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Banklash: How Media Coverage of Bank Scandals Moves Mass Preferences on Financial Regulation

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Abstract: *Financial regulation is often adopted in the wake of scandals and crises. Yet political science has little to say about the political effects of corporate scandals. We break that silence, asking whether exposure to news coverage of bank scandals changes the preferences of voters for financial regulation. Drawing from the literatures on media influence and public opinion, we argue that news coverage of bank scandals should increase voters' appetite for regulation. We test our hypothesis with data from six countries, using original nationally representative panel surveys with embedded experiments (total* N *= 27,673). Our pooled and country-specific analyses largely support our expectation that exposure to news coverage of scandals increases regulatory preferences. We reproduce this finding in a separate survey wave, using different scandals than in our original analysis. These results contribute to studies on media influence on public opinion, the political significance of scandals, and the political economy of regulation.*

Verification Materials: The data and materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/GTSYTZ.

It is scandalous how little political science has to say about the political impact of corporate scandals. Scandals involving politicians can topple a government. They are thus fertile terrain for studying scandal effects on voting intentions (Arias et al. 2018; Dziuda and Howell 2021; Entman 2012; Nyhan 2014). However, this tight focus on what Hacker and Pierson (2010) call "politics as electoral spectacle" limits the purview of politics to office seeking, to the neglect of policy seeking. Corporate scandals can disrupt markets and inflict mayhem on the lives of ordinary people, making them consequential to the outcome of policy battles. Such scandals can upend the traditionally dominant position of business interests in securing their preferred outcomes on economic regulation (Culpepper 2011; Hertel-Fernandez, Mildenberger, and Stokes 2019).

Indeed, previous work characterizes it as an iron law that financial regulation in the United States is enacted quickly and with insufficient information in the wake of scandals and crises (Romano 2005; 2014, 56–57). Such scandals also manifest the power to overturn previously steadfast opposition to regulation. The

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corporate accounting scandals at Enron and WorldCom led to the quick passage of the Sarbanes–Oxley law on corporate governance in 2002, a law the Republican majority in the House of Representatives had opposed only weeks earlier (Jones and Baumgartner 2005, 72–76). The Dodd–Frank financial reform law passed in 2010 only after a media firestorm revealed conflicts of interest during a congressional hearing with Goldman Sachs, leading Republican opponents of the law to drop their filibuster of the act (Culpepper and Lee 2022). This dynamic of scandal-driven regulatory expansion is not restricted to the United States or to finance. In the European Union, the emissions scandal surrounding German carmaker Volkswagen (i.e., "Dieselgate") helped lead to the adoption of the 2018 New Deal for Consumers, which strengthened consumer protection laws across the EU (Tommasi 2020). In all these cases, powerful business interests wound up on the losing end of a policy battle in which lurid stories of scandal in the media played a major role in catalyzing popular outrage and galvanizing political action (cf. Jones and Baumgartner 2005).

While corporate scandals drive policy reforms in the real world, political scientists have yet to systematically study how democratic publics respond to mediatized scandals such as these. That is the challenge we take up in this article: Do mass publics demand more stringent regulation in response to media coverage of corporate scandals? Our regulatory domain of interest is the banking sector. Banks and financial services are integral to modern economies, yet their influence is widely regarded as hidden and their interests as protected in the policymaking process (Culpepper and Reinke 2014; James 2018). The financial sector is thus an ideal place to examine the effects of corporate malfeasance brought into the limelight. Prior work on media effects and public opinion leads us to expect that media coverage of bank scandals will shift voters to favor greater financial regulation.

The capacity for mediatized scandals to rouse voters matters in politics because policy entrepreneurs can successfully exploit public cries for greater regulation to advance their own proposals for more stringent regulation (Romano 2005; Ziegler and Woolley 2016). Since the financial crisis of 2008, banks and bankers have faced popular backlash across the globe, sentiments that have further fueled populist anger against a perceived elite capture of democratic politics (Gava, Sabaté, and Morales 2022; Massoc 2020). This phenomenon, which we refer to as "banklash," has spawned new interest by political scientists in the determinants of public opinion on financial regulation (Chaudoin and Wilf 2019; Culpepper and Lee 2022, 2021). Some scholars argue that the increased financialization of the economy leads voters to view their interests as aligned with banks, and therefore to support policies that favor finance (Chwieroth and Walter 2019; Pagliari, Phillips, and Young 2020; Young and Yagci 2019). If media coverage shapes regulatory preferences in the financial sector, scandals can potentially countervail both the "quiet politics" lobbying advantages as well as the "financialization of policy preferences" advantages that typically favor big banks (Culpepper 2011; Pagliari, Phillips, and Young 2020).

Our research design to study the effects of scandal coverage is novel in several key respects. First, we apply a conceptually similar scandal framework across different countries, through which we seek to understand the general effect of scandal coverage. Second, we test the generalizability of our findings within countries using multiple scandals. We are aware of no study that has attempted operationalization of a similar conceptual treatment across countries and within countries in this way. We examine six countries that vary in their political and media institutions (Hallin and Mancini 2004) and in the role of banks and the heft of financial services in their overall economies (Hardie et al. 2013): Australia, France, Germany, Switzerland, the United Kingdom, and the United States. What they have in common is that they matter for the politics of global finance. They are home to more than half the world's globally systemically important banks, and each country has experienced at least two prominent financial scandals since 2008.

To draw internally valid inferences about media effects, we fielded online panel surveys of nationally representative samples from the six countries, with embedded experiments conducted over three waves during 2020. To draw externally valid inferences about media effects, our experimental treatments exposed panel respondents to journalistic coverage using language that appears in articles about real scandals that took place in the financial sector. The multi-country, multi-frame scope of our findings, the large panel structure of the data, and the rigorous adherence to testing the effect of the news as it is actually written set our empirical approach apart. We think these lessons about scandals also extend beyond the important context of financial regulation.

After a wave 1 study that measured the baseline sociodemographic, political, and attitudinal background of our respondents, we tested the effects of different types of scandal frames on attitudes toward financial regulation in wave 2. Our pooled analysis demonstrates that, as per our preregistered expectation, all scandal-related treatments increased regulatory preferences. In country-specific analyses, we have varying results, yet our broad finding is that scandals move public opinion. In wave 3 of

our panel survey, we aim to replicate our findings using different scandal treatments than in wave 2. The replication is successful in the pooled analysis, and we find results that are the same as those in wave 2 in four of the six countries.

This article contributes to several areas of political inquiry. To the literature on media coverage of scandals (Entman 2012; Nyhan, 2014, 2017; Puglisi and Snyder 2011), we make a compelling case for why corporate scandals merit interest alongside political scandals. To the literature on media influence on public opinion (Barnes and Hicks 2018; Dilliplane 2014; Ladd and Lenz 2009; Leeper and Slothuus 2020), we add findings on an issue area characterized by relatively low levels of salience and elite-level polarization. Finally, we contribute to the political economy literature on postcrisis financial regulation, which has gestured to the importance of public opinion (Kastner 2018), but which has concentrated on the inability of business to get its way rather than whether and how activated mass publics may contribute to that outcome (James 2018; Massoc 2020; Ziegler and Woolley 2016). We elaborate on these points and draw implications for future research in the concluding section.

