Bank-Supervision in the EU and Blame Attribution in Germany

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Abstract

The 2010-12 European banking crisis triggered severe recessions, job losses, and austerity measures. In response, member states delegated some bank-supervision authority to the European Union (EU). We test if this delegation enables governments to shift blame for bank failures to the EU. Using a conjoint survey experiment with 1,724 participants in Germany, admittedly a least likely country for our test, we find that a hypothetical taxpayer-funded bailout reduces support for governing parties by between 12 and 18 percent, and that this effect is worse if the EU dominates bank-supervision. Heterogeneity tests find no blame-shifting advantages to delegation, across the gender, regional, socio-economic, education, and left-right divides, across varying levels of exposure to banks, and regardless of whether people know that the German bank regulator (*Bafin*) is independent from the government. Indeed, our findings reveal strong heterogeneous contrarian effects: Especially for old people and for those who self-identify as high-class, the SSM is a blame-attribution mechanism, compounding rather than moderating the fall in support for the government in the wake of taxpayer-funded bailouts. EU-level supervision also especially undermines public support for left-wing governments, costing the government 20-23 percent of the vote, and for Eurosceptic governments, which may lose more than half of the vote, while support for Eurosceptic non-government parties increases (14%). Thus, this study helps explore key patterns of public support for governments that delegate policies to the EU, and by implication support for delegation itself, across geographical spaces and social groups. This is also a study of the implications, in terms of public support for delegation, of IO-related outcomes such as bank-supervisions.

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Introduction

The crisis in Europe's banks, first as an aftershock to the 2008 Wall Street crisis, and then as a result of the Euro area's bailout policies towards some of its member states in 2010-12, was associated with deep recession, job losses and austerity policies. The crisis almost led to governments defaulting on their debts and to the Euro area's collapse. In the first phase, banks were bailed-out at great costs to taxpayers' money, and in the second phase they were bailed-in, imposing large costs on investors. People who expected banks to help provide prosperity and stability were furious with their governments, which they held accountable to the crisis. Governments were desperately scrambling for a response and leaders were ousted not necessarily because they were directly responsible for the banking crisis, but because in the eyes of the public the buck simply stopped with them. Extreme parties across Europe gained public support.

In response to this crisis, member state governments decided to launch the European Banking Union (EBU), including supranational banking supervision. The literature on EBU has so far analyzed this delegation of supervision authority mostly as a rational solution to a European market failure, with inter-state allocation conflicts (Epstein 2017; Howarth & Quaglia, 2016; Spendzharova 2013). However, inspired by the growing blame-avoidance literature we hypothesize that delegation can create opportunities for governments to avoid blame for supervisory failures by shifting it to EU actors. We explore this route by asking whether, and under what conditions, such blame-shifting is can be effective. Specifically, does delegation of bank supervision authority to EU institutions increase support for the government when the burden of bank failure or underperformance falls on citizens?

We fielded a pre-registered paired-profile conjoint survey experiment, recruiting a total of 1,724 adult citizen-residents of Germany in the summer of 2024. Germany, with its formally independent federal banking supervisor (*Bafin*) and initial reluctance towards the EBU, represents a least likely case for our hypothesis, thereby providing a stringent test. Each participant in the experiment chooses between two hypothetical and

randomly profiled parties running in national elections, under randomly assigned shocks to the banking system and crucially, randomly assigned balance of authority between the national government and the EU.

We find that a taxpayer-funded bailout, costs parties in government between 12 and 18 percent of the general public's support, and that contrary to our hypothesis, this effect is worse if the EU dominates bank supervision. Heterogeneity tests find no blame-shifting advantages to delegation, across the gender, regional, socio-economic, education, and left-right divides, across varying levels of exposure to banks, and regardless of whether people are aware that *Bafin* is independent from the government. Indeed, our findings reveal strong heterogeneous contrarian effects: For old people and for those who self-identify as high-class, the SSM is a blame-attribution mechanism, compounding rather than moderating the fall in support for the government in the wake of taxpayer-funded bailouts. EU-level supervision also especially undermines public support for left-wing governments, especially the more authority the SSM has, and especially the more extreme the left-wing party is, costing the government 20-23 percent of the vote. This is even more true for Eurosceptic governments, which may lose more than half of the vote, while support for Eurosceptic non-government parties increases (14%).

These findings provide important insights into how political elites use international organizations (IO) to shape public opinion and secure electoral advantages. Our study shows that supranational delegation can backfire, making it harder for national leaders to shift blame for crises onto international bodies, such as when the EU takes on a dominant supervisory role. While the concept of elite blame avoidance is well-established (Hinterleitner, 2017; Hood, 2011; Weaver, 1986), our study uniquely examines how the public perceives these strategies. This perspective is crucial for understanding the limits of blame avoidance and its implications for democratic governance as public perception plays a key role in shaping citizens' trust in political institutions and determining whether elites are held accountable, ultimately impacting the quality of democratic governance.

More broadly, as global governance has become politicized and contested, public opinion of international Organizations (IOs) has become increasingly important. In this context, our study helps explore some patterns

and consequences of public opinion toward IOs. Specifically, we analyze key patterns of public support for governments that delegate policies to the EU, and by implication for delegation itself, across geographical spaces and social groups. We also study the implications, in terms of public support for governments, of IO-related outcomes such as bank failures and the distribution of their costs between bank owners and taxpayers.

The next section develops the theoretical expectation for blame-avoidance through delegation, and the third section describes our design in detail. The fourth section reports results and the fifth section provides conclusions.

Theoretical Framework

For democratic polities to command legitimacy, it is essential that the actors responsible for making policies can be publicly held to account. Holding policy-makers accountable presupposes that responsibility for policies can be attributed to identifiable political actors (Rittberger *et al*. 2017; Wilson & Hobolt 2015). However, governments seek to avoid being blamed for contested policies. The growing literature on blame avoidance highlights that delegation of governance tasks to third parties, especially the European Union (EU) and its agencies, improves governments' blame avoidance opportunities.

The blame avoidance literature highlights the blame shifting effect of delegation to private agents (Bach & Wegrich 2019), independent non-governmental domestic public agents (Mortensen 2016) and international agents. The EU in particular is used as a scapegoat (Schlipphak & Treib 2017). Indeed, this may be one reason for member state governments to delegate tasks to the EU (Schimmelfennig 2020). Blame shifting to EU-level actors is more likely when EU policy is implemented at the EU-level rather than the national-level (Heinkelmann-Wild *et al*. 2020; Rittberger *et al*. 2017), when voters hold Eurosceptic views, in countries with longer EU membership, and where EU institutions are more visible targets for strategic attribution (León *et al*. 2018). Delegation to supranational EU bodies enhances governments' blame shifting opportunities compared

with delegation to intergovernmental EU actors (Heinkelmann-Wild *et al*. 2021), because under the latter governments are the policymakers, but under-deliver due to inter-governmental compromises. In return, EU agencies may shift blame to governments under national-level implementation (Heinkelmann-Wild & Zangl 2020) and federal governments may shift blame to regional governments (León *et al*. 2018). In general, the more autonomous an agency is from ministerial oversight, the more blame can be shifted to it (Mortensen 2016). Blame avoidance is conditioned by party ideology – it is likelier when policies conflict with government ideology (Sommer 2020).

Blame-avoidance scholars have studied contested bailout and austerity policies in the context of the European sovereign debt crisis (Heinkelmann-Wild *et al*. 2021; Sommer 2020) and EU migration policies (Heinkelmann-Wild *et al*. 2021; Heinkelmann-Wild *et al*. 2020; Heinkelmann-Wild & Zangl 2020; Rittberger *et al*. 2017), in the wake of a major crisis of the rapid transit railway system (S-Bahn) in the capital region of Berlin in 2009 (Bach & Wegrich 2019) and during the transformation of the Danish Railway Company (DSB) from 1995 to 2007 (Mortensen 2016).

This project aims to contribute to the blame avoidance literature by studying if EU member state governments can avoid blame for banking supervision failures. This is an issue area that has so far received little attention in the blame avoidance literature, in spite of its political and economic importance. Banking supervision is one of the most crucial regulatory functions of the modern state, as banking crises can have severe consequences for the domestic and global economy. Moreover, given the economic importance of the banking sector, financial sector instability poses a major threat for policy-makers to retain political office.

Banks regularly face liquidity risks (cash withdrawals exceeding cash reserves) and credit risks (borrowers not repaying their loans). Competition, profit-maximization and time-inconsistency can easily lead them to assume greater risks by keeping dangerously low reserves and lending to risky borrowers. The state can reduce such negative externalities by regulating banking risks, and providing banks with safe assets (sovereign bonds) and lender-of-last-resort services. Large banks may come to expect that the government even bail them out when

they become insolvent (Pagliari *et al*. 2020). For their part, governments need banks as sources of finance and as vehicles of redistribution among sectors and constituencies (Clift & Woll 2012; Hardie & Macartney 2016).

Thus, bank-state relations are characterized by interdependence, which might transform into a mutual-capture relationship (Calomiris & Haber 2014; Woll 2014). In this relationship, banks may subsidize excessive government borrowing and become powerful political agenda setters. As a result, state supervision of banks may be lax (Epstein 2017), and state guarantees of banks, formal or implied, may subsidize risk-taking ('moral hazard'), exacerbating rather than mitigating the above externalities (Strahan 2013). Increasingly risky government securities may gradually make bank balance sheets more fragile rather than safer.

We assume that voters are interested in cheap and abundant credit, provided by safe and stable banks, rigorously regulated by the state, but are poorly placed to contribute policy inputs and review the government's conduct in such an opaque policy area. Most people can only judge the outputs of bank supervision. Eventually resorting to taxpayers' money in order to fund costly bank bailouts, not to mention deep banking crises that result in assets, income and job losses, are a sign of government supervision failure (Funke *et al*. 2016). The redistributive effects of bailouts may strengthen the feeling that banks have captured state's policies at the expense of taxpayers (Chwieroth & Walter 2017; 2019). Financial crises are thus normally followed by political turmoil (Mian *et al*. 2014), protests and riots. In democracies, voters punish politicians that fail to employ timely and efficient crisis management or prevention measures (Hobolt & Tilley 2016; Hernández & Kriesi 2016); Extreme parties gain in support and influence (Hobolt & de Vries 2016; Nicoli 2017), government majorities shrink and legislatures become ever more fractionalized. Governments' interest in blame avoidance over bank supervision failure would thus seem to be evident.

Prior to the financial and economic crisis in the EU, banking supervision was a predominantly national competence, exercised by national regulatory authorities. Supervisory policies were coordinated at the EU-level through the so-called Lamfalussy Process, yet given its non-binding approach, it remained a weak instrument for the EU-wide supervision of banks. Against the backdrop of the economic and financial crisis in the EU,

member state governments decided to create the EU's Single Supervisory Board (SSB) in 2013, and entrust it with the task to supervise the EU's most important banks. Inspired by the above literature we hypothesize that the delegation of banking supervision tasks from a domestic prerogative to an EU-level competence allows governments to shift blame for banking supervision failures to the novel EU agency. The SSB is a likely target of blame because it is a relatively autonomous supranational agency, responsible for implementation of supervision for Europe's largest banks, about 130 of them (although it shares implementation powers with national agencies for almost 6,000 smaller banks – see Donnelly 2018).[1]

Hypothesis: When the public suspects that supervision of banks has failed, it will attribute less blame to the party in government, the more authority the SSB has to directly implement banking supervision.

Inspired by blame avoidance literature, which expects government parties to try harder to shift blame when a particular policy failure conflicts with their ideology (Sommer 2020), we expect blame shifting in banking supervision to be especially manifest under left-wing governments, which ideologically are not supposed to pander to banks at the expense of the wider public. The literature also leads us to expect that blame shifting in banking supervision is especially manifest in member states with a strong Eurosceptic electorate.

In this way this manuscript also contributes to the literature on European Banking Union (EBU), which has so far analyzed the SSB mostly as a rational solution to a European market failure, with inter-state allocation

[1] We don't expect the SSB to have allowed member states' governments to obfuscate blame for banking supervision failure, which is another mechanism suggested in the literature for avoiding blame (Bach & Wegrich 2019; Mortensen 2016; Rittberger *et al.* 2017; Wilson & Hobolt 2015), because the potential cost of such failure are so large that they cannot be depoliticized (Heinkelmann-Wild *et al.* 2020) – voters will demand a scapegoat and the government can easily inform them about the SSB even if it is otherwise a specialized body that is not familiar to voters.

conflicts (Epstein 2017; Howarth & Quaglia, 2016; Spendzharova 2013), but has failed to account for the important blame avoidance motivation in EBU.

Design Overview[2]

Setting: To test the causal effects of bank supervision failure on blame attribution across the varying presence of other information signals, subjects have participated in a conjoint survey experiment. As in real world choices that typically involve complicated tradeoffs, conjoint survey experiments allow testing and controlling for the extent to which multiple randomly varied attributes affect subjects' choices through a set of alternatives (Hainmueller *et al.*, 2014). Conjoint surveys can also help to conceal the purpose of the experiment. Conjoint survey experiments have been shown to have strong external validity, low social desirability bias, and cognitive burdens that do not increase much as the number of attributes and tasks grow (Clayton *et al.* 2023).

