German	111	84	5
Belgium	Agence Belga	BELGAF	French	102	77	11
Bulgaria	Bulgarian News Agency	BTA	English	22	19	3
Croatia	HINA	HINA	English	3	3	0
Czechia	CIA - Daily News	CZPRDI	English	9	9	0
	CTK Daily News	CTKDAN	English	22	19	4
Denmark	Ritzau General News Service	RITZG	Danish	423	362	0
Estonia	Baltic Daily - Political News	BALPOLE	English	18	15	0
	Прибалтийские новости	BFB	Russian	8	8	0
Finland	Suomen Tietotoimisto STT	STT	Finnish	178	160	4
France	Agence France Presse	AFPFR	French	862	694	51
Germany	WELT online	WELTON	German	133	97	3
	bild.de	BILDE	German	64	41	3
	Die Welt	DWELT	German	73	56	0
Greece	Athens News Agency	ANA	English	9	7	3
Ireland	The Irish Times	IRTI	English	109	79	4
	RTE.ie	RTEI	English	32	25	1
Italy	Agenzia Giornalistica Italia	AGI	Italian	203	167	6
Latvia	Латвийское Агентство Новостей (LETA)	LETAR	Russian	9	6	2
	Latvian News Agency	LETA	English	9	9	7
	Baltic News Network (BNN)	BNNRU	Russian	29	29	9
Lithuania	Литовское Агентство Новостей (ELTA)	ELTAR	Russian	7	7	1
	Lithuanian News Agency - ELTA	ELTA	English	17	17	3
Poland	Rzeczpospolita	RZEPOL	Polish	53	43	6
	Gazeta Wyborcza	GAZWYB	Polish	5	3	1
Portugal	Agencia Lusa	LUSA	Portuguese	720	258	14
Romania	AGERPRES	ROMPES	English	8	8	1
Slovakia	SITA Slovenska Tlacova Agentura	SITASL	Slovak	47	44	1
Slovenia	STA	STA	English	52	51	2
Spain	Agencia EFE - Servicio Nacional	AGESER	Spanish	477	372	258
Sweden	Dagens Nyheter	DGNS	Swedish	119	100	4
Turkey	Cihan News Agency (CNA)	CINEWA	English	48	32	0
United Kingdom	Press Association National Newswire	PRESSA	English	146	116	67
Total				4,127	3,017	474

Figure B1: Validation of the LLM-assisted article classification



Notes. The figure reports confusion matrices comparing the LLM-assisted classification with independent manual coding for a validation sample of 100 articles from the Eurobarometer corpus. Rows indicate the manually assigned ("true") classification and columns the classification produced by the LLM-assisted procedure. In Panel (a), label 1 denotes an article classified as concerning the Panama Papers; in Panel (b), label 1 denotes an article classified as implicating at least one domestic political figure. Label 0 denotes the corresponding negative category.