Corporate Scandals and Policy Preferences

Scandals are stories about transgressions of norms or rules that are brought into the public eye. Scandals admit a range of transgressions, with some stories concerning violations of social norms and others concerning corruptions of law or other formalized codes of conduct. Importantly, scandals require publicity as a precondition because what makes the story scandalous is that what was once private and known to few is made public and widely discussed. In modern life, scandals are a mediatized social construction (Adut 2008; Nyhan 2014; Thompson 2000).[1]

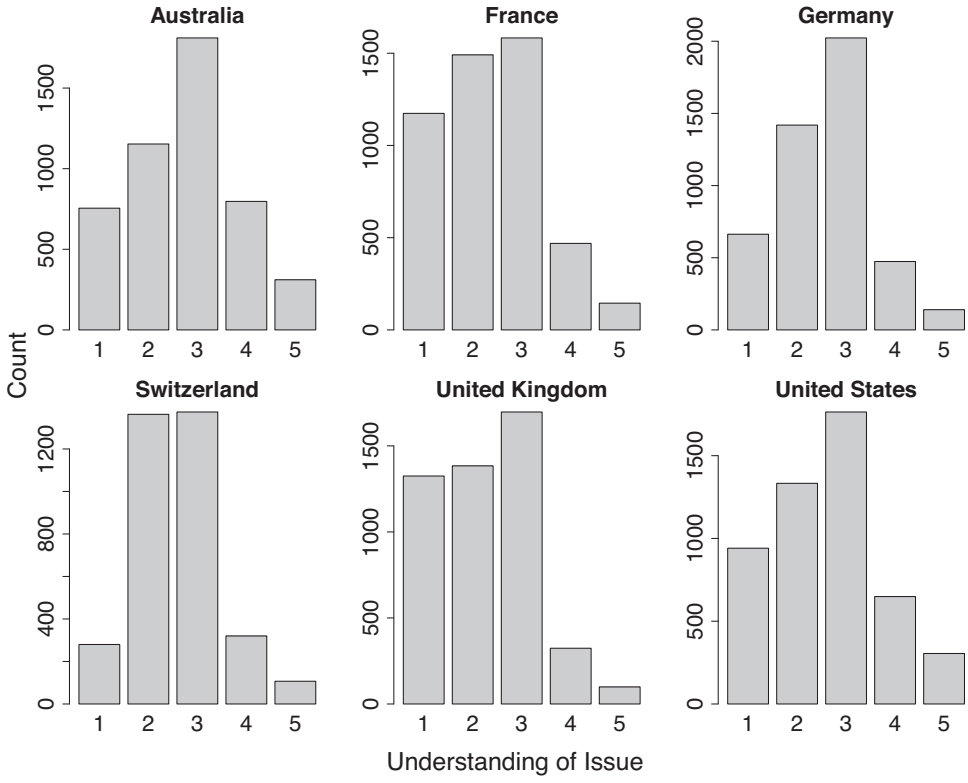
Political science research on scandals tends to focus exclusively on politicians and political parties. Quantitative analysis of scandals has largely concentrated on their electoral and institutional consequences (Agerberg 2020; Bowler and Karp 2004). Formal analysis has highlighted the strategic considerations facing political actors involved in actual or potential scandals, including the choice to publicize potential malfeasance as a scandal (Basinger and Rottinghaus 2012; Dewan and Myatt 2007; Dziuda and Howell 2021). Some recent work has focused explicitly on the involvement of the media (Entman 2012; Nyhan 2014; 2017), though typically without attention to the effects of media coverage of scandals on public attitudes.

Our study draws the attention of political scientists to the study of corporate scandals, and in particular the relationship between media coverage of these scandals and voters' preferences for regulation. We posit that media coverage of corporate scandals can have an identifiable and independent effect in moving voters' preferences in favor of greater regulation. Although the claim may seem intuitive, establishing that media coverage per se affects attitudes is far from self-evident. In highly polarized societies like the United States, voters are prone to choose their media outlets according to their partisan leanings and discount messages that are inconsistent with their partisan priors (Arceneaux, Johnson, and Murphy 2012; Bennett and Iyengar 2008). Even when countervailing media frames break through the echo chambers and filter bubbles voters inhabit, motivated reasoning generally prevails (Druckman, Peterson, and Slothuus 2013).

We conjecture nonetheless that media coverage of corporate scandals may influence public opinion because economic regulation is a domain on which voters tend to have less fixed views and prior information. A recent study finds that media coverage of scandals is more likely to generate negative evaluations when the scandalized individuals are less well known to voters (e.g., local businesspeople rather than state governors) or when the issues involved are relatively unfamiliar to voters (Green, Zelizer, and Kirby 2018). Corporate regulation, whose details can be technical, constitutes what Carmines and Stimson (1980) call a hard issue. Hard issues ask voters to think about unfamiliar and cognitively challenging aspects of an issue to reckon their preferences. Consider finance, the domain of corporate regulation we consider in this article. Figure 1 shows that in all six countries we survey, at least 40% of respondents said that they understood "the important issues about finance and banking facing our country" either "Not well at all" or "Slightly well." It is in such hard issue areas that we expect exposure to media coverage to be more likely to be received as a relevant consideration and to thus shape policy preferences (Zaller 1992).

[1]The literature on scandals is related to the much vaster literature on corruption. The corruption literature deals primarily with the financial malfeasance of politicians or parties (Arias et al. 2018; Ferraz and Finan 2008; Larreguy, Marshall, and Snyder 2020). Corruption may be recurring or be about a single action. Scandals, by contrast, are almost always discrete events. Indeed, significant scandals are often known by shorthand; Teapot Dome, Watergate, and Iran–Contra are all sufficient descriptors of mediatized scandals in the American context. Where media coverage of corruption goes viral, there you will typically find what we define as a scandal.

FIGURE 1 Self-Reported Understanding of Important Issues of Finance and Banking

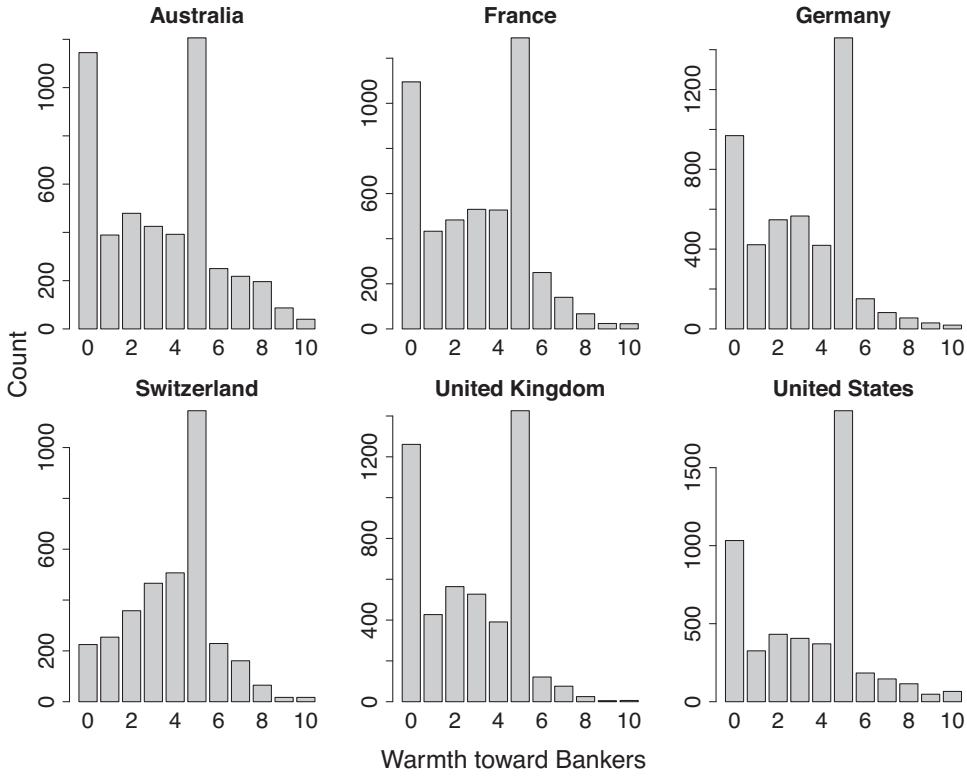


Notes: Values of how well respondents understand issues of finance and banking on the horizontal axes range from 1 ("Not well at all") to 5 ("Extremely well"). The vertical axes refer to the number of respondents in each country. Data are from wave 1.