Recruitment: To ensure high statistical power of 0.95, 1,724 adult citizen-residents of Germany were randomly sampled,[3] aiming for a Minimum Detectable Effect of two percent (see power calculations below). The sampling was done in two survey windows. We first ran a pre-test pilot experiment with 127 participants on 25 July 2024, from 9:39 AM to 12:31 PM CET (see report at OSF.IO/4AFQ8). We then ran the survey with an additional 1,597 participants from 11:34 AM CET on 11 August 2024, to 10:06 AM on 17 September 2024 (see full description and analysis of survey performance and quality in Appendix B). Germany was chosen for this experiment because it is the most reluctant large member state of EBU. German voters are generally regarded as highly risk-averse in banking, but a politically-important part of the German banking sector is made

[2] This is a brief description. The full survey questionnaire and flow are available at OSF.IO/4AFQ8.

[3] Participants were recruited to the experiment via *Prolific*, which is a commercial survey company that recruits subjects to participate via its online survey platform.

of banks that are chartered to prioritize financial stability over profits, and all banks are supervised by an independent federal authority – *Bafin*. It thus can be expected that German voters on average would be less likely to attribute blame to their government if a bank fails, compared with voters in other countries. Indeed, by its reluctance to join EBU the German government seems to have revealed its lack of interest in attributing blame to an EU authority. All this makes Germany a less likely case of blame attribution in banking, so If the experiment supports the hypotheses it is likely to do so in other member states too.[4]

Pre-experiment survey: Before the experiment began, participants signed a consent form, and then answered a series of questions that recorded their socio-demographic characteristics, social and political values, trust in political institutions, their attitudes towards European integration, and their exposure to banking services.[5] The questions included in the pre-experiment survey were selected based on their relevance for either heterogeneity tests or re-weighting of the sample so that it matches the voter population in Germany. Attention and manipulation checks were included in the pre-experiment survey and at the end of the experiment.

General experiment setup: After completing the pre-experiment survey, each participant was presented with different hypothetical profiles of two parties who are running for an upcoming hypothetical election to the federal legislature, and was asked to decide which of them he/she supports more based on randomly assigned values of a given set of attributes (see Tables A1 and A2 in Appendix A). The context of the election was shaped by hypothetical pre-election developments, appearing as additional attributes in the conjoint design. Since the

[4] The consent form clearly conveyed to participants that they have the right to withdraw from the study at any point.

[5] The authors are grateful to Andreas Kern for his help in formulating the questions on banking services. See Aklin & Kern (2021) and Kern & Amri (2021). The formulations of other pre-survey experiment questions are based on Rudolph *et al*. (2023), Senninger & Bischof (2023), the European Social Survey (ESS), the German Longitudinal Election Survey (GLES), or the World Values Survey (WVS).

values of these developments must be randomly assigned they may differ across the two party profiles, which means that the two parties cannot be assumed to be fighting each other in a single election. Participants were thus informed that the parties are not competing against each other in a single election. Rather, each party is involved in a different hypothetical election (hence, it is participants' political preferences that are being studied, rather than their actual voting behavior). Conventionally, participants in conjoint experiments are asked to make choices in a context of single election, but our design could be equivalent to the conventional one if participants' preferences are consistent regardless of whether parties compete against each other (henceforth direct contest) or not (indirect contest). We measure the potential inconsistency in participants' preferences across direct and indirect contest (when all else is equal), which we refer to as Indirect Contest Choice Inconsistency (ICCI), and suggest a mechanism for correcting results accordingly (see next sub-section).[6]

Each party profile contained nine attributes. The first attribute was the party's place in or out of government (large party in opposition, small party in opposition, new party (not yet represented in the legislature), small party in government, large party in government). The second attribute was a position on economic ideology (high values=anti-market and private property), and each party was randomly assigned a position along a 5-point scale (center position, and moderate and extreme positions in either direction). Randomly assigned positions along a 5-point scale for each party followed over Social values (Liberal-Conservative) and European integration (pro-anti).

Five additional attributes, again randomly assigned along a 5-point scale, represented hypothetical developments occurring, or situations prevailing before the particular election that each party is contesting, simulating the

[6] Alternatively, the party-attributes could be embedded in a vignette design, each vignette representing a particular pre-election development, but this would effectively amount to a conjoint design with identical values across parties for the attributes representing those developments, which violates the randomization that is essential to the experiment.

balance of banking supervision authority (between the federal authority and the EU authority), the burden of bank bailouts on taxpayers, deterioration of banking services for individuals, and the national and personal business cycles (to control for non-banking economic effects). Hence, in each decision participants were exposed to one of 5^9=1,953,125 combinations of attributes per party, so more than 3.8 trillion different possible combinations of attributes for the two parties in each task, with equal probability. The order of presentation of attributes was fixed, as described above, to make it easier for participants to choose between the parties.

Choice and potential choice inconsistency: Binary support for each party took place in two rounds. Round 1 choice had three options: 1=Support Party A; 2=Support Party B; 3=Support none of these parties, or don't know. Option 3 is meant to help simulating voter turnout and making support for each party more independent of the other. We refer to this as optional binary choice, or three-way choice. Following this, in the same screen, in Round 2, participants were asked to decide again, this time without Option 3 (forced binary choice, or two-way choice). Following this, each participant was presented with a new pair of parties (the next task) and again asked to express support for each one of them.

This process was repeated eleven times (participants were presented with a total of eleven pairs of parties, or eleven tasks).[7] However, only eight of these tasks (Tasks 2-5, 8-11) were later used for testing this study's hypothesis. Tasks 1, 6 and 7 were used to measure two potential choice inconsistencies. To measure the

[7] Bansak *et al*. (2018), Bansak *et al*. (2021), Clayton *et al*. (2023) and Jenke *et al*. (2021) suggest that this is neither an excessive number of attributes nor an excessive number of tasks. See Lehrer *et al*. (2023) and Frederiksen (2023) for recently published examples of successful conjoint experiments with ten tasks. Horiuchi *et al*. (2018) employed nine attributes in their conjoint experiment. Clayton *et al*. (2023) also present evidence that respondents are not confused by potentially inconsistent party profiles (with unusual combination of values across attributes due to randomization) or non-divergent pairs of profiles (with similar attribute values).

swapping error – participants' choice inconsistency even without shifting from direct to indirect choice (Clayton *et al*. 2023) – Task 2 was repeated in Task 7 (both under indirect contest), with the exact same pair of profiles, but reverse columns' order. Clayton *et al*. (2023) find that participants do not realize that they are being presented with the same pair of parties. Participants' forced binary choice in Task 7 were compared with their choice in Task 2. Clayton *et al*. (2023) suggest that the average probability of the swapping error among participants – τ – can be estimated as the proportion disagreement between the choices made in these two identical tasks.[8]

Participants' ICCI was measured by replacing the five attributes representing pre-electoral developments with five additional party characteristics in Tasks 1 and 6 (see Table A3 in Appendix A). The same two party profiles appeared in both tasks with reverse order of columns (profiles). Participants were told in Task 1 that the parties are competing directly with each other in the same election, and in Task 6 that they are competing in different elections. Participants' ICCI can be estimated similar to Clayton *et al*. (2023) τ, comparing participants' forced binary choice in Task 6 with their choice in Task 1. If ICCI > τ, then some participants' choices are inconsistent between direct and indirect contest.[9]

Hypothesis testing: The Average Marginal Component Effect (AMCE) of each attribute on the support for the incumbent party is estimated in linear or probit regression analysis. In this analysis, each party in each pair (task)

[8] If participants' choices are informed, τ should be lower than the 0.5 probability of a random choice; Clayton *et al*. (2023) indeed find that it is normally around 0.25.

[9] Clayton *et al*. (2023) find that τ is not sensitive to the experiment's complexity (the number of attributes and the number of words describing attribute values). We thus assume that τ is identical for Tasks 1/6 and Tasks 2/7. To be safe, we include the additional party characteristics – ideological positions on the Right-Left divide, religion, and climate policy, and the party leader's age and gender – to keep the number of attributes at nine.

is treated as an individual observation. Therefore, the total number of observations is equal to 16 times the count of reliable participants (recall that only Tasks 2-5, 8-11, are used for testing the hypothesis). Each of the two party-support variables (the three-way and the two-way choice variables) is alternatively specified as the dependent variable (*B3way* and *B2way* respectively). The two binary choices are operationalized as dummy variables coded 1 if the party was chosen, 0 if it was not (which in the optional/three-way binary choice could also represent no party being chosen). The independent variables include the nine attributes detailed above for these eight tasks, personal traits based on the pre-experiment survey, and a task count. As Clayton *et al*. (2023) suggest, the AMCE from a linear regression can be corrected for the swapping error by dividing it by $(1-2\tau)$. The same correction will be alternatively applied for the ICCI.

Each of the nine attributes are operationalized with a dummy for the two values in one of the directions from its center value (0 representing the two values in the opposite direction, and the center value). Specifically, *AGovParty* is a dummy for a party in government (either small or large). *AEconParty*, *LCParty*, *AEUParty* are dummies for parties that are respectively economically left-wing, socially liberal, anti-EU. *AEUSuper* is a dummy for EU-dominated supervision of banks. *ABailoutTax*, is a dummy for bank bailouts that are partly or fully funded by taxpayers. *ABadBankPers* is a dummy for poor credit and deposit terms at a personal level. *ARecession* and *AIncomeFallPers* are dummies for business cycle downturns at respectively the national and personal levels. For the socio-demographic controls, *NonMale* is a dummy for female or third-gender participants and *Age* is their age. *Education*, *Class*, *EconView*, *LeftRight*, *EUimage*, *EUmembership* and *TrustInstitutions* are ordinal scales increasing with respectively education, low social class, economically left-wing views, politically right-wing views, a negative image of the EU, intention to vote 'Leave' if an EU membership referendum were held, and general trust in institutions.[10] The observations are clustered on participant IDs, since choosing one party in a pair is a

[10] The latter scale is an index calculated as the average value across the nine separate institutional trust scales (using the alpha command in Stata).

choice against the other, and to account for unobserved participant traits that affect the vote (Hainmueller *et al.*, 2014, 16-17).

To test whether participants' responses are sensitive to the sequence of the experiment, the task count variable is interacted with each of the attributes. Statistically significant coefficients for these interactions would signal such sequence effect, in which case the later tasks may be dropped from the data. In the next draft of this paper, results will also be analyzed with and without those who failed the post-experiment attention and manipulation checks, and with or without the 10 percent of fastest participants to complete the survey (in case this indicates lack of attention). Since the sample is not a nationally representative population-based sample, robustness tests will make sure that sampling does not drive the findings. This will be done by re-weighting the sample on age, gender, region, education, class and EU preferences so that it matches the voter population in Germany. Alternatively, the key treatments will be interacted with these variables (Horiuchi *et al*. 2018; Senninger and Bischof 2023).

However, for the purpose of hypothesis testing, *Gov* and *Super* are replaced by a set of three government-supervision dummies. One (*GovSSM*)represents parties in government (large or small) when the EU authority (SSM) is (mostly or entirely) responsible for banking supervision. Two additional dummies represent non-government parties, with either EU-dominant or supervision of banks (*NonGovSSM*), or the EU playing a minor supervisory role (*NonGovNonSSM*). Thus, the default case in this specification is that of a government party when the EU plays a minor role in bank supervision.[11]

[11] Since probabilistically 40 percent of observations will include a party in government, and 40 percent of parties will operate under a (mostly) supranational banking supervision, we expect approximately 16 percent of observations to be of an incumbent party under EU banking supervision. Similarly, we expect 24 percent of observations to be of a government party under non-EU banking supervision.

We then interact each of *GBanks* and *YBank* with each of the these three *Gov-Super* dummies, and of course also specify *GBanks* and *YBank* on their own. The coefficients of *GBanks* and *YBank* represent the effect of bad banking events on support for government parties when the EU does not dominate banking supervision. The effect of such events on support for the government when the EU does dominate banking supervision is the sum of the coefficients of each of these two dummies and the coefficients of their interactions with *GovSSM*. Thus, the coefficients of these interactions represent the difference in the effect of bad banking events on support for government when bank-supervisions alternates from the national to the EU levels. Our hypothesis would be supported if the coefficient of the interaction of *GBanks* with *GovSSM* is positive.

Other classifications for potentially interesting heterogeneity checks involve left-wing government parties and Eurosceptic participants, which the literature expects to more susceptible to blame avoidance in this context, participants having high exposure to banking services and to risky banks, those who do not trust banks, old people (who have more savings), participants who may not expect to pay for bank bailouts because they are poor (egocentric logic) or live in poor states (socio-tropic considerations), participants who are less expected to attribute blame to EU authorities because they are educated and/or aware that the federal supervising authority (*Bafin*) is independent from the government, and participants with left-wing political and economic preferences (who are likelier to expect state intervention). Again, the causal effects of these traits will be estimated with the benefits of randomization within such subgroups of participants and compared to the general case (Clayton *et al*. 2023).

Test power and Minimum Detectable Effect (MDE): A G*Power analysis (Faul *et al*. 2007; 2009) was conducted to determine the MDE in testing our hypothesis, consistent with high levels for statistical power (β=0.95) and significance level (α=0.05), and given the sample size.[12] Testing our hypothesis requires the specification of a

[12] Calculator can be downloaded at: https://www.psychologie.hhu.de/arbeitsgruppen/allgemeine-psychologie-und-arbeitspsychologie/gpower.html. From the Test family menu select "F tests". From the

two-way interaction. Taking into account that the test for each of the hypotheses involves an interaction among two or three binary dummies (1 df), and the full sample size is 25,700, the MDE is 0.02. In heterogeneity tests the sample size is smaller, and the corresponding MDE is report in each table (assuming β=0.95 and α=0.05 throughout).

Preliminary regression analysis of conjoint experiment results

As Table B1 in Appendix B shows, basic regressions without interactions show that participants responded strongly to negative economic shocks, both at the national and personal level, choosing against any party with a profile that included such events. This is true regardless of the dependent variable or the functional form of the regressions. According to probit estimates, taxpayer-funded bank bailouts, and personal disappointment with deposit and credit opportunities result in a fall of 16-19 percent in support for parties; national and personal economic downturns have even more dramatic effects. We interpret this as evidence that people react against the entire political class when faced with such hardships. There is a small incumbency effect, but only when

Statistical test menu select "ANOVA: Fixed effects, special, main effects and interactions". From the Type of power analysis menu select "Sensitivity…" The df is the product of the (numbers of groups, minus 1) in each of the interacted variables (for hypothesis testing), which in any interaction among dummies is always 1. The number of groups is the product of the numbers of groups in each of the interacted variables, which is 4 in our case. To the authors' knowledge, no survey experiments exist on the blame voidance effect of European Banking Union. Thus, there is no benchmark level for the effect size of interest. The G* Power 3.1 manual recommends a conventional effect size of 0.1 (p.29), but we think that even a 5 percent change in support for parties is important in parliamentary representation. Indeed, 0.05 is the median AMCE in 15 highly cited forced-choice conjoint experiments (Schuessler & Freitag, 2020).

faced with an option not to choose between the parties, which means the effect exists only in relation to the undecided, not in relation to non-government parties. Other significant coefficients reflect the biases of the particular sample of participants. We henceforth base our analysis on the results of probit regressions, which are more appropriate given the bounded and binary nature of the dependent variable, and the ease of interpretation (probabilities). Results in OLS regressions differ only in that the magnitude of the coefficients is smaller than in probit estimations. We also focus on the three-way choice, rather than the two-way choice, because the former is more realistic (voters can be undecided, and abstain from voting) and the latter, while common in conjoint experiments, forces the undecided to make a choice, which may result in random results (it may also a bit more prone to task bias – see discussion on Table B2 in Appendix B).

Before testing our hypothesis, we would like to confirm if participants generally tend to attribute blame to governments for negative economic shocks. In Table 1 we interact the four dummies for bad economic events with the dummy for being a party in government (Column 1) and a dummy for being a large party in government (Column 2). Unexpectedly, when bank bailouts are taxpayer-funded, participants on average seem to attribute less blame to government parties, especially large ones, than to non-government parties: The coefficient of the interaction term in Column (2) is 0.112, compared with a coefficient of -0.211 for non-government parties or minor government parties. While on a net basis large government parties are less popular than before the bailout, in relative terms this is a potential gain for them over other parties. Furthermore, participants do not seem to attribute anymore blame to government parties than to non-government parties following other negative events.

Table 1: Do voters blame government parties for bad economic news?

	(1)	(2)
A_*GovParty*	0.019	0.010
	(0.032)	(0.039)
A_*BailoutTax*	-0.215***	-0.211***
	(0.023)	(0.020)
A_*GovParty* × A_*BailoutTax*	0.066*	0.112***
	(0.035)	(0.042)
A_*BadBankPers*	-0.142***	-0.151***
	(0.022)	(0.019)
A_*GovParty* × A_*BadBankPers*	-0.048	-0.056
	(0.035)	(0.044)
A_*Recession*	-0.270***	-0.266***
	(0.022)	(0.020)
A_*GovParty* × A_*Recession*	0.027	0.034
	(0.035)	(0.043)
A_*PersIncomeFall*	-0.346***	-0.333***
	(0.023)	(0.020)
A_*GovParty* × A_*PersIncomeFall*	0.001	-0.064
	(0.035)	(0.043)
A_*EconParty*	-0.212***	-0.212***
	(0.020)	(0.020)
A_*LCParty*	0.264***	0.264***
	(0.019)	(0.019)
A_*EUParty*	-0.502***	-0.501***
	(0.021)	(0.021)
A_*EUSuper*	-0.021	-0.021
	(0.017)	(0.017)
Task	-0.015***	-0.015***
	(0.002)	(0.002)
NonMale	-0.100***	-0.100***
	(0.023)	(0.023)
Age	-0.001	-0.001
	(0.001)	(0.001)
Education	-0.027***	-0.027***
	(0.009)	(0.009)
Class	-0.023	-0.024
	(0.014)	(0.014)
EconView	0.003	0.002
	(0.011)	(0.011)
LeftRight	0.059***	0.059***
	(0.015)	(0.015)
EUimage	0.012	0.012
	(0.017)	(0.017)
EUmembership	0.000	0.001
	(0.026)	(0.026)
TrustInInstitutions	-0.004	-0.004
	(0.021)	(0.021)
_cons	0.369***	0.376***
	(0.141)	(0.140)
N	25,700	25,700
pseudo R^2	0.067	0.067

Results of probit regression. Dependent variable is *B3way*. In Column 2, the dummy for being a party in government relates to large parties only. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. Other variables are based on pre-experiment survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Appendix C we explore heterogeneous effects of these results, and find that participants' leniency towards large government parties in the wake of taxpayer-funded bank bailouts is typical of participants who self-identify as non-male, are young (within the bottom three quartiles of the age distribution), live in rich Länder, self-identify as low-class (see Table C1).[13] Such leniency is also typical of participants without a university degree, with pro-EU[14] and/or economically right-wing views (Table C2), with politically-left-wing views, high exposure to banks,[15] preference for working with protected banks, [16] and not being aware that *Bafin* is independent from the government (Table C3).

Our interpretation of all this is that people who stand most to lose from bank collapses are likelier to approve the bailouts and to see taxes as a mechanism for sharing the pain, which they anyway expect to shoulder. People who are especially concerned with social stability may also be lenient with large parties in government. This

[13] Länder are classified as rich/poor if their average household income (2019) according to the Federal Statistical Office of Germany (https://en.wikipedia.org/wiki/List_of_German_states_by_household_income) is above/below Germany's national average.

[14] Participants were classified as having Eurosceptic views if they answered either that they have a negative image of the EU, would vote anything other than "Remain" in a hypothetical referendum on Germany's EU, or do not trust the EU. Participants were classified as having pro-EU views if they provided none of these answers.

[15] Participants who self-identified as keeping most or all of their savings in banks, or as having large debt repayments to banks.

[16] Participants who self-identified as keeping most or all of their accounts in protected banks. While all banks in Germany have deposit insurance, some banks such as Sparkassen und Giroverband, Landesbanken, or Volks- und Raiffeisenbanken are also protected against their own insolvency. We asked participants about their preferences for such banks.

perhaps may explain the leniency of non-male, pro-EU and politically-left-wing people (who may care more about stability of institutions) people who self-identify as low-class (who may not expect to pay much taxes but still depend on functioning banks) and those from rich Länder and with high exposure to banks (who may have more to lose without a bailout). Participants who prefer working with protected banks are likely to place a premium on stable banking, and perhaps thought about their protected banks when answering our survey, as deposit insurance does not cover against the loss of a good source of credit. As Table C4 shows, the dummies that are used to classify participants for the purpose of this heterogeneity analysis are very weakly correlated with each other (with the exception that politically left-wing views are somewhat associated with economically left-wing views, as are Euroscepticism and mistrust of institutions. This suggests that the above results are for the most part not mere reflections of each other.

It is less straightforward to explain the age, education, economic ideology and *Bafin*-awareness factors uncovered here. We would expect the old to be more concerned about bail-ins, which wipe out their shareholder value, more than bailouts, which mostly tax the young's wages. We would expect university graduates to be better informed about the merit of preserving financial institutions. And why would economic right-wingers support bailouts? Why would people who know that *Bafin* is independent, not be lenient with government parties trying to manage a banking crisis? Perhaps the answer lays in the regulatory balance between the national and EU levels?

Testing our hypothesis

We now turn to testing our hypothesis. Does the SSM provide government with blame avoidance opportunities when having to bailout banks? In Table 2 we interact the tax-funded bailout dummy (*BailoutTax*) with each of the government supervision dummies detailed above: cases of a party in government when bank supervision is with the SSM (*GovSSM*), a non-government party when bank supervision is with the SSM (*NonGovSSM*) and a

non-government party when bank supervision is not with the SSM (*NonGovNonSSM*). The default case is thus a party in government when bank supervision is not with the SSM, so the difference between the coefficient of the interaction of *BailoutTax* with *GovSSM* and the coefficient of *BailoutTax* is the blame-avoidance effect of the SSM. Column (1) operationalizes the cases involving bank supervision mostly or entirely by the SSM. Column (2) operationalizes the cases involving bank supervision entirely by the SSM. While the coefficient of *BailoutTax* is estimated at -0.12, or -0.14 (the case when the SSM is not responsible), the coefficient of the interaction is not statistically significant. This means that if a taxpayer-funded bailout occurs, support for the government is not significantly different if the SSM assumes responsibility for bank supervision. So no evidence of blame avoidance effect. Indeed, if any, the results suggest a slight blame-enhancing effect for SSM supervision, as the coefficient of A_*BailoutTax* in Column (2) is substantively greater than in Column (1). This result means that if moderate SSM leadership is grouped with national-level supervision, support for the government falls further.

We proceed to test our hypothesis under various heterogeneous conditions, assuming that the SSM has sole responsibility for bank supervision. Table 3 shows that the blame-avoidance effect is absent for males and non-males alike, for young people, and in all regions and across all classes (and people in poor Länder are especially angry about taxpayer-funded bailouts). Interestingly, for old people and for those who self-identify as high-class, the SSM turns into a blame-attribution mechanism (columns 3 and 7), compounding rather than moderating the fall in support for the government. Tables 4 and 5 show that the blame-avoidance effect is absent across additional types of divides. Interestingly, people who do not trust (mostly national) institutions (column 3 in Table 4) are especially angry about taxpayer-funded bailouts, while those that do trust institutions, people with anti-EU views (columns 4 and 5 in Table 4 respectively), and those who prefer working with non-protected banks (column 6 in Table 5) are not angry with the government at all. Table 5 also shows that the SSM's blame avoidance effect is absent regardless of whether people are aware that Bafin is independent from the government.

Table 2: Does the SSM provide blame avoidance to governments?

	(1)	(2)
GovSSM	0.020	-0.046
	(0.033)	(0.043)
A_*BailoutTax*	-0.119***	-0.138***
	(0.035)	(0.031)
GovSSM × A_*BailoutTax*	-0.075	-0.052
	(0.053)	(0.067)
NonGovSSM	-0.016	-0.038
	(0.030)	(0.035)
NonGovSSM × A_*BailoutTax*	-0.109**	-0.077
	(0.049)	(0.057)
NonGovNonSSM	0.005	-0.015
	(0.027)	(0.024)
NonGovNonSSM × A_*BailoutTax*	-0.087*	-0.076*
	(0.045)	(0.039)
A_*EconParty*	-0.212***	-0.212***
	(0.020)	(0.020)
A_*LCParty*	0.264***	0.264***
	(0.019)	(0.019)
A_*EUParty*	-0.502***	-0.501***
	(0.021)	(0.021)
A_*BadBankPers*	-0.162***	-0.162***
	(0.018)	(0.018)
A_*Recession*	-0.260***	-0.259***
	(0.018)	(0.018)
A_*PersIncomeFall*	-0.346***	-0.346***
	(0.019)	(0.019)
Task	-0.015***	-0.015***
	(0.002)	(0.002)
NonMale	-0.100***	-0.100***
	(0.023)	(0.023)
Age	-0.001	-0.001
	(0.001)	(0.001)
Education	-0.027***	-0.027***
	(0.009)	(0.009)
Class	-0.023	-0.023
	(0.014)	(0.014)
EconView	0.003	0.003
	(0.011)	(0.011)
LeftRight	0.059***	0.059***
	(0.015)	(0.015)
EUimage	0.012	0.012
	(0.017)	(0.017)
EUmembership	0.001	0.001
	(0.026)	(0.026)
TrustInInstitutions	-0.004	-0.004
	(0.021)	(0.021)
_cons	0.365**	0.383***
	(0.142)	(0.141)
N	25,700	25,700
pseudo R^2	0.067	0.067

Results of probit regressions. Dependent variable is *B3way*. In Column (1) the suffix SSM refers to cases of bank supervision mostly or entirely by the SSM. In Column (2) it refers to cases of supervision entirely by the SSM. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. Other variables are based on pre-experiment survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table 6 we test whether the SSM provides blame-avoidance opportunities specifically to Left-Wing (*LW*) parties (in their economic agenda) in government. For this, we split *GovSSM* into a dummy for the case of left-wing parties in government when the SSM dominates bank supervision (*LWGovSSM*) and a dummy for the case of other parties in government when the SSM dominates bank supervision (*NonLWGovSSM*). We also add a dummy for the case of non-left-wing parties in government when the SSM does not dominates bank supervision (*NonLWGovNonSSM*). In this way, the default case (the one not accounted for by any dummy) is that of a left-wing party in government when the SSM does not dominate bank supervision; the effect of taxpayer-funded bank bailouts for this default case is the coefficient of *BailoutTax*. In Column (2) we specifically focus on large left-wing parties in government (the default case being any other type of party in government when the SSM does not dominate bank supervision). In column (3) we focus on cases of a left-wing party in government when the SSM is solely responsible for bank supervision, and in Column (4) we focus on extreme left-wing parties in government. We find that the SSM is a blame fostering mechanism for left-wing government (Column 1), rather than blame-avoiding. this effect weakens when focusing on large left-wing parties in government (Column 2), but strengthens the more authority the SSM has (Column 3) and especially the more extreme the left-wing party is (Column 4), costing the government 20-23 percent of the vote.