At the same time, media coverage of corporate scandals has the potential to create negative attitudes by highlighting perceived differences between "the people" and "the elites." This is evident from the reactions among large segments of the public that followed the bank bailouts of the 2008 financial crisis. Moreover, it is echoed in historical research. Massoc (2019) and Münnich (2017), for example, demonstrate that anti-finance attitudes among the public can easily be mobilized when there are antagonistic anti-finance narratives in the public domain. Our data confirm the existence of a deep well of negative affect toward finance. Figure 2 measures warmth toward bankers. In every country we study, more people feel cold than warm toward bankers (see Supporting Information [SI] Appendix B, pp. 1–2).

In positing an effect of scandal coverage on mass preferences over economic regulation, we join others who identify the media's influence on voters (Barnes and Hicks 2018; Gabel and Scheve 2007; Ladd and Lenz 2009). In the economic domain, Barnes and Hicks use the British Election Study and a survey experiment to show that news influences attitudes toward fiscal policy. Chaudoin and Wilf (2019) also use survey experiments to show that Americans' support for financial regulation is responsive to media stories that emphasize the consequences of regulation for financial stability of the American economy or network stability of the global economy. Ferraz and Finan (2008) use corruption audits in Brazil, where municipalities were randomly selected for such audits, to demonstrate that media coverage acts as an accountability mechanism for incumbents. Larreguy, Marshall, and Snyder (2020) exploit variation in the public release of audit studies in Mexico using a difference-in-difference design to study the media's role in democratic accountability. These studies give us additional reason to expect that media coverage of corporate scandals influences voters' attitudes toward corporate regulation.

FIGURE 2 Warmth toward Bankers



Notes: Values of how warm people feel toward bankers on the horizontal axes range from 0 ("Extremely cold") to 10 ("Extremely warm"). The vertical axes refer to the number of respondents in each country. Data are from wave 1.

Bank Scandals and Financial Regulation

In this article, we focus on one particular type of corporation: big banks. Since the financial crisis of 2008, big banks have come to epitomize inequality and the yawning gap between Main Street and Wall Street in the popular imagination. This is not just an American phenomenon—bank bailouts around the globe have fueled perceptions that finance benefited from sweetheart deals at the expense of the general population (Culpepper and Reinke 2014; Grossman and Woll 2014). The firestorm against finance since 2008 has created momentum for reforms of financial regulations in the United States, the United Kingdom, and the countries of the European Union (James 2018; Massoc 2020; Ziegler and Woolley 2016).

Notwithstanding the media's extensive coverage of bank scandals since 2008, we lack a good understanding of how media coverage of bank scandals affects voters' attitudes on financial regulation. Given the importance of banks and bankers in populist reactions against elite domination of politics (Münnich 2017), the financial sector is an apt place to study the question of what effect media coverage has on voters' policy preferences. Much of the scholarship on postcrisis reforms has focused on interest group politics as the domain in which political conflict has been adjudicated. Yet, as discussed previously, media coverage is likely to shape voters' attitudes on issue areas such as financial regulation, which are hard to properly understand but at the same time can be cast in valenced, populist terms. Interest groups often claim public opinion is behind them in pushing their "off-the-rack" solutions (Romano 2014). Additionally, political scientists have underlined that nothing changes the outcome of interest group battles like a significant shift in public opinion (Hacker and Pierson 2002; Jones and Baumgartner 2005; Trumbull 2012).

Furthermore, financial regulation is an area that is less likely than others to be subject to partisan motivated reasoning, by virtue of its greater issue complexity and lack of partisan issue ownership. Many of the key areas of financial regulation since 2008—systemic risk, regulatory capture, and executive accountability—are not

TABLE 1 Country Cases

	Australia	France	Germany	Switzerland	United Kingdom	United States
G-SIBs	0	4	1	2	3	8
Finance as percent of economy	8.9	3.7	3.9	10.0	6.7	7.9

Source: G-SIBs from Financial Stability Board (2020). Finance as a percent of economy from OECD Value Added by Activity, 2020 (data for Australia and the United States from 2019).

issues on which parties of center-left and center-right, or the media outlets associated with them, generally have detailed positions. Where mainstream parties have made their positions known, they differ very little: Don't let banks be too big to fail and make executives individually responsible for malfeasance. Moreover, because financial regulation comprises issues of market functioning (associated with the right) as well as consumer protection (associated with the left), it is not clear which parties can claim ownership over it. In other words, modern financial regulation often appears as a valence issue in which the goal of different parties is the same, and the question is which party is best able to achieve the goal.

The core theoretical expectation that we test in the article is that media coverage of bank scandals leads people to express attitudes that are more supportive of regulating big banks. In terms of mechanisms, we expect both priming and learning to be the processes through which voters express pro-regulatory attitudes after being exposed to media coverage of bank scandals (Chong and Druckman 2007; Lenz 2009; Matthews 2019). Priming implies that scandal coverage makes considerations related to banking misconduct salient in people's minds, thereby shifting their views in a pro-regulatory direction. Learning suggests that scandal coverage provides information people did not know previously, thereby causing them to update their regulatory preferences. In either case, we expect to see a "banklash" effect of media coverage of scandals that drives greater preferences for financial regulation.

Research Design

To generalize about the effects of scandals on attitudes, we test our argument in multiple countries that vary along important political and economic dimensions: Australia, France, Germany, Switzerland, the United Kingdom, and the United States. Scandals are mediatized constructions, so we test across countries with different types of media systems (Hallin and Mancini 2004). Also, banks in these countries play different roles in the economy (Hardie et al. 2013), all while being politically important. As shown in Table 1, five of these democracies are home to 18 of the 30 institutions classified as globally systemically important banks (G-SIBs) by the Financial Stability Board; Australia lacks a G-SIB but possesses a concentrated banking sector that accounts for a large share of the domestic economy. We expect big, economically significant banks to be politically influential, so by examining these countries we are seeing whether voters' preferences for regulation increase in the wake of scandalous media coverage even in polities with highly influential banks. The countries vary in terms of the overall role of financial services in the national economy, ranging from 10% in Switzerland to less than 4% in France and Germany.

Our empirical strategy was to collect survey data over three waves of an online panel survey with embedded experiments in the six countries. Our data were collected by the firm YouGov in Australia (AU), France (FR), Germany (DE), the United Kingdom, and the United States. In Switzerland (CH), LINK collected our data, as YouGov did not have an adequately representative online panel there.[2] We chose a panel structure because the data collection on which this article is based is part of a larger project that examines other research questions that exploit the panel structure of our data, such as the longevity of treatment effects. Our panel structure also enables a comprehensive baseline survey of sociodemographic, political, and attitudinal variables that are cleanly pretreatment.

The first wave of our survey collected data on respondents' sociodemographic, political, and attitudinal characteristics and was fielded between January and March 2020. The total sample sizes are 4,827 (AU), 4,862 (FR), 4,719 (DE), 3,444 (CH), 4,829 (UK), and 4,992

[2] For France, Germany, and Switzerland, we wrote the survey questions in English and had the survey companies translate them to French and German. We then had native language research assistants verify the translations.

(US).[3] Wave 2 was fielded between February and May 2020 with a sampling frame of all respondents who participated in wave 1. Panel retention rates range from 57% in the United States to 73% in Switzerland, with wave 2 sample sizes of 2,958 (AU), 3,026 (FR), 2,898 (DE), 2,505 (CH), 3,217 (UK), and 2,870 (US). The second wave was the first experimental element of the panel survey.[4] We randomized eight experimental arms, with each arm showing a mock news article in vignette form.[5] Respondents were asked to read the news article and then answer a series of questions, one battery of which examined attitudes toward financial regulation. The final, third wave of the panel further probed the results from wave 2 with follow-up experiments, which we discuss after presenting the results from wave 2.