In Table 7 we test whether the SSM provides blame-avoidance opportunities specifically to Eurosceptic government parties. For this, we repeat the methods and classifications used in Table 6, this time for Eurosceptic governments (*EUSc* replaces *LW* in variables' names). In this way, the default case (the one not accounted for by any dummy) is that of a Eurosceptic party in government when the SSM does not dominate bank supervision; the effect of taxpayer-funded bank bailouts for this default case is again the coefficient of *BailoutTax*. We find that Eurosceptic parties in government are punished for a taxpayer-funded bailout when the SSM dominates bank supervision compared with a bailout when the SSM is not dominant (Column 1), but perhaps not in the case of large Eurosceptic parties in government (Column 2). The effect is again stronger if the SSM is the exclusive supervisor of banks (Column 3). Notably, extreme Eurosceptic parties in government face the most

severe consequences, potentially losing up to half of their public support (Column 4). The loss of public support is greater for Eurosceptic parties than for left-wing parties, substantively and in statistical significance.

In Table 8 we stray a bit from strictly testing the blame avoidance hypothesis, and focus on the effect that bank bailouts under EU supervision have on non-government Eurosceptic parties. This is relevant when studying public opinion and IOs because public support for such parties is often argued to increase following policy failures at the EU level. To estimate this effect, we split *NonGovSSM* into a dummy for the case of non-government Eurosceptic parties when the SSM dominates bank supervision (*EUScNonGovSSM*) and a dummy for the case of other non-government parties when the SSM dominates bank supervision (*NonEUScNonGovSSM*). We also add a dummy for the case of non-government non-Eurosceptic parties when the SSM does not dominates bank supervision (*NonEUScNonGovNonSSM*). In this way, the default case is that of a non-government Eurosceptic party when the SSM does not dominate bank supervision; the effect of taxpayer-funded bank bailouts for this default case is the coefficient of *BailoutTax*. In Table 8, the coefficients of *EUScNonGovSSM* represents the relative gain in public support for non-government Eurosceptic parties, when the EU dominates bank supervision and a taxpayer-funded bank bailout occurs. This gain amounts to 14 percent.

Our interpretation of all of these results is that supporters of left-wing governments value state autonomy as a defense against 'the forces of global capitalism'. For them, the EU may be part of these forces and if so they probably blame their government for (being complicit in) ceding authority to the EU. Of course, this only becomes an issue when there is actually a problem in banks. Likewise, supporters of Eurosceptic governments are upset when they are complicit in delegation to the EU, which is the focus of their outrage. This frustration explains why Eurosceptic opposition parties gain under such circumstances. In this sense, the loss of public support for governments handling a crisis after delegating authority to the EU reflects lack of support for delegation itself. Generally, the lesson we draw from our findings is that delegation on average does not provide blame avoidance opportunities for governments.

Table 3: Does sole SSM authority provide blame avoidance opportunities to governments? Gender, age, Länder and class

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GovXSSM	0.049	-0.123**	-0.055	-0.046	-0.054	-0.031	0.052	-0.029
	(0.062)	(0.059)	(0.091)	(0.049)	(0.050)	(0.081)	(0.070)	(0.048)
A_*BailoutTax*	-0.148***	-0.133***	-0.133**	-0.142***	-0.108***	-0.210***	-0.115**	-0.116***
	(0.045)	(0.043)	(0.059)	(0.036)	(0.037)	(0.056)	(0.054)	(0.035)
GovXSSM × A_*BailoutTax*	-0.114	-0.003	-0.256*	0.022	-0.066	-0.027	-0.198*	-0.110
	(0.094)	(0.095)	(0.135)	(0.077)	(0.079)	(0.125)	(0.109)	(0.075)
NonGovXSSM	-0.044	-0.035	0.001	-0.053	-0.040	-0.044	0.024	-0.017
	(0.051)	(0.049)	(0.068)	(0.041)	(0.043)	(0.061)	(0.058)	(0.039)
NonGovXSSM × A_*BailoutTax*	-0.147*	-0.006	-0.082	-0.070	-0.042	-0.156	-0.063	-0.098
	(0.082)	(0.080)	(0.111)	(0.066)	(0.069)	(0.101)	(0.093)	(0.064)
NonGovNonXSSM	0.005	-0.032	-0.037	-0.009	-0.024	0.002	-0.007	-0.021
	(0.034)	(0.034)	(0.047)	(0.028)	(0.029)	(0.044)	(0.041)	(0.027)
NonGovNonXSSM × A_*BailoutTax*	-0.098*	-0.056	-0.042	-0.085*	-0.110**	0.002	-0.116*	-0.084*
	(0.058)	(0.054)	(0.078)	(0.046)	(0.047)	(0.072)	(0.069)	(0.044)
A_*EconParty*	-0.135***	-0.285***	-0.278***	-0.192***	-0.252***	-0.126***	-0.308***	-0.253***
	(0.027)	(0.028)	(0.040)	(0.023)	(0.024)	(0.036)	(0.032)	(0.022)
A_*LCParty*	0.331***	0.205***	0.197***	0.287***	0.247***	0.303***	0.291***	0.270***
	(0.028)	(0.026)	(0.038)	(0.022)	(0.023)	(0.034)	(0.030)	(0.021)
A_*EUParty*	-0.583***	-0.430***	-0.488***	-0.506***	-0.491***	-0.528***	-0.588***	-0.540***
	(0.029)	(0.030)	(0.042)	(0.024)	(0.025)	(0.038)	(0.037)	(0.024)
A_*BadBankPers*	-0.208***	-0.119***	-0.146***	-0.166***	-0.173***	-0.133***	-0.154***	-0.155***
	(0.027)	(0.024)	(0.036)	(0.020)	(0.022)	(0.031)	(0.029)	(0.020)
A_*Recession*	-0.283***	-0.238***	-0.213***	-0.276***	-0.239***	-0.303***	-0.213***	-0.243***
	(0.024)	(0.026)	(0.036)	(0.020)	(0.021)	(0.032)	(0.030)	(0.020)
A_*PersIncomeFall*	-0.317***	-0.374***	-0.385***	-0.334***	-0.373***	-0.289***	-0.340***	-0.333***
	(0.028)	(0.026)	(0.040)	(0.022)	(0.023)	(0.034)	(0.033)	(0.022)
N	12,624	13,076	6,526	19,174	17,774	7,926	9,180	20,730
pseudo R^2	0.077	0.060	0.068	0.069	0.069	0.069	0.081	0.072

Dependent variable is *B3way*. Observations are restricted to participants who self-identify as non-male (1) male (2), are old (3), young (4), live in (5) rich Länder, (6) poor Länder, and self-identify as (7) high-class (8) low-class. Results of probit regressions when the SSM is fully responsible to bank supervision. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. The constant, task-count, and socio-demographic variables are suppressed for presentation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Does sole SSM authority provide blame avoidance opportunities to governments? Education, trust in institutions, Euroscepticism and economic values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GovXSSM	-0.069	-0.018	-0.046	-0.039	-0.109	-0.033	0.039	-0.096*
	(0.059)	(0.062)	(0.062)	(0.060)	(0.099)	(0.048)	(0.067)	(0.057)
A_*BailoutTax*	-0.169***	-0.105**	-0.216***	-0.064	-0.091	-0.153***	-0.169***	-0.115***
	(0.043)	(0.045)	(0.044)	(0.044)	(0.070)	(0.034)	(0.050)	(0.039)
GovXSSM × A_*BailoutTax*	-0.018	-0.076	0.018	-0.130	0.017	-0.070	-0.082	-0.051
	(0.094)	(0.096)	(0.094)	(0.096)	(0.143)	(0.076)	(0.109)	(0.085)
NonGovXSSM	-0.067	-0.006	-0.044	-0.041	0.056	-0.062	-0.137**	0.028
	(0.049)	(0.051)	(0.049)	(0.051)	(0.079)	(0.040)	(0.057)	(0.046)
NonGovXSSM × A_*BailoutTax*	-0.029	-0.132*	-0.027	-0.123	-0.121	-0.079	-0.026	-0.119
	(0.081)	(0.079)	(0.081)	(0.081)	(0.129)	(0.064)	(0.090)	(0.074)
NonGovNonXSSM	-0.030	0.004	-0.003	-0.029	0.066	-0.044	-0.020	-0.015
	(0.032)	(0.036)	(0.034)	(0.034)	(0.055)	(0.027)	(0.038)	(0.031)
NonGovNonXSSM × A_*BailoutTax*	-0.045	-0.111*	-0.015	-0.137**	-0.133	-0.060	-0.070	-0.081
	(0.054)	(0.058)	(0.057)	(0.055)	(0.088)	(0.044)	(0.063)	(0.051)
A_*EconParty*	-0.248***	-0.176***	-0.163***	-0.264***	-0.258***	-0.207***	-0.036	-0.335***
	(0.028)	(0.028)	(0.029)	(0.028)	(0.046)	(0.022)	(0.030)	(0.026)
A_*LCParty*	0.280***	0.250***	0.234***	0.297***	0.020	0.330***	0.424***	0.158***
	(0.026)	(0.027)	(0.028)	(0.026)	(0.044)	(0.021)	(0.029)	(0.024)
A_*EUParty*	-0.589***	-0.409***	-0.300***	-0.701***	-0.016	-0.628***	-0.663***	-0.395***
	(0.030)	(0.029)	(0.030)	(0.028)	(0.043)	(0.023)	(0.032)	(0.027)
A_*BadBankPers*	-0.155***	-0.171***	-0.150***	-0.174***	-0.134***	-0.171***	-0.164***	-0.161***
	(0.025)	(0.025)	(0.026)	(0.024)	(0.040)	(0.020)	(0.027)	(0.023)
A_*Recession*	-0.273***	-0.246***	-0.256***	-0.268***	-0.285***	-0.258***	-0.264***	-0.255***
	(0.025)	(0.025)	(0.024)	(0.026)	(0.037)	(0.020)	(0.029)	(0.023)
A_*PersIncomeFall*	-0.349***	-0.345***	-0.371***	-0.324***	-0.383***	-0.340***	-0.267***	-0.406***
	(0.027)	(0.027)	(0.028)	(0.026)	(0.046)	(0.021)	(0.028)	(0.026)
N	13,620	12,080	12,210	13,490	4,792	20,908	10,804	14,896
pseudo R^2	0.079	0.057	0.052	0.092	0.041	0.084	0.090	0.064

Observations are restricted to participants who self-identify as (1) university graduates, (2) non-university graduates, (3) NOT trusting institutions, (4) trusting institutions, (5) having anti-EU views, (6) having pro-EU views, (7) having left-wing economic views, (8) NOT having left-wing economic views. See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Does sole SSM authority provide blame avoidance opportunities to governments? Political values, exposure to banks, protected banks and Bafin knowledge