The experimental vignettes are based on actual news articles in the major news outlets in each country. Five of the eight vignettes in wave 2 are about bank scandals (our treatments of interest), one is a control vignette, one is a positive article about banks and chief executives, and one is a neutral article reporting banks' earnings and strategies. Our primary interest is not in the effect of the positive article, but we discuss it briefly to put the effects of the scandal treatments in perspective. We omit discussion of the neutral vignette, as it is irrelevant for this article.

Vignettes

Our objective was to understand whether media coverage of scandals affects preferences across countries. To that end, we first created conceptually similar treatments that were adapted to existing scandals of actual financial institutions in all six countries. Beyond gaining greater external validity over more fictionalized scandals, this design choice also bakes in greater variation in corporate scandals as a treatment variable. Not all the real scandals we use are equally shocking in the alleged transgressions, and not all were equally salient in the news at the time they unfolded. We see this greater variation as an a fortiori test of our expectations and a design that will produce more robust inferences.

We refer to the experimental arms in wave 2 as *nonbank, scandal, left partisan, right partisan, capture, victim*, and *positive*. Our control arm (*nonbank*) is an article about the generic business of a nonbank company. We are interested in the effect of a banking scandal, not in the effects of general economic news on people's attitudes. Accordingly, our control article presents nonscandalous economic news about a large nonbank company that has no obvious political salience. These are companies with which most respondents are familiar, just as in the case of the banks we examine. The control companies are Coles for Australia, Carrefour for France, Rossmann for Germany, Coop for Switzerland, Next for the United Kingdom, and Office Depot for the United States.

The five treatment arms are news articles that report a bank scandal in different ways. Our research design is in line with Blumenau and Lauderdale's (2022) argument that using multiple implementations of a treatment is important for experimental research that is interested in the influence of a general type of treatment. As part of a broader project on media coverage of banking, we have collected a comprehensive set of newspaper articles from Australia, the United Kingdom, and the United States from 2007 to 2018. Figure 3 displays combined data from a computational analysis performed on a corpus of newspaper articles from the three English-speaking countries. We identified articles relevant to banks and used a supervised classifier to identify articles featuring scandals. Figure 3 demonstrates that scandal articles are a regular and possibly growing feature of postcrisis articles about banking in these countries.

Some stories report scandal without reference to politics, focusing simply on the details of the scandal itself. Others provide information about the scandal within a political context, in which politicians state their views about the behavior of companies and possible policy remedies. Yet others portray scandals as the consequence of regulatory capture and the cozy relations between banks, mainstream parties, and government agencies (Culpepper and Lee 2021). The vignettes that we use reflect these diverse depictions of bank scandals that exist in actual news coverage of the six countries we study. All scandals in each country are about the same bank. The banks are Westpac (AU), Société Générale (FR), Deutsche Bank (DE), UBS (CH), Lloyds (UK), and Wells Fargo (US).

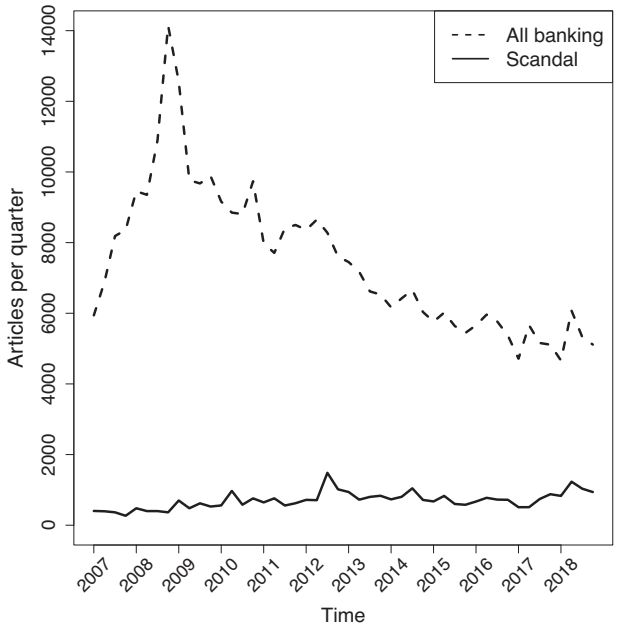
Our baseline bank scandal is the *scandal* vignette. This treatment does not have explicit political references. It focuses on the details of the scandal, such as the predatory practices of Wells Fargo toward its customers.

[3] See SI Appendix A (p. 1).

[4] We registered a preanalysis plan on February 16, 2020. Because of space constraints, we present a shortened version of the preregistration in SI Appendix L (pp. 21–24).

[5] Before data collection, we conducted power analysis using the treatment effect size (0.4) and standard deviation (1.18) of financial regulatory attitudes from our previous unpublished work. Based on those benchmarks, we needed a sample size of at least 276 to achieve 80% power at the .05 significance level. The smallest sample size we have for an experimental arm in wave 2 is 313. From that perspective, wave 2 results are well powered.

FIGURE 3 Banking and Scandal Articles in Australia, the United Kingdom, and the United States



Notes: The dotted line indicates the number of newspaper articles related to banks from 2007 to 2018 in a selection of major newspapers from Australia, the United Kingdom, and the United States. The solid line indicates the number of newspaper articles that deal with banking scandals, which we identified using a supervised classifier.

The other four scandal-related treatment articles in each country—*left partisan*, *right partisan*, *capture*, and *victim*—build on the framework of the basic *scandal* vignette.

The *left partisan* and *right partisan* arms build on the *scandal* article by adding statements and positions from major political parties regarding the scandal. The *left partisan* arm includes discussions by a party of the left in each country (e.g., SPD in Germany), and the *right partisan* arm has discussions from a party of the right (e.g., Liberals in Australia).[6] In all countries except Switzerland, both articles end with a virtually identical policy position. In Switzerland, because the most important left and right parties had different views on the policy question at issue in the scandal, the positions in the two articles differ.

The *capture* arm discusses the bank scandal by referring to how banks have captured regulators and government policymaking, such as through the revolving door between senior jobs in government and in banks. This article adds a political dimension to the scandal not by mentioning political parties, but by discussing structural issues behind bank scandals. This treatment allows us to examine whether the structural entanglement between politics and business shapes the influence of the scandal on people's attitudes (Culpepper and Lee 2021).

The *victim* arm is an article that discusses the bank scandal by focusing on the experience of a particular victim. This type of narrative is found in existing media, and it is a version of episodic frames—portrayals of issues in personalized, eventful, dramatized terms—discussed in the framing literature (Iyengar 1991).

Finally, our *positive* treatment assesses whether positive information about a bank would have opposing effects on regulatory preferences, influencing voters in the direction of *less* stringent banking regulation. In each country, the *positive* arm is an article that discusses in upbeat language the competence of the bank's CEO and the positive direction in which the bank is moving, with minimal reference to a scandal. The article then concludes with a policy cue favorable to the position of banks on regulation or taxation.

We pretested all the articles before data collection. We used manipulation checks to ensure they are read in the intended ways (see SI Appendix D, p. 3). We also checked for balance between the control group and the treatment groups on key sociodemographic variables from wave 1 (see SI Appendix E, p. 3).

Outcome

After being randomly assigned to read one of the articles, wave 2 panelists were asked about their attitudes toward financial regulation. Respondents were asked their views on a battery of six statements related to financial regulation on a Likert scale ranging from strong agreement to strong disagreement (see Table 2). We derived these statements from our review of scholarship and journalistic coverage of the major issues that animated public debates around financial regulation across the six countries: systemic risk, the legal culpability of executives, the incentives created by a bonus compensation structure, consumer protection, tax evasion, regulatory capture, and the risks of overregulation (cf. Emmenegger 2017; Ganderson 2020; Massoc 2018, 77–101). Financial regulation involves sometimes complicated themes, but we used language that spoke to these issues in ways that people might talk about them, not in technocratic

[6]SI Appendix C (p. 2) includes the names of the parties we use in each country.