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GovXSSM	-0.011	-0.107	-0.080	-0.023	-0.018	-0.119	-0.061	-0.042
	(0.056)	(0.067)	(0.066)	(0.057)	(0.051)	(0.081)	(0.086)	(0.050)
A_*BailoutTax*	-0.147***	-0.137***	-0.139***	-0.140***	-0.174***	-0.072	-0.138**	-0.138***
	(0.041)	(0.047)	(0.050)	(0.039)	(0.038)	(0.055)	(0.064)	(0.035)
GovXSSM × A_*BailoutTax*	-0.068	-0.017	0.030	-0.087	0.002	-0.107	-0.066	-0.048
	(0.088)	(0.104)	(0.106)	(0.087)	(0.081)	(0.121)	(0.135)	(0.077)
NonGovXSSM	-0.086*	0.024	-0.020	-0.052	-0.041	-0.033	-0.102	-0.016
	(0.047)	(0.054)	(0.055)	(0.046)	(0.042)	(0.066)	(0.074)	(0.040)
NonGovXSSM × A_*BailoutTax*	-0.103	-0.035	-0.096	-0.062	-0.067	-0.086	0.088	-0.130**
	(0.074)	(0.090)	(0.089)	(0.075)	(0.070)	(0.101)	(0.115)	(0.066)
NonGovNonXSSM	-0.023	-0.002	-0.000	-0.026	-0.021	-0.008	-0.080	0.007
	(0.031)	(0.038)	(0.036)	(0.032)	(0.029)	(0.043)	(0.050)	(0.027)
NonGovNonXSSM × A_*BailoutTax*	-0.097*	-0.051	-0.130**	-0.042	-0.041	-0.154**	-0.061	-0.083*
	(0.051)	(0.063)	(0.064)	(0.051)	(0.049)	(0.068)	(0.082)	(0.045)
A_*EconParty*	-0.071***	-0.419***	-0.218***	-0.209***	-0.213***	-0.209***	-0.262***	-0.197***
	(0.025)	(0.030)	(0.030)	(0.027)	(0.025)	(0.034)	(0.041)	(0.023)
A_*LCParty*	0.392***	0.088***	0.285***	0.253***	0.238***	0.314***	0.252***	0.269***
	(0.025)	(0.028)	(0.030)	(0.025)	(0.023)	(0.033)	(0.037)	(0.022)
A_*EUParty*	-0.643***	-0.315***	-0.562***	-0.459***	-0.470***	-0.558***	-0.572***	-0.478***
	(0.027)	(0.032)	(0.032)	(0.028)	(0.025)	(0.038)	(0.039)	(0.025)
A_*BadBankPers*	-0.165***	-0.155***	-0.188***	-0.147***	-0.168***	-0.147***	-0.181***	-0.157***
	(0.024)	(0.027)	(0.028)	(0.023)	(0.022)	(0.032)	(0.035)	(0.021)
A_*Recession*	-0.265***	-0.254***	-0.290***	-0.243***	-0.252***	-0.286***	-0.252***	-0.264***
	(0.024)	(0.028)	(0.029)	(0.023)	(0.022)	(0.032)	(0.038)	(0.020)
A_*PersIncomeFall*	-0.325***	-0.386***	-0.377***	-0.326***	-0.347***	-0.359***	-0.387***	-0.333***
	(0.024)	(0.031)	(0.031)	(0.024)	(0.023)	(0.034)	(0.038)	(0.022)
N	15,478	10,222	10,434	14,914	16,914	8,314	6,404	19,296
pseudo R^2	0.086	0.060	0.081	0.059	0.064	0.078	0.078	0.065

Observations are restricted to participants who self-identify as (1) having left-wing political views, (2) or not, (3) highly exposed to banks, (4) or not, (5) preferring to work with protected banks, (6) or not, (7) being aware that Bafin is independent from the government, (8) or not. See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Does the SSM provide blame avoidance opportunities for left-wing governments?

	(1)	(2)	(3)	(4)
LWGovSSM	0.062	0.037	-0.059	0.078
	(0.053)	(0.077)	(0.069)	(0.077)
A_*BailoutTax*	-0.056	-0.001	-0.084*	0.017
	(0.057)	(0.081)	(0.049)	(0.080)
LWGovSSM × A_*BailoutTax*	-0.164*	-0.107	-0.197*	-0.233*
	(0.087)	(0.127)	(0.108)	(0.127)
NonLWGovSSM	0.040	0.090	-0.023	0.172***
	(0.054)	(0.059)	(0.063)	(0.061)
NonLWGovSSM × A_*BailoutTax*	-0.122	-0.213**	-0.057	-0.209**
	(0.079)	(0.094)	(0.093)	(0.093)
NonLWGovNonSSM	0.046	0.074	0.014	0.165***
	(0.049)	(0.057)	(0.045)	(0.058)
NonLWGovNonSSM × A_*BailoutTax*	-0.103	-0.147	-0.089	-0.168*
	(0.073)	(0.091)	(0.062)	(0.089)
NonGovSSM	0.013	0.044	-0.029	0.118**
	(0.043)	(0.055)	(0.046)	(0.056)
NonGovSSM × A_*BailoutTax*	-0.172***	-0.227***	-0.132*	-0.244***
	(0.066)	(0.088)	(0.068)	(0.087)
NonGovNonSSM	0.033	0.065	-0.007	0.139**
	(0.041)	(0.053)	(0.037)	(0.055)
NonGovNonSSM × A_*BailoutTax*	-0.150**	-0.206**	-0.131**	-0.222***
	(0.064)	(0.086)	(0.055)	(0.085)
A_*EconParty*	-0.212***	-0.209***	-0.212***	-0.192***
	(0.025)	(0.021)	(0.025)	(0.021)
A_*LCParty*	0.264***	0.264***	0.265***	0.265***
	(0.019)	(0.019)	(0.019)	(0.019)
A_*EUParty*	-0.501***	-0.501***	-0.501***	-0.502***
	(0.021)	(0.021)	(0.021)	(0.021)
A_*BadBankPers*	-0.162***	-0.162***	-0.162***	-0.162***
	(0.018)	(0.018)	(0.018)	(0.018)
A_*Recession*	-0.259***	-0.259***	-0.259***	-0.259***
	(0.018)	(0.018)	(0.018)	(0.018)
A_*PersIncomeFall*	-0.346***	-0.346***	-0.346***	-0.346***
	(0.019)	(0.019)	(0.019)	(0.019)
N	25,700	25,700	25,700	25,700
pseudo R^2	0.067	0.067	0.068	0.068

Models focus on the general case of left-wing parties in government (Column 1), large left-wing parties in Government (2), left-wing parties in government under sole SSM authority (3), and extreme left-wing parties in government (4). See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Does the SSM provide blame avoidance opportunities for Eurosceptic governments?

	(1)	(2)	(3)	(4)
EUScGovSSM	0.010	0.006	-0.039	0.108
	(0.054)	(0.078)	(0.068)	(0.078)
ABailoutTax	-0.098*	-0.054	-0.143***	0.123
	(0.057)	(0.078)	(0.050)	(0.080)
EUScGovSSM × A_*BailoutTax*	-0.228**	-0.124	-0.222**	-0.522***
	(0.089)	(0.130)	(0.112)	(0.132)
NonEUScGovSSM	0.003	0.042	-0.064	0.141**
	(0.054)	(0.059)	(0.063)	(0.059)
NonEUScGovSSM × A_*BailoutTax*	-0.025	-0.145	0.050	-0.274***
	(0.077)	(0.090)	(0.090)	(0.091)
NonEUScGovNonSSM	-0.023	0.019	-0.014	0.141**
	(0.050)	(0.057)	(0.045)	(0.056)
NonEUScGovNonSSM × A_*BailoutTax*	-0.033	-0.081	0.007	-0.296***
	(0.071)	(0.087)	(0.064)	(0.089)
NonGovSSM	-0.030	-0.001	-0.047	0.099*
	(0.044)	(0.055)	(0.046)	(0.055)
NonGovSSM × A_*BailoutTax*	-0.130**	-0.174**	-0.073	-0.350***
	(0.066)	(0.084)	(0.068)	(0.087)
NonGovNonSSM	-0.009	0.020	-0.024	0.120**
	(0.042)	(0.054)	(0.037)	(0.054)
NonGovNonSSM × A_*BailoutTax*	-0.108*	-0.153*	-0.072	-0.329***
	(0.065)	(0.084)	(0.057)	(0.087)
A_*EconParty*	-0.212***	-0.212***	-0.212***	-0.212***
	(0.020)	(0.020)	(0.020)	(0.020)
A_*LCParty*	0.265***	0.264***	0.265***	0.265***
	(0.019)	(0.019)	(0.019)	(0.019)
A_*EUScParty*	-0.499***	-0.501***	-0.499***	-0.489***
	(0.026)	(0.022)	(0.026)	(0.022)
A_*BadBankPers*	-0.162***	-0.162***	-0.162***	-0.162***
	(0.018)	(0.018)	(0.018)	(0.018)
A_*Recession*	-0.260***	-0.260***	-0.260***	-0.261***
	(0.018)	(0.018)	(0.018)	(0.018)
A_*PersIncomeFall*	-0.346***	-0.346***	-0.346***	-0.347***
	(0.019)	(0.019)	(0.019)	(0.019)
N	25,700	25,700	25,700	25,700
pseudo R^2	0.068	0.067	0.068	0.068

Models focus on the general case of Eurosceptic parties in government (Column 1), large Eurosceptic parties in Government (2), Eurosceptic parties in government under sole SSM authority (3), and extreme Eurosceptic parties in government (4). See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Does the SSM increase public support for Non-government Eurosceptic parties?

	(1)
EUScNonGovSSM	-0.093*
	(0.051)
A_*BailoutTax*	-0.262***
	(0.035)
EUScNonGovSSM × A_*BailoutTax*	0.142**
	(0.068)
NonEUScNonGovSSM	0.004
	(0.035)
NonEUScNonGovSSM × A_*BailoutTax*	-0.027
	(0.056)
NonEUScNonGovNonSSM	-0.028
	(0.047)
NonEUScNonGovNonSSM × A_*BailoutTax*	0.151**
	(0.059)
GovSSM	0.005
	(0.036)
GovSSM × A_*BailoutTax*	0.067
	(0.053)
GovNonSSM	-0.015
	(0.032)
GovNonSSM × A_*BailoutTax*	0.142***
	(0.049)
A_*EconParty*	-0.213***
	(0.020)
A_*LCParty*	0.264***
	(0.019)
A_*EUScParty*	-0.505***
	(0.030)
A_*BadBankPers*	-0.162***
	(0.018)
A_*Recession*	-0.260***
	(0.018)
A_*PersIncomeFall*	-0.346***
	(0.019)
N	25,700
pseudo R^2	0.068

See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Testing the hypothesis without potentially unreliable responses

	(1)	(2)	(3)
GovXSSM	-0.035	-0.061	-0.031
	(0.045)	(0.046)	(0.045)
A_*BailoutTax*	-0.169***	-0.179***	-0.140***
	(0.033)	(0.033)	(0.032)
GovXSSM × A_*BailoutTax*	-0.044	-0.001	-0.047
	(0.071)	(0.071)	(0.069)
NonGovXSSM	-0.046	-0.068*	-0.040
	(0.037)	(0.037)	(0.037)
NonGovXSSM × A_*BailoutTax*	-0.061	-0.044	-0.080
	(0.060)	(0.061)	(0.060)
NonGovNonXSSM	-0.024	-0.030	-0.005
	(0.025)	(0.026)	(0.025)
NonGovNonXSSM × A_*BailoutTax*	-0.060	-0.042	-0.073*
	(0.042)	(0.042)	(0.041)
A_*EconParty*	-0.229***	-0.210***	-0.208***
	(0.021)	(0.021)	(0.021)
A_*LCParty*	0.281***	0.260***	0.280***
	(0.020)	(0.020)	(0.020)
A_*EUParty*	-0.505***	-0.512***	-0.501***
	(0.022)	(0.023)	(0.022)
A_*BadBankPers*	-0.178***	-0.176***	-0.155***
	(0.019)	(0.019)	(0.018)
A_*Recession*	-0.273***	-0.265***	-0.251***
	(0.019)	(0.019)	(0.019)
A_*PersIncomeFall*	-0.341***	-0.344***	-0.343***
	(0.020)	(0.020)	(0.020)
N	23,168	22,656	23,590
pseudo R^2	0.071	0.069	0.067

Observations are restricted to participants who (1) are not within the shortest decile of response duration, (2) did not fail both attention checks at the end of the experiment, (3) did not know what the experiment is about. See Table 3 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table 9, as a robustness check, we test our hypothesis under the scenario of SSM-only supervision, but exclude potentially unreliable responses to our survey: very short responses (Column 1), those who failed to recall profile attributed after taking the experiment (2), and those who perhaps realized what the experiment is about (3). Our results from Table 2 are confirmed.

Conclusion

This paper examined the political implications of delegating banking supervision authority to the EU, specifically focusing on how such delegation affects public support for national governments in the context of banking crises. Our research contributes to the evolving literature on blame avoidance strategies in multi-level governance

systems (Heinkelmann-Wild *et al.*, 2020; Mortensen, 2016; Schlipphak & Treib, 2017) by extending the analysis to the critical yet understudied domain of banking supervision.

Contrary to the prevailing focus in the EBU literature, which has primarily analyzed the delegation of supervisory powers as a solution to market failures and inter-state allocation conflicts (Epstein, 2017; Howarth & Quaglia, 2016; Spendzharova, 2013), our paper hypothesizes that this institutional arrangement can also serve as a strategic blame avoidance mechanism for national governments. However, our findings do not substantiate this hypothesis, revealing that the delegation of banking supervision to the EU level on average does not alter the dynamics of blame attribution in the event of banking crises.

The results of our conjoint survey experiment, conducted in Germany—a country with an independent federal banking supervisor and initial reluctance towards EBU—show that hypothetical taxpayer-funded bank bailouts result in a substantial decrease (as much as 18 percent) in public support for government parties regardless of whether the EU dominates bank supervision. The absence of a blame avoidance effect persists across various demographic and socioeconomic divisions, levels of banking exposure, and even knowledge of the independence of the national banking regulator (*Bafin*).