TABLE 2 Financial Regulation Items

Item 1	Bank executives who take excessive risks that jeopardize jobs and the economy should face jail time.
Item 2	Government should NOT regulate the size of bonuses that banks pay their executives even if those bonuses create incentives for bankers to take greater risks with customers' money.
Item 3	Government should NOT regulate banks too heavily, because that will reduce the availability of credit.
Item 4	Government should crack down on big banks to ensure that they cannot exploit loopholes to avoid paying their fair share of taxes.
Item 5	It is unacceptable for former government employees to work as executives for banks they used to regulate.
Item 6	Government or regulators should NOT have greater power to punish banks that take advantage of consumers.

Notes: This table shows the version we used for the United States.

language.[7] The wording for our financial regulation battery is shown in Table 2.

To reduce straightlining among respondents, three of the statements were worded negatively using the word "NOT" (Paulhus 1991). For analyses, we reverse-coded respondents' answers to those items so that higher values indicate preferences for more stringent regulation. For the outcome variable (*Regulation*), we create a rescaled factor score ranging from 1 to 5. This variable allows the six items to load to different degrees while keeping the composite values easily interpretable. Table 3 shows the factor loadings and Cronbach's α scores of the items in each country. Loadings range from 0.33 to 0.73 with a mean of 0.58. Cronbach's α ranges from 0.71 to 0.79, indicating the overall internal reliability of the scale.

[7]We selected the areas covered by the six statements and their wordings based on iterative pilots of different formulations using exploratory factor analysis that allows for correlation between factors. These pilots were conducted in the United States (using Amazon Mechanical Turk). We chose areas and question wordings that substantively cover the range of financial regulation and most coherently load onto one factor.

To illustrate the regulation scale we created, we present distributions of the scale from wave 1. Figure 4 shows the density distributions in each country, with the vertical dotted line indicating the weighted mean. The modal opinion across the six items in France, Germany, and the United States sits right in the middle of the scale, consistent with our earlier discussion that financial regulation is largely a valence issue.

Analysis and Results

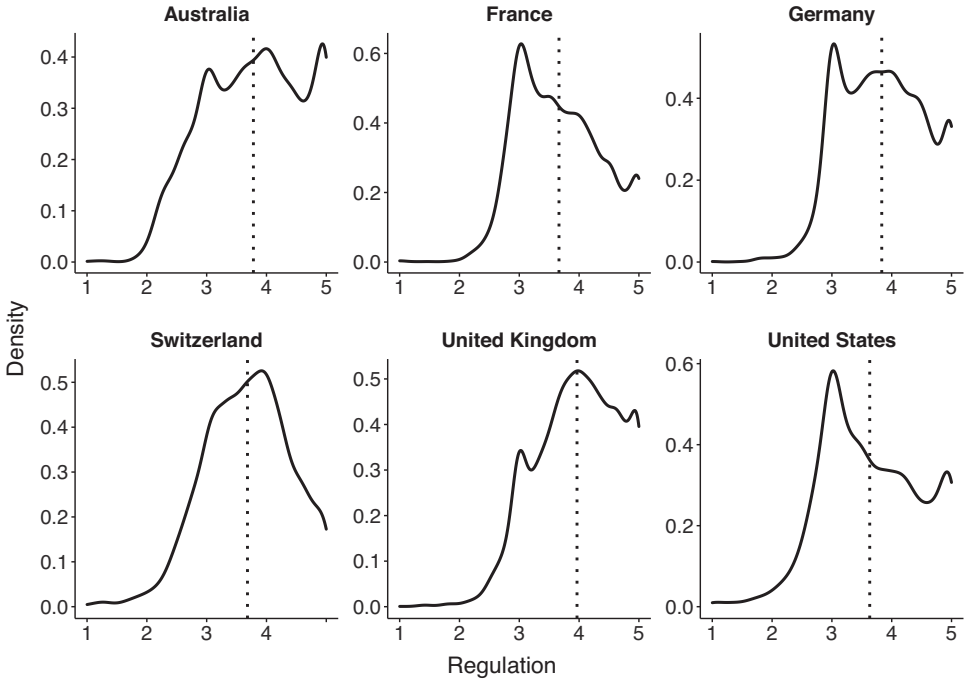
To analyze the effects of the treatments on regulatory preferences, we present first the results pooled across all six countries (Figure 5). They are based on weighted ordinary least squares (OLS) with country fixed effects, using post-stratification weights from YouGov and LINK. Weighted analyses are appropriate because we are interested in understanding generalizable effects of scandal-related treatments; however, our samples are not pure random samples, and it is unlikely that treatment effects are constant across all subgroups (Franco et al. 2017;

TABLE 3 Factor Loadings and Cronbach's α of Financial Regulation Items

	Australia	France	Germany	Switzerland	United Kingdom	United Kingdom
Item 1	0.38	0.54	0.60	0.50	0.51	0.55
Item 2	0.67	0.60	0.53	0.63	0.64	0.72
Item 3	0.73	0.59	0.58	0.67	0.63	0.72
Item 4	0.48	0.57	0.68	0.63	0.57	0.66
Item 5	0.33	0.46	0.46	0.45	0.45	0.37
Item 6	0.72	0.45	0.68	0.64	0.65	0.68
Cronbach's α	0.73	0.71	0.76	0.76	0.75	0.79

Notes: The first six rows show standardized factor loadings using minimum residual solution. The last row shows standardized Cronbach's α scores.

FIGURE 4 Distributions of the Regulation Scale across Countries



Notes: Distribution of regulation factor scores from wave 1 for each country is shown. Dotted lines show weighted means. Sample sizes: 4,827 (Australia), 4,862 (France), 4,719 (Germany), 3,444 (Switzerland), 4,829 (United Kingdom), and 4,992 (United States).

Miratrix et al. 2018). In Figure 5, the vertical axes indicate the treatment of interest, compared to the control, and the horizontal axes show the effect of the treatment on *Regulation* and its 95% confidence interval.

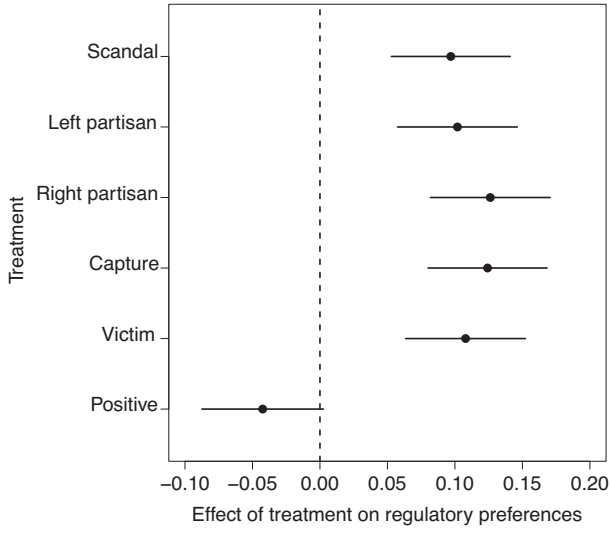
Figure 5 shows that all five scandal-related treatments have a significant and positive effect on attitudes toward financial regulation. The effect sizes of the different arms are similar: 0.10 (*scandal*), 0.10 (*left partisan*), 0.13 (*right partisan*), 0.12 (*capture*), 0.11 (*victim*). The effects are not significantly different from each other. These results are consistent with our preregistered expectation that media coverage of bank scandals increases regulatory preferences among the public. The substantive effect sizes range from 13% to 17% of the standard deviation of the outcome variable. By way of comparison with an important pretreatment covariate measured in wave 1—trust in banks—we see that this effect size is similar to that of a unit decrease in trust, which is associated with a 0.16 increase in regulatory preferences. In contrast, the *positive* treatment shown in Figure 5 does not have a significant effect on regulatory preferences.[8]

Do our effects also hold when we move from the pooled data to the level of the individual countries, given that our conceptually similar scandal treatments were in fact based on different national scandals? To answer that question, we examine the joint effect of all scandal-related treatments for each country separately (see Figure 6). We run weighted *t*-tests between the treatment group and the control group in each country. The figure shows differences in means and their 95% confidence intervals.