Interestingly, our findings reveal strong heterogeneous contrarian effects: For old people and for those who self-identify as high-class, the SSM turns into a blame-attribution mechanism, compounding rather than moderating the fall in support for the government. EU-level supervision also appears to potentially undermine public support for left-wing or Eurosceptic governments, particularly extreme ones. This aligns with previous research indicating that blame avoidance strategies are conditioned by party ideology (Sommer, 2020). Our interpretation is that supporters of left-wing governments pin high hopes in state autonomy against 'the forces of global capitalism'. For them, the EU may represent these forces, leading them to blame their government for (being complicit in) ceding authority to the EU. Of course, this only becomes an issue when there is actually a problem in banks. Likewise, supporters of Eurosceptic governments are upset when they are complicit in delegation to the EU, which is the focus of their outrage. This also explains why Eurosceptic opposition parties

gain under such circumstances. In this sense, the loss of public support for governments handling a crisis after delegating authority to the EU reflects lack of support for delegation itself.

As global governance has become politicized and contested, public opinion of IOs has become increasingly important. Our study helps explore patterns and consequences of public opinion toward IOs. Specifically, we analyze key patterns of public support for governments that delegate policies to the EU, and by implication for delegation itself, across geographical spaces and social groups. We also study the implications, in terms of public support for governments, of IO-related outcomes such as bank failures and the distribution of their costs between bank owners and taxpayers.

Our research also opens up new avenues for future inquiry. One promising direction is to explore how public opinion towards IOs evolves over time and across different contexts in response to blame-shifting strategies in supranational governance. Longitudinal studies could help track changes in the effectiveness of these mechanisms over time as public awareness of EU-level supervision evolves. Additionally, comparative analyses across different EU member states could provide insights into how varying national contexts and attitudes towards European integration influence the success of blame avoidance strategies in banking supervision. The heterogeneous effects that we uncover also inspire a rich set of follow-up questions. Such research can deepen our understanding of the broader impact of IOs on public trust and democratic accountability. Understanding these dynamics can help clarify the nexus of IOs and public opinion.

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Appendix A: Attribute tables

Table A1: Conjoint attributes and levels

Attribute	**Level**
Party in government?	1=Large party in opposition; 2=Small party in opposition; 3=New party; 4=Small party in government; 5=Large party in government.

Party characteristics:

Economic ideology	1=Very pro-free market and private property; 2=Moderately pro-market and private property; 3=Centrist; 4=Moderately anti-free market and private property; 5=Very anti-free market and private property.
Social values	1=Very Liberal; 2= Moderately Liberal; 3=Centrist; 4=Moderately Conservative; 5=Very Conservative.
European integration	1=Very anti-EU; 2=Moderately anti-EU; 3=Centrist; 4=Moderately pro-EU; 5=Very pro-EU.

Before each election:

Banks were supervised by:	1=The federal authority only; 2=Mostly the federal authority, but also the EU authority; 3=The federal authority and the EU authority in equal measure; 4=Mostly the EU authority, but also the federal authority; 5=The EU authority only.
The government rescued banks:	1=Fully paid by banks owners, not taxpayers; 2=Mostly paid by banks owners, but taxpayers also paid; 3=Evenly shared among banks owners and taxpayers; 4=Mostly paid by taxpayers, but banks owners also paid; 5=Fully paid by taxpayers, not by banks owners.
Your bank gave you:	1=All the credit you wanted at very favorable rates, and very preferential return on your deposits; 2=Most of the credit that you wanted at favorable rates, and preferential return on your deposits; 3=Part of the credit that you wanted at market rates, and market-level return on your deposits; 4=A small part of the credit that you wanted at unfavorable rates, and disadvantageous return on your deposits; 5=None of the credit that you wanted, and highly disadvantageous return on your deposits.
The German economy:	1=Grew very fast and unemployment fell significantly; 2= Grew fast and unemployment fell; 3=Grew slowly, and no new jobs were created; 4=Entered a recession, and some people lost their jobs; 5=Was in crisis, many people lost their jobs.
Your income:	1=Increased significantly; 2=Increased; 3=Did not change; 4=Fell slightly; 5=Fell significantly.

Table A2: Example of party profiles

	Party A, contesting election in 'Year X'	Party B, contesting election in 'Year Y'
Party in government?	Large party in government	Small party in opposition

Party characteristics:

Economic Ideology	Moderately anti-market and private property	Moderately pro-market and private property
Social values	Centrist	Very Conservative
European Union	Very pro-EU	Centrist

Before the election:

Banks were supervised by:	The federal authority only	The EU authority only
The government rescued banks:	Fully paid by banks owners, not taxpayers	Mostly paid by taxpayers, but banks owners also paid
Your bank gave you:	Most of the credit that you wanted at favorable rates, and preferential return on your deposits	Part of the credit that you wanted at market rates, and market-level return on your deposits
The German economy:	Was in crisis, many people lost their jobs	Grew fast and unemployment fell
Your income:	Fell slightly	Did not change

Note: In Tasks 1 headline reference to "contesting election in 'Year X/Y' is omitted

Table A3: Conjoint attributes and levels for Tasks 1 and 6

Attribute	**Level**
Party in government?	1=Large party in opposition; 2=Small party in opposition; 3=New party; 4=Small party in government; 5=Large party in government.

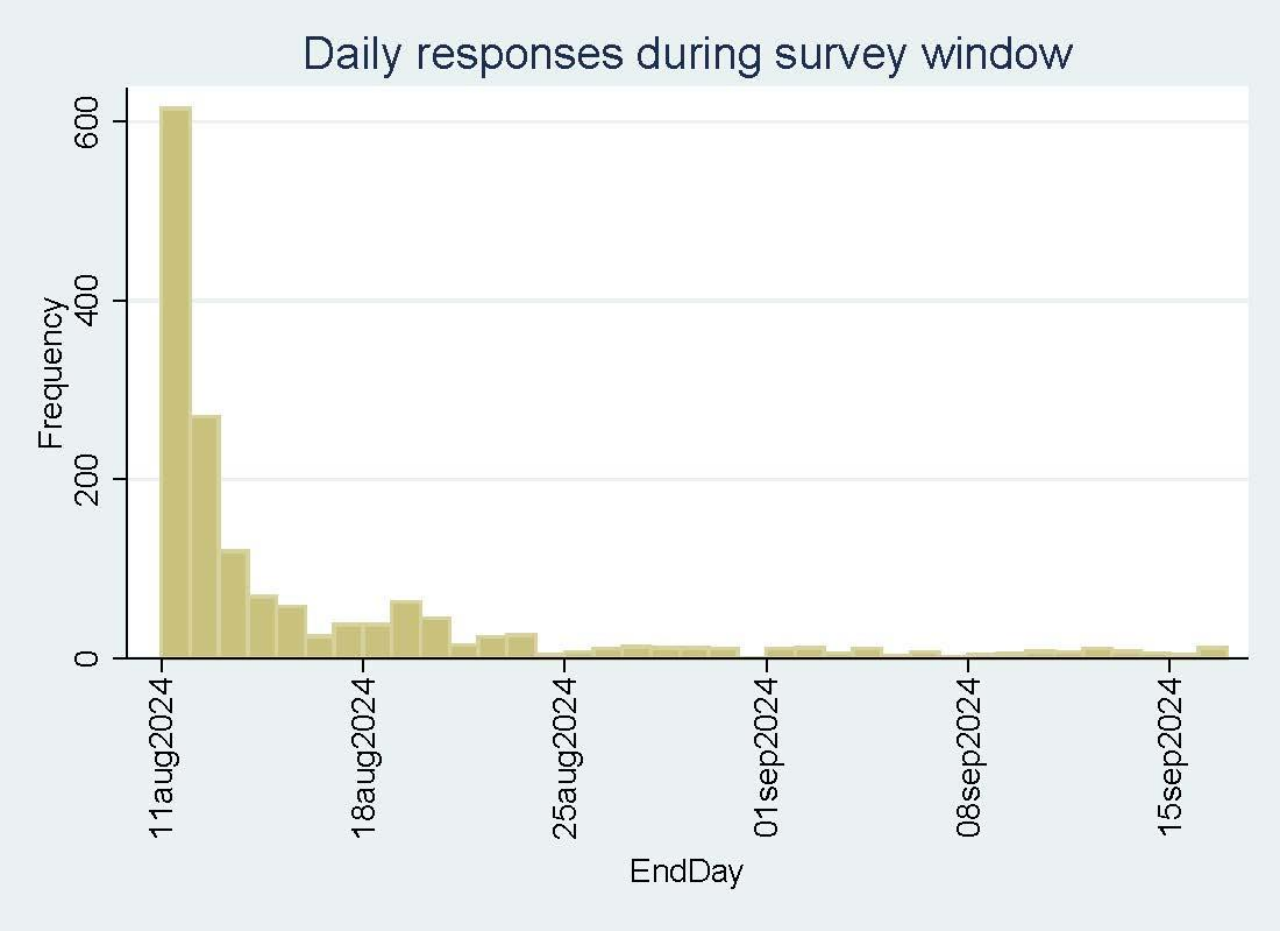
Party characteristics:

Economic ideology	1=Very pro-free market and private property; 2=Moderately pro-market and private property; 3=Centrist; 4=Moderately anti-free market and private property; 5=Very anti-free market and private property.
Social values	1=Very Liberal; 2= Moderately Liberal; 3=Centrist; 4=Moderately Conservative; 5=Very Conservative.
European integration	1=Very anti-EU; 2=Moderately anti-EU; 3=Centrist; 4=Moderately pro-EU; 5=Very pro-EU.
Right-Left ideology	1=Very Left; 2= Moderately Left; 3=Centrist; 4=Moderately Right; 5=Very Right.
Religion	1=Very religious; 2=Moderately religious; 3=Centrist; 4=Moderately secular; 5=Very secular.
Climate	1=Very anti climate policy; 2=Anti climate policy; 3=Centrist; 4=Supports climate policy; 5=Very supportive of climate policy.
Leader's gender	1=Male; 2=Female; 3=Other.
Leader's age	1=Very young; 2=Young; 3=Middle-age; 4=Old; 5=Very Old.

Appendix B: description and analysis of survey performance and quality

Timing

The sampling was done in two survey windows. We first ran a pre-test pilot experiment with 127 participants on 25 July 2024, from 9:39 AM to 12:31 PM CET (see report at OSF.IO/4AFQ8). We then ran the survey with an additional 1,597 participants from 11:34 AM CET on 11 August 2024, to 10:06 AM on 17 September 2024 (37 days), but as the histogram of daily responses below shows, the responses were heavily stacked on the survey's first few days. Unless otherwise specified, this reports will now aggregate both survey windows.



Participants were not pre-screened and the sample was not modeled to be representative, so any socio-demographic bias among participants is random. However, an equal number of men and women were sought.

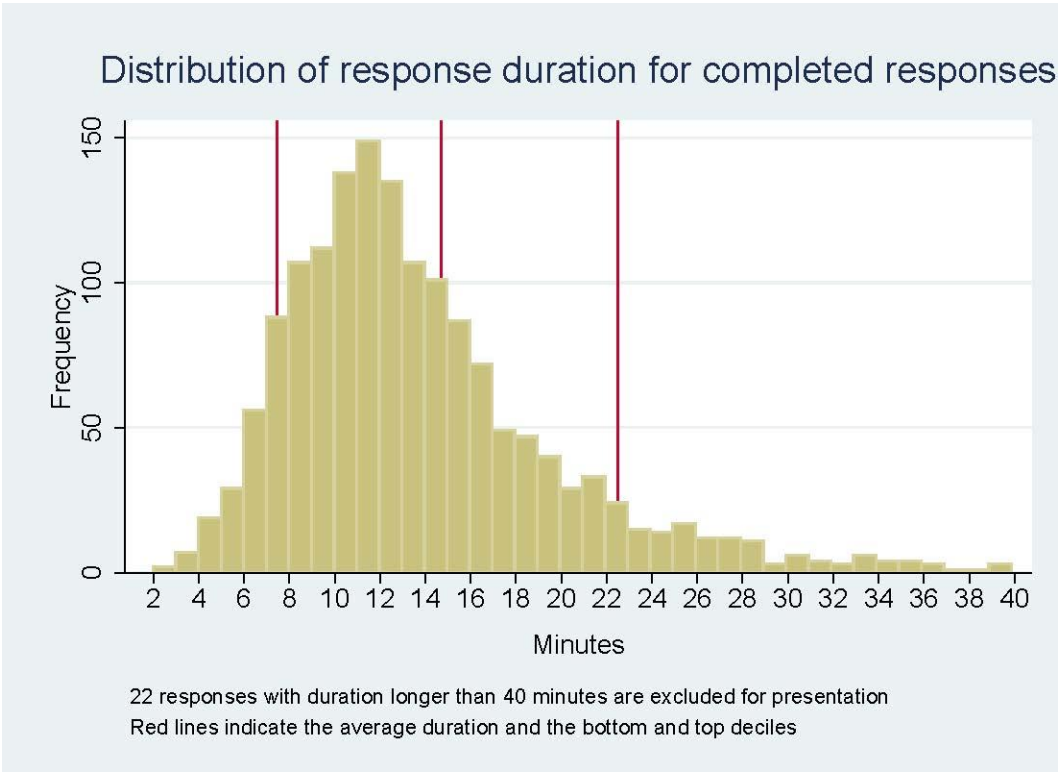
Of a total of 1,724 responses, 25 were incomplete, which means that have completed at least Task 2, but have not gone through to answer the manipulation check at the very end of the survey.