Compared to the pooled data, the individual countries present a less uniform picture. In Australia, France, Germany, and the United States, scandal treatments have a significant positive effect on regulation. In Switzerland and the United Kingdom, the effect is not statistically significant even though the direction is also positive. The null results in Switzerland and the United Kingdom are not significantly different from the positive effects found in the other four countries. To understand these results in further detail, we also examined the effect of each scandal-related treatment (*scandal*, *left partisan*, *right partisan*, *capture*, *victim*) separately in each country (see SI Figure F.1). Multiple scandal-related treatments

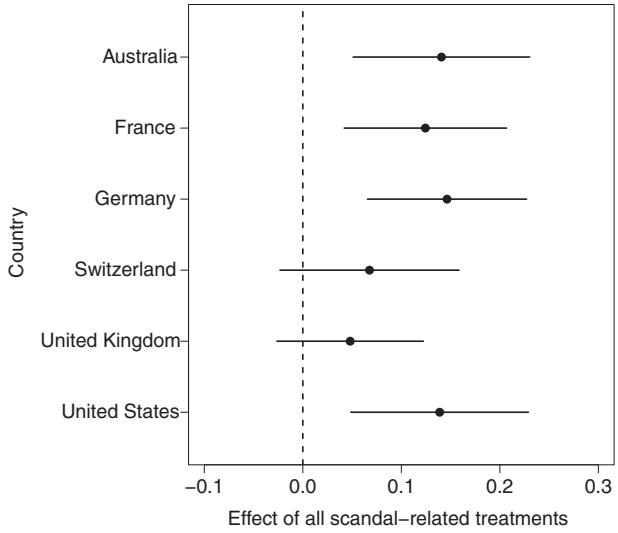
[8]We also investigated whether treatment effects occur due to priming and/or learning. Our data are not sufficient to discriminate cleanly between these two mechanisms, but the evidence we have suggests stronger support for learning (see SI Appendix G, pp. 12–15).

FIGURE 5 Treatments' Effects on Regulatory Preferences, Pooled



Notes: For the treatment types indicated on the vertical axis, the dots show the effects of treatment in OLS models with country fixed effects and post-stratification weights. Bars show 95% confidence intervals. Sample sizes for each model: 4,334 (scandal), 4,351 (left partisan), 4,332 (right partisan), 4,333 (capture), 4,352 (victim), and 4,267 (positive).

FIGURE 6 Treatments' Effects on Regulatory Preferences, by Country



Notes: For the countries indicated on the vertical axis, the dots show the weighted differences in means between the control group and the treatment group on the outcome variable, *Regulation*. Bars show 95% confidence intervals. Sample sizes for each country—AU: 366 (control), 1,862 (scandal-related treatments); FR: 382 (control), 1,916 (scandal-related treatments); DE: 355 (control), 1,835 (scandal-related treatments); CH: 313 (control), 1,567 (scandal-related treatments); UK: 368 (control), 2,061 (scandal-related treatments); and US: 342 (control), 1,831 (scandal-related treatments).

have positive effects in Australia, France, Germany, and the United States. *Scandal* comes close to having a significant effect in Switzerland, and *capture* has a significant positive effect in the United Kingdom. In short, our scandal treatments do not have uniform effects across the six countries, but we did not preregister any expectations for differences across cases, and these variations within countries cannot be distinguished from statistical noise. The overall evidence suggests that scandal articles move public opinion in a pro-regulatory direction across different countries.

Our findings are robust to running linear regressions after controlling for pretreatment covariate imbalance, using an alternative outcome variable that averages respondents' answers to the six items on financial regulation, running unweighted models, running models that include a fixed effect for the timing of data collection for data collected through YouGov, including pretreatment covariates as controls, taking into account the impact of potential pre-treatment effects (Druckman and Leeper 2012), and running pre–post analyses suggested by Clifford, Sheagley, and Piston (2021; see SI Appendix E, pp. 3–4; SI Appendix F, pp. 4–10).

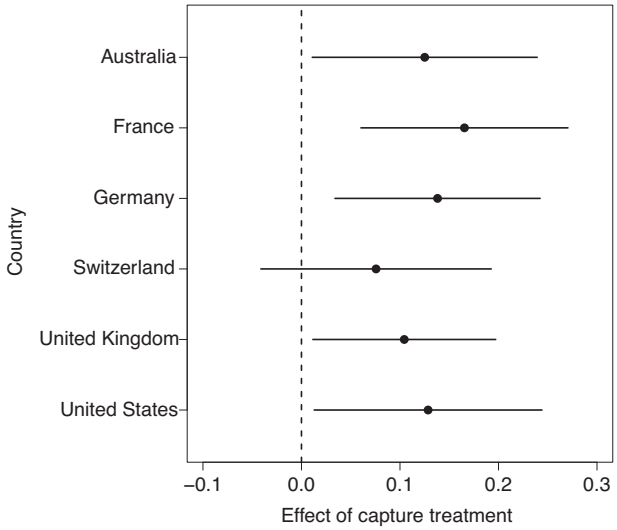
We used wave 3 to see whether our findings could be replicated using a different set of scandals than in the previous wave. Even though our wave 2 already subjects our hypothesis to a stringent test by using multiple countries and treatments, we further pushed to see whether our finding is specific to the content of the particular scandals we tested in wave 2. In wave 3, we used the treatment type that most consistently moves public opinion across the countries in wave 2 (see SI Figure F.1)—the one that frames bank scandals in terms of political capture. We summarize the effects of the capture treatments in each country in Figure 7.

Research Design: Follow-Up Study

Wave 3 of our panel survey features four treatment vignettes and one control vignette in Australia, France, Germany, the United Kingdom, and the United States. In Switzerland, because of the smaller sample size, we have three treatment vignettes and one control vignette. One of the treatment vignettes across all countries is a *scandal-capture* article, which replicated the conceptual treatment from wave 2 but did so using a different scandal.[9] Wave 3 was in the field between August and

[9]We do not discuss the other arms in wave 3 because they are unrelated to this article.

FIGURE 7 Capture Treatments' Effects on Regulatory Preferences, by Country



Notes: For the countries indicated on the vertical axis, the dots show the weighted differences in means between the control group and the capture group on the outcome variable, *Regulation*. Bars show 95% confidence intervals. Sample sizes for each country—AU: 366 (control), 375 (capture); FR: 382 (control), 382 (capture); DE: 355 (control), 352 (capture); CH: 313 (control), 313 (capture); UK: 368 (control), 409 (capture); and US: 342 (control), 376 (capture).

October 2020, and the sample sizes are 2,895 (AU), 3,216 (FR), 3,114 (DE), 2,417 (CH), 3,226 (UK), and 2,977 (US); this gives us a sample size of roughly 600 per experimental arm in each country.[10] To increase the sample sizes, we distributed the survey not only to those who answered both wave 1 and wave 2, but also to those who answered just wave 1. We preregistered wave 3 on August 14, 2020 (see SI Appendix M, pp. 25–26).

Vignettes

The control vignette in each country is largely the same as the control article we used in wave 2, except that the content was updated to reflect the periods in which wave 3 was fielded.

The *scandal-capture* treatment included in all six countries was a conceptual replication of the *capture* article used in wave 2. The *scandal-capture* article in wave 3 involved a different scandal and a different bank than in wave 2, but it addressed the same structurally captive relationship between banks and political elites in each country. The banks discussed in the *scandal-capture* articles are Commonwealth Bank (AU), BNP-Paribas (FR), Wirecard (DE), Raiffeisen (CH), RBS (UK), and Goldman Sachs (US).