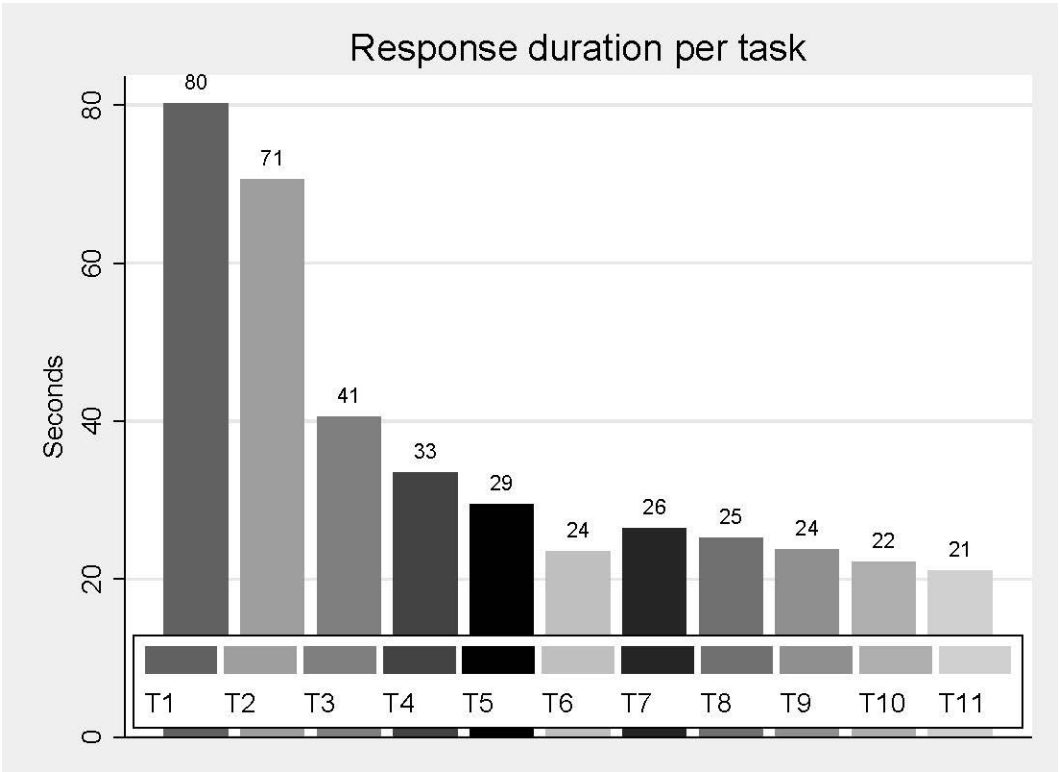
Cognitive load

We next check if the experiment burdened participants with excessive cognitive demands, and conclude that it did not. For this we analyze total response duration, task duration, consistency of choice, attention checks, and trends in choices over times.

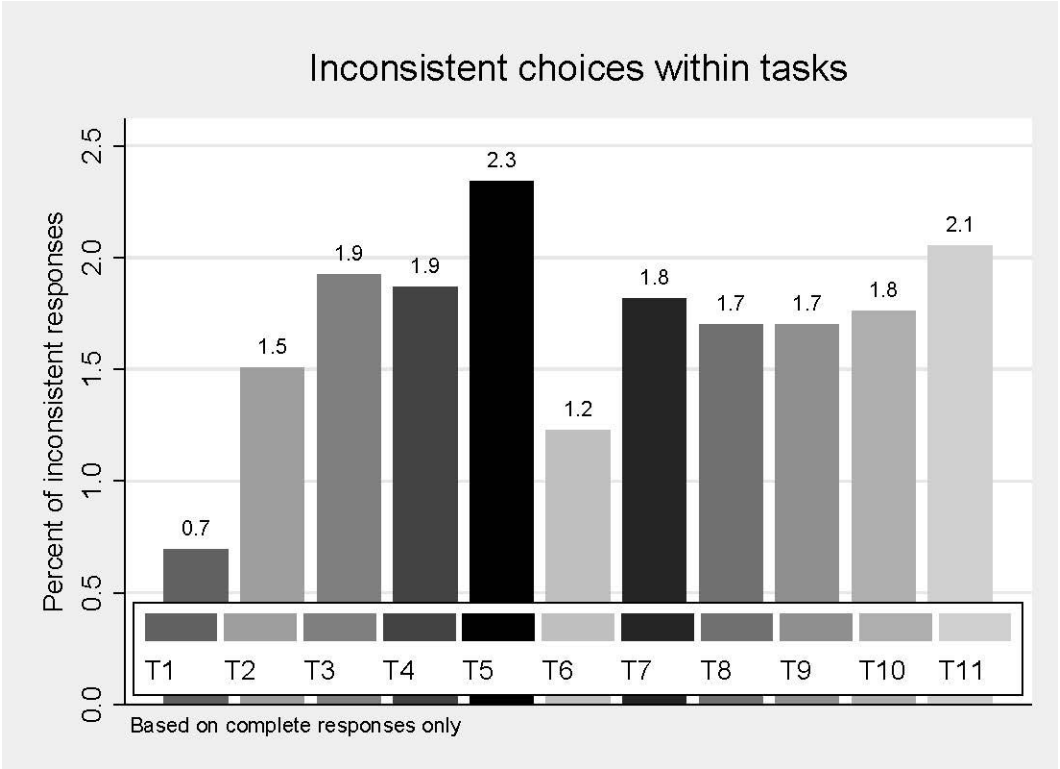
Response duration: Among completed answers, the median response time (the time it took participants to complete the entire survey experiment) was 12:30 minutes (750 seconds) and the average was 14:40 (880 seconds), compared with the 15 minutes that participants were informed to expect in the consent form. This suggest that overall the survey did not pose a particular challenge to participants in terms of complexity and length. However, 22 responses took more than 40 minutes to complete, 4:46 hours at most. At the other tale of the response duration distribution, the shortest response took 2:45 minutes, and 10 percent of responses took 7:28 minutes or less. This of course raises concerns about the extent of attention such short responses gave to the survey, which we address below.

Task duration: As the figure below demonstrates, among completed responses median values of response times per task diminish consistently, as participants get familiar with the particular structure of this experiment. Response time per task falls from 80 seconds in the first task, to 21 seconds in the 11th task. This seems like sufficient time dedicated by participants to understand the experiment early on, followed by a reasonable learning curve. A slight duration increase is registered in Task 7, when participants return to events-driven choice after no events in Task 6. We interpret it as further evidence of attention on the part of participants.


Distribution of response duration for completed responses
Frequency
0
50
100
150
2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32 34 36 38 40
Minutes
22 responses with duration longer than 40 minutes are excluded for presentation
Red lines indicate the average duration and the bottom and top deciles


Response duration per task
Seconds
0
20
40
60
80
80
71
41
33
29
24
26
25
24
22
21
T1 T2 T3 T4 T5 T6 T7 T8 T9 T10 T11

Consistency of choice: Among the 1,699 complete responses, we calculated that Clayton *et al*. (2023) swapping error (the rate of disagreement between the two-way choices in Tasks 2 and 7) is 0.2507, Clayton *et al*. suggest. The ICCI (the rate of disagreement between Tasks 1 and 6) is even better at 0.1913. This suggests that asking participants to choose among parties that are not directly competing in the same election is not too cognitively demanding for them. As for inconsistent choice within each task (choosing Party A in the three-way choice and then Party B in the two-way choice in a single task, or Party B first and Party A next), such instances become gradually more frequent from Task 1 to Task 5, then fall in Task 6 and slightly rise again in the following tasks (see figure below). Overall, however, this relates to no more than 2.3 percent of tasks in Task 5. When excluding Tasks 1, 6 and 7 and focusing on the eight tasks that are included in regression analysis, only 1.9 percent of these tasks among the 1,699 complete responses are inconsistent. We interpret this as evidence of some fatigue that builds up with tasks, but greater attention in Task 6, when the parties are again directly competing in elections.



Attention checks: The survey included four attention check questions, two before the experiment and two after the last task. Eight participants who failed both pre-experiment attention checks (the one on sources of information after the questions on the EU, and the fourth line in the block on trust in institutions) were immediately rejected and did not participate in the experiment. Post-experiment we asked participants if they can (1) recall party attributes from the tasks they had just completed and (2) recall events from these tasks. Participants are considered to have failed these attention checks if the selected any attributes/events that were not included in any task, or selected fewer than three of the four correct attributes, or two of the three correct event. Rates of failure in attention checks are much lower than the chance of failure in random guessing:

1st pre-experiment attention check (sources of information on politics): 34 percent failed compared with 98 percent in random guessing (1-(1/7)×(1/6)).

2nd pre-experiment attention check (trust in institutions): 1 percent failed compared with 83 percent in random guessing (1-(1/6)).

1st post-experiment attention check (party attributes in the conjoint): 32 percent of complete responses failed compared with 60 percent in random guessing (1-0.8×0.75×0.66).

2nd post-experiment attention check (events in the conjoint): 19 percent of complete responses failed compared with 70 percent in random guessing (1-0.6×0.5).

Indeed, failure rates are not any worse in the post-experiment checks than in the first attention check, which suggests that the experiment is not too cognitively demanding (statistics of the pre-experiment checks include the above eight participants).

Trends in choices over times: We next tested for a possible trend in participants' choices during the experiment. For this, we reshaped the data such that each line (observation) represents a single party in a single task done by a single participant, but excluded Tasks 1, 6 and 7. We then ran OLS and probit regression analyses of the three-way and two-way binary choices (standard errors clustered on participants in all regressions). As independent

variables we specified a task count (*Task*), the nine attribute dummies, and the socio-demographic controls. The coefficient of *Task* was insignificant for the two-way binary choice, but significant in the three-way choice, implying that participants were up to 1.5 percent likelier to select the "Don't know" option with every new task (see Table B1).

We next interacted *Task* with each of the nine attributes, generating 72 (9×8) interaction-coefficients in each regression (one per value of *Task*). In the OLS regressions for the three-way choice, only four of these interaction-coefficients are statistically significant at $p<0.10$; Seven are identified for the two-way choice (see Table B2). Similar counts apply to the probit regressions. Furthermore, these statistically significant interaction-coefficients are spread-out across different attributes and tasks, with no clear pattern emerging. All of this suggests that while occasionally fatigue may affect choices in our survey, they are small in magnitude and non-systematic.

Manipulation

Of the completed responses, only less than 8 percent were able to correctly identify what this study is about, in that they chose all three options "Parties in Government", "The European Union" and "Banking policies" and did not select any of the other four options in the manipulation check question (the very last question in the survey). Thus, we are confident that the vast majority of participants did not manipulate their choices in ways that affect our results.

Table B1: Basic regressions

	(1)	(2)	(3)	(4)
	B3way	*B2way*	*B3way*	*B2way*
A_*GovParty*	0.013**	0.008	0.036**	0.022
	(0.006)	(0.006)	(0.017)	(0.016)
A_*EconParty*	-0.073***	-0.067***	-0.212***	-0.178***
	(0.007)	(0.007)	(0.020)	(0.018)
A_*LCParty*	0.091***	0.095***	0.264***	0.253***
	(0.007)	(0.007)	(0.019)	(0.018)
A_*EUParty*	-0.170***	-0.176***	-0.501***	-0.465***
	(0.007)	(0.007)	(0.021)	(0.019)
A_*EUSuper*	-0.007	-0.006	-0.021	-0.015
	(0.006)	(0.006)	(0.017)	(0.017)
A_*BailoutTax*	-0.064***	-0.064***	-0.189***	-0.169***
	(0.006)	(0.006)	(0.018)	(0.017)
A_*BadBankPers*	-0.055***	-0.061***	-0.162***	-0.162***
	(0.006)	(0.006)	(0.018)	(0.017)
A_*Recession*	-0.088***	-0.101***	-0.260***	-0.269***
	(0.006)	(0.006)	(0.018)	(0.017)
A_*PersIncomeFall*	-0.117***	-0.133***	-0.346***	-0.354***
	(0.006)	(0.007)	(0.019)	(0.018)
Task	-0.005***	0.000	-0.015***	0.001
	(0.001)	(0.000)	(0.002)	(0.001)
NonMale	-0.034***	0.001	-0.100***	0.002
	(0.008)	(0.002)	(0.023)	(0.005)
Age	-0.000	0.000	-0.001	0.000
	(0.000)	(0.000)	(0.001)	(0.000)
Education	-0.009***	-0.000	-0.027***	-0.000
	(0.003)	(0.001)	(0.009)	(0.002)
Class	-0.008	0.000	-0.023	0.001
	(0.005)	(0.001)	(0.014)	(0.003)
EconView	0.001	0.001	0.003	0.003
	(0.004)	(0.001)	(0.011)	(0.003)
LeftRight	0.020***	0.000	0.059***	-0.000
	(0.005)	(0.001)	(0.015)	(0.003)
EUimage	0.004	-0.001	0.012	-0.003
	(0.006)	(0.001)	(0.017)	(0.004)
EUmembership	-0.000	0.000	0.001	-0.000
	(0.009)	(0.002)	(0.026)	(0.006)
TrustInInstitutions	-0.000	-0.001	-0.004	-0.002
	(0.007)	(0.002)	(0.021)	(0.004)
_cons	0.611***	0.698***	0.361***	0.527***
	(0.048)	(0.014)	(0.140)	(0.037)
N	25,700	25,700	25,700	25,700
adj. R^2	0.082	0.076		
pseudo R^2			0.067	0.058