As we did for wave 2, we pretested all articles before data collection. We used manipulation check questions to ensure the articles were read as intended (see SI Appendix H, p. 15). We also checked for balance between the control group and the treatment groups on key sociodemographic variables from wave 1 (see SI Appendix I, p. 15).

Outcomes

As in wave 2, we used a six-item battery on financial regulation preferences. Differently from wave 2, only one item included the word "NOT." We opted for this change in wording because upon doing factor analysis of earlier waves, we found that the six items do not load onto one factor very well because of the negatively worded items. Although we aimed to reduce straightlining, our negatively worded items seemed to make the items more difficult to understand (Swain, Weathers, and Niedrich 2008).[11] Therefore, in wave 3, we decided to keep only one item negatively worded (the third item in Table 2).[12] We reverse-coded the negatively worded item so that larger values for all six items indicate preferences for more regulation. We again use factor scores that have been rescaled to range from 1 to 5 as our outcome variable. Table 4 shows the factor loadings and Cronbach's α scores of the items in each country. Loadings range from 0.31 to 0.84 with a mean of 0.65. Cronbach's α ranges from 0.79 to 0.84, indicating the overall internal reliability of the scale.

Analysis and Results

To test the effect of the *scandal-capture* treatment on regulatory preferences, we run a pooled analysis using

[10]We did power analysis for each country in wave 3 using wave 2 results. In Australia, for example, we used the effect size of the capture treatment and the standard deviation of regulatory attitudes in wave 2 data. Using simulations, we found that we need a sample size of at least 900 to achieve 80% power at the .05 significance level. That is why we have a sample size of about 1,200 respondents for each country in wave 3.

[11]This two-factor structure in wave 2 data is not problematic for inferential purposes (see SI Appendix F, pp. 5–6, 11–12).

[12]We did this following additional pilot tests we conducted prior to wave 3 in Australia, Germany, and the United States, which are the countries where we found a separate factor consisting of "NOT" items in wave 1 data. We compared the factor analysis results of item wordings from waves 1 and 2 with those of wordings where we have only two NOT items and one NOT item (instead of three). Even with two NOT items, there is a tendency to create a factor of its own, so we decided to use one NOT item in wave 3.

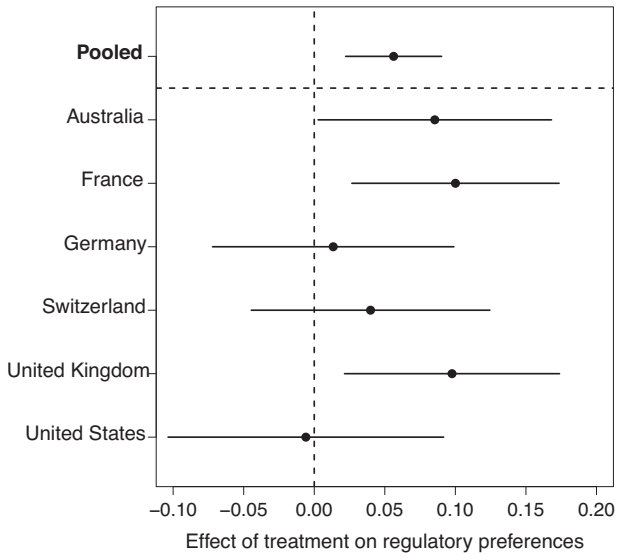
TABLE 4 Factor Loadings and Cronbach's α of Financial Regulation Items (Follow-Up Study)

	Australia	France	Germany	Switzerland	United Kingdom	United States
Item 1	0.66	0.64	0.73	0.60	0.66	0.70
Item 2	0.68	0.66	0.72	0.66	0.70	0.74
Item 3	0.46	0.33	0.31	0.43	0.46	0.51
Item 4	0.73	0.78	0.78	0.77	0.75	0.84
Item 5	0.55	0.63	0.62	0.52	0.61	0.54
Item 6	0.80	0.74	0.74	0.74	0.79	0.79
Cronbach's α	0.81	0.80	0.81	0.79	0.82	0.84

Notes: The first six rows show standardized factor loadings using minimum residual solution. The last row shows standardized Cronbach's α.

weighted OLS with country fixed effects, as well as weighted *t*-tests for the individual countries. Figure 8 displays the results pooled for all six countries as well as the effects for the individual countries. As expected in our preregistered plan, the positive effect of the *scandal-capture* treatment on regulatory preferences is replicated for the pooled sample in this wave ($b = 0.06$, $p = .00$).

FIGURE 8 Treatments' Effects on Regulatory Preferences (Follow-Up Study)



Notes: The top of the figure shows the effect of treatment when we combine the data from all countries and run a weighted OLS model with country fixed effects. Below that, the figure shows for each country the weighted difference in means between the control group and the treatment group on the outcome variable, *Regulation*. Bars indicate 95% confidence intervals. Sample sizes—Pooled: 3,699 (control), 3,696 (scandal-capture); AU: 580 (control), 584 (scandal-capture); FR: 648 (control), 642 (scandal-capture); DE: 622 (control), 623 (scandal-capture); CH: 603 (control), 606 (scandal-capture); UK: 647 (control), 647 (scandal-capture); and US: 599 (control), 594 (scandal-capture).

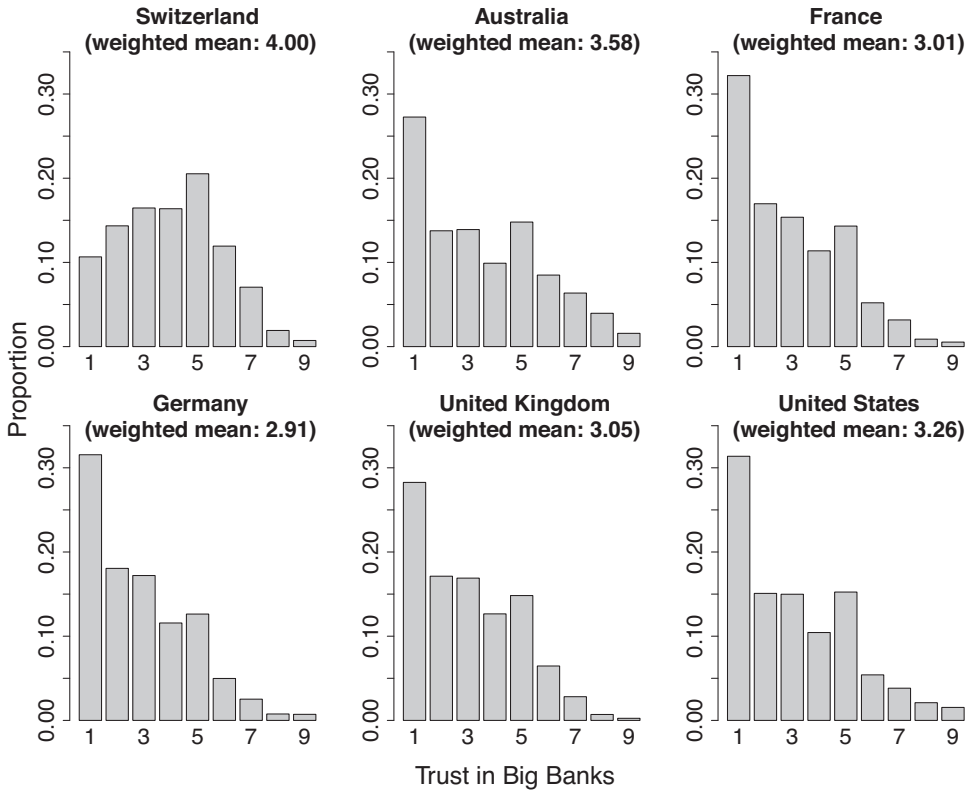
To put this result in perspective, this is about half the effect of a unit decrease in trust toward banks on regulatory preferences.

In country-specific analyses shown below the pooled results at the top of Figure 8, we find that the *scandal-capture* treatment increases regulatory preferences in Australia, France, and the United Kingdom. In Australia, the effect size is 0.09, which is 12% of the standard deviation of the outcome variable in the scandal-capture group. In France, the effect size is 0.10, which is about 15% of the standard deviation. In the United Kingdom, the magnitude is again 0.10 (15% of the standard deviation). Where we find effects, the effect sizes are similar to each other and to those observed in wave 2.

Overall, the findings we have across the six countries are robust. We also ran linear regressions after controlling for any pretreatment covariate imbalance, used an alternative outcome variable that averages respondents' answers to the six items on financial regulation, used models without weights, controlled for pretreatment covariates, ran analyses while taking into account pretreatment effects from wave 2 (Druckman and Leeper 2012), and conducted pre–post analysis (Clifford, Sheagley, and Piston 2021; see SI Appendix I, p. 15; SI Appendix J, pp. 16–19). In some of the analyses, the *scandal-capture* treatment no longer has an effect in Australia at the .05 significance level. But in other cases, the results of the main analysis are replicated.

Switzerland is the only country in which the *scandal-capture* treatment fails to have a significant effect in either experimental wave. We think we do not find effects in Switzerland because of the large role that banks play in the Swiss economy and the consequently tight relationship between the political elite and banks (Emmenegger 2017; Überbacher and Scherer 2020). We have data on respondents' trust in big banks, which we collected in wave 1, that are consistent with this

FIGURE 9 Distributions of Baseline Trust in Big Banks across Countries



Notes: Distribution of trust in big banks from wave 1 for each country is shown. Values of trust in big banks on the horizontal axes range from 1 ("Do not trust them at all") to 9 ("Trust them a great deal"). Weighted means are shown in parentheses. Sample sizes: 4,827 (Australia), 4,862 (France), 4,719 (Germany), 3,444 (Switzerland) 4,829 (United Kingdom), and 4,992 (United States).

explanation. Figure 9 shows the distributions of trust in big banks across countries, along with their weighted means. Respondents in Switzerland have significantly higher levels of trust in big banks compared to respondents in the other five countries. The modal response in Switzerland is 5—the midpoint of the scale—whereas in every other country the modal response is 1, the lowest possible number. This background of trust in banks suggests that there may be less of a latent Swiss opinion contra banks for scandal articles to activate than in our other countries. The comparative data on warmth toward bankers, presented in Figure 2 earlier in the article, tell a similar story; unlike the other countries, there is not a large group of respondents expressing the coldest possible value (0) toward bankers. These more positive predispositions that the Swiss have for bankers could be a result of the dominant role of banks in the Swiss economy and politics. Another potential interpretation is that the Swiss could have collectively established different boundaries between morally legitimate and illegitimate financial practices than in the other countries in our study (cf. Münnich 2016).

In wave 3, we do not find the *scandal-capture* treatment affecting regulatory preferences in Germany and the United States, unlike in wave 2. The null effects in Germany and the United States are not significantly different from the positive effects in Australia and France but are significantly different from the positive effect in the United Kingdom. We have evidence that this null effect is due to the particular characteristics of the treatment used for Germany and the fevered electoral climate of the United States in September 2020. For reasons of space, we restrict our discussion of this evidence to SI Appendix K (pp. 19–21).

With wave 3, we further probed the ability of a scandal treatment to move public opinion on financial regulatory preferences. A different operationalization of the *capture* treatment, using a different bank and a different scandal than in wave 2, has positive effects on regulatory preferences in the pooled analysis, thus

confirming our central finding from wave 2. At the level of individual countries, we find a positive effect on regulatory preferences in Australia, France, and the United Kingdom. As in wave 2, we do not find an effect in Switzerland, whose exceptional characteristics we have discussed. In Germany and the United States, we also do not find effects, unlike in wave 2, yet we have evidence that those results are due to particular contextual factors. Overall, the results suggest that the scandal treatment with a capture narrative is one that can be politically potent when discussed in the context of a banking scandal.

Conclusion

Corporate scandals are focal events—media coverage of them can force previously non-salient issues onto the political agenda (Birkland 1997; Jones and Baumgartner 2005). Such events do not just bring attention to issues; they can shift the preferences of voters (Bishop 2014). Yet political science has had little to say about how media coverage of corporate scandals influences public opinion on questions of economic regulation: a policy domain prone to charges of capture by business interests (Culpepper 2011; Hertel-Fernandez, Mildenberger, and Stokes 2019). To address that gap in knowledge, we have examined the effect of media coverage of bank scandals on voters' preferences for financial regulation. Using original, nationally representative three-wave panel survey data from six countries, we find evidence that news articles about bank scandals move people's preferences in a pro-regulatory direction.

Our findings have implications for several research areas. First, these results speak to the literature on media effects. There is a large body of work on the influence of the media on voters' attitudes and behaviors (Barnes and Hicks 2018; Dilliplane 2014; Ladd and Lenz 2009). We add to this research by focusing on the effect of the media in policy areas that usually are not at the center of political debate. Such low-salience domains are precisely where the media can play a normatively important role (Peterson 2021), bringing elites' and voters' attention to oft neglected issues. Just as the presence of media outlets enables electoral punishment for corrupt politicians (Ferraz and Finan 2008; Larreguy, Marshall, and Snyder 2020), we find that coverage of economic scandals creates calls for political punishment of companies—that is, stricter regulation—in public opinion.

Second, we have opened a window onto the potential effects of media coverage of scandals whose protagonists are not politicians. Political scientists have in the past studied the political bias of the media in covering political scandals (Galvis, Snyder, and Song 2016; Puglisi and Snyder 2011) and the dynamics of media coverage of political scandals themselves (Entman 2012; Nyhan 2014, 2017). These are important insights, but there are many unexplored avenues that could connect corporate scandals to important issues of democratic politics. Scandals represent a potential mobilizing resource to upend existing status quo equilibria. We have shown that coverage of corporate scandals can change public preferences, providing some clues about the role the media can play in accountability and democratic responsiveness in battles over policymaking, not just in elections. We invite other scholars to further open this window into the politically consequential effects of economic scandals.

Finally, our article adds to the political economy literature on financial regulation. Most previous work has focused on understanding the policies governments have adopted or rejected, especially in light of the 2008 financial crisis and the subsequent Eurozone debt crisis (Ganderson 2020; James 2018; Massoc 2020). These accounts relegate to the background the role of public opinion in shaping government's responses to these financial crises, even though this background resource is often central in allowing policy entrepreneurs to succeed in getting their policy adopted (Romano 2014; Ziegler and Woolley 2016). In this article, we have mainly focused on generalizable effects across countries. Future research could usefully explore how bank scandals may have different impacts on public opinion across different countries (cf. Münnich 2016). We argue in this article for the importance of bringing voters' preferences into the light and, by doing so, advance our understanding of how spectators can be brought into a fight even on low-salience issues like financial regulation that typically sit in the realm of "quiet politics."

Our study is a targeted one, focusing on how real bank scandals affect public opinion in countries at the center of world finance. But the fact that these scandals are a recurring feature of economic life in many countries, and that politicians have responded to past financial scandals with more stringent rules (Romano 2005), is suggestive of the broader importance of our findings. In many countries, we are witness to a crescendoing drumbeat of populist reaction against big banks and the political elite—a "banklash"—for which scandals can serve as triggering events. These events can have policy consequences. Just as minor seismic activity can be a precursor to major earthquakes, corporate scandals can release social forces that have the capacity to transform regulatory policy. It is high time political science paid attention to them.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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