Results of OLS regressions in Columns 1-2, probit regressions in Columns 3-4. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. Other variables are based on pre-experiment survey. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Table B2: Basic regressions with task-count interactions

	(1)	(2)	(3)	(4)
	B3way	*B2way*	*B3way*	*B2way*
3.Task	0.002	-0.029	-0.005	-0.083
	(0.027)	(0.028)	(0.079)	(0.077)
4.Task	-0.010	-0.002	-0.025	-0.003
	(0.028)	(0.028)	(0.081)	(0.077)
5.Task	-0.008	-0.021	-0.023	-0.058
	(0.028)	(0.028)	(0.080)	(0.077)
8.Task	-0.056**	-0.038	-0.152*	-0.105
	(0.028)	(0.028)	(0.081)	(0.077)
9.Task	-0.047	-0.017	-0.120	-0.048
	(0.029)	(0.029)	(0.085)	(0.079)
10.Task	-0.008	0.000	-0.007	-0.003
	(0.028)	(0.028)	(0.081)	(0.077)
11.Task	-0.078***	-0.033	-0.200**	-0.090
	(0.028)	(0.028)	(0.082)	(0.078)
A_*GovParty*	0.003	-0.014	0.011	-0.034
	(0.016)	(0.017)	(0.047)	(0.046)
3.Task × A_*GovParty*	0.012	0.026	0.032	0.068
	(0.023)	(0.025)	(0.067)	(0.066)
4.Task × A_*GovParty*	0.012	0.003	0.033	0.003
	(0.024)	(0.024)	(0.068)	(0.065)
5.Task × A_*GovParty*	0.023	0.037	0.062	0.097
	(0.023)	(0.025)	(0.068)	(0.066)
8.Task × A_*GovParty*	-0.005	0.024	-0.019	0.059
	(0.023)	(0.024)	(0.068)	(0.065)
9.Task × A_*GovParty*	0.028	0.062**	0.082	0.165**
	(0.023)	(0.025)	(0.068)	(0.065)
10.Task × A_*GovParty*	-0.009	0.006	-0.034	0.013
	(0.024)	(0.025)	(0.069)	(0.066)
11.Task × A_*GovParty*	0.015	0.013	0.042	0.032
	(0.023)	(0.024)	(0.067)	(0.064)
A_*EconParty*	-0.071***	-0.064***	-0.202***	-0.172***
	(0.017)	(0.017)	(0.049)	(0.046)
3.Task × A_*EconParty*	-0.018	-0.012	-0.051	-0.031
	(0.024)	(0.025)	(0.069)	(0.067)
4.Task × A_*EconParty*	-0.025	-0.039*	-0.075	-0.108*
	(0.023)	(0.024)	(0.069)	(0.064)
5.Task × A_*EconParty*	0.008	0.013	0.024	0.036
	(0.023)	(0.024)	(0.067)	(0.064)
8.Task × A_*EconParty*	0.027	0.020	0.075	0.053
	(0.023)	(0.025)	(0.068)	(0.066)
9.Task × A_*EconParty*	-0.014	-0.015	-0.050	-0.038
	(0.023)	(0.024)	(0.068)	(0.063)
10.Task × A_*EconParty*	-0.011	-0.002	-0.041	-0.005
	(0.022)	(0.024)	(0.066)	(0.064)
11.Task × A_*EconParty*	0.016	0.013	0.041	0.034
	(0.022)	(0.024)	(0.066)	(0.064)
A_*LCParty*	0.124***	0.106***	0.352***	0.283***
	(0.017)	(0.018)	(0.047)	(0.047)
3.Task × A_*LCParty*	-0.028	-0.016	-0.078	-0.044
	(0.023)	(0.024)	(0.064)	(0.064)
4.Task × A_*LCParty*	-0.030	-0.006	-0.085	-0.015
	(0.024)	(0.024)	(0.068)	(0.066)
5.Task × A_*LCParty*	-0.037	-0.001	-0.102	-0.002
	(0.024)	(0.024)	(0.069)	(0.066)

8.Task × A_*LCParty*	-0.035	-0.004	-0.091	-0.012
	(0.024)	(0.024)	(0.068)	(0.066)
9.Task × A_*LCParty*	-0.045**	-0.026	-0.119*	-0.069
	(0.023)	(0.024)	(0.066)	(0.064)
10.Task × A_*LCParty*	-0.061***	-0.024	-0.163**	-0.066
	(0.023)	(0.024)	(0.066)	(0.064)
11.Task × A_*LCParty*	-0.027	-0.009	-0.063	-0.022
	(0.023)	(0.024)	(0.067)	(0.065)
A_*EUParty*	-0.174***	-0.165***	-0.505***	-0.437***
	(0.016)	(0.017)	(0.048)	(0.045)
3.Task × A_*EUParty*	-0.006	-0.009	-0.008	-0.020
	(0.022)	(0.024)	(0.067)	(0.063)
4.Task × A_*EUParty*	-0.001	-0.016	-0.004	-0.043
	(0.022)	(0.023)	(0.067)	(0.063)
5.Task × A_*EUParty*	-0.005	-0.027	-0.012	-0.069
	(0.022)	(0.023)	(0.066)	(0.063)
8.Task × A_*EUParty*	-0.003	-0.021	-0.026	-0.054
	(0.022)	(0.024)	(0.068)	(0.063)
9.Task × A_*EUParty*	0.032	0.014	0.080	0.038
	(0.022)	(0.023)	(0.069)	(0.062)
10.Task × A_*EUParty*	-0.011	-0.027	-0.051	-0.065
	(0.022)	(0.023)	(0.067)	(0.063)
11.Task × A_*EUParty*	0.024	-0.005	0.045	-0.012
	(0.022)	(0.023)	(0.067)	(0.063)
A_*EUSuper*	-0.020	-0.026	-0.060	-0.069
	(0.016)	(0.017)	(0.048)	(0.047)
3.Task × A_*EUSuper*	0.015	0.026	0.044	0.070
	(0.023)	(0.024)	(0.066)	(0.065)
4.Task × A_*EUSuper*	0.010	0.038	0.030	0.101
	(0.023)	(0.024)	(0.068)	(0.065)
5.Task × A_*EUSuper*	-0.007	0.003	-0.015	0.008
	(0.023)	(0.025)	(0.068)	(0.066)
8.Task × A_*EUSuper*	0.030	0.038	0.091	0.103
	(0.023)	(0.024)	(0.067)	(0.065)
9.Task × A_*EUSuper*	0.019	-0.003	0.055	-0.005
	(0.023)	(0.025)	(0.068)	(0.065)
10.Task × A_*EUSuper*	0.011	0.023	0.039	0.062
	(0.023)	(0.025)	(0.068)	(0.066)
11.Task × A_*EUSuper*	0.027	0.040*	0.081	0.107*
	(0.023)	(0.024)	(0.067)	(0.065)
A_*BailoutTax*	-0.076***	-0.090***	-0.223***	-0.243***
	(0.016)	(0.017)	(0.048)	(0.046)
3.Task × A_*BailoutTax*	0.030	0.053**	0.094	0.144**
	(0.023)	(0.025)	(0.067)	(0.066)
4.Task × A_*BailoutTax*	0.015	0.018	0.048	0.049
	(0.023)	(0.024)	(0.068)	(0.066)
5.Task × A_*BailoutTax*	0.001	0.047*	0.004	0.126**
	(0.022)	(0.024)	(0.065)	(0.064)
8.Task × A_*BailoutTax*	0.019	0.023	0.057	0.066
	(0.023)	(0.024)	(0.068)	(0.066)
9.Task × A_*BailoutTax*	0.012	0.025	0.035	0.069
	(0.023)	(0.024)	(0.069)	(0.064)
10.Task × A_*BailoutTax*	0.010	0.020	0.028	0.054
	(0.022)	(0.024)	(0.067)	(0.065)
11.Task × A_*BailoutTax*	0.005	0.030	0.006	0.082
	(0.023)	(0.025)	(0.070)	(0.066)
A_*BadBankPers*	-0.083***	-0.062***	-0.242***	-0.165***
	(0.016)	(0.017)	(0.048)	(0.047)

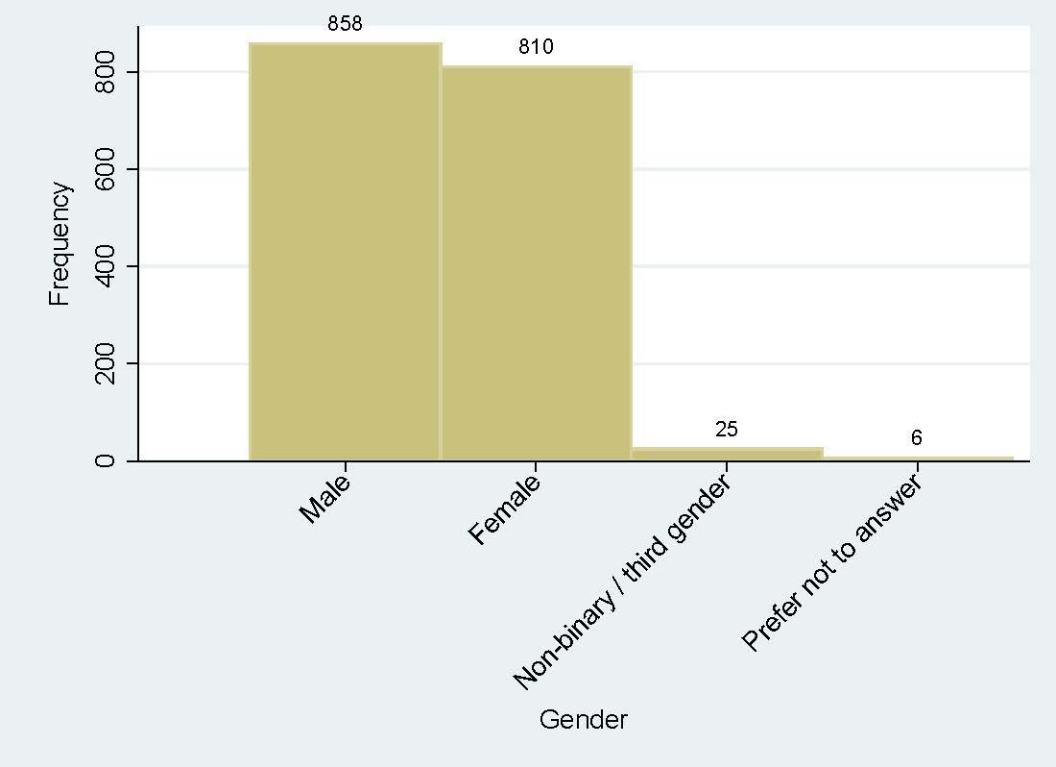
3.Task × A_*BadBankPers*	0.032	-0.007	0.096	-0.018
	(0.023)	(0.025)	(0.067)	(0.065)
4.Task × A_*BadBankPers*	0.026	-0.010	0.074	-0.028
	(0.023)	(0.024)	(0.068)	(0.065)
5.Task × A_*BadBankPers*	0.041*	0.001	0.121*	0.001
	(0.023)	(0.024)	(0.067)	(0.066)
8.Task × A_*BadBankPers*	0.035	0.033	0.103	0.090
	(0.023)	(0.024)	(0.068)	(0.064)
9.Task × A_*BadBankPers*	0.037	0.008	0.107	0.022
	(0.024)	(0.025)	(0.070)	(0.066)
10.Task × A_*BadBankPers*	0.019	-0.014	0.051	-0.036
	(0.023)	(0.024)	(0.068)	(0.065)
11.Task × A_*BadBankPers*	0.032	-0.002	0.086	-0.006
	(0.023)	(0.024)	(0.068)	(0.065)
A_*Recession*	-0.101***	-0.124***	-0.297***	-0.330***
	(0.016)	(0.017)	(0.049)	(0.046)
3.Task × A_*Recession*	-0.002	-0.002	0.000	-0.004
	(0.023)	(0.024)	(0.067)	(0.065)
4.Task × A_*Recession*	0.019	0.035	0.055	0.089
	(0.023)	(0.024)	(0.068)	(0.064)
5.Task × A_*Recession*	0.011	0.017	0.035	0.043
	(0.024)	(0.025)	(0.070)	(0.066)
8.Task × A_*Recession*	0.026	0.038	0.074	0.104
	(0.023)	(0.025)	(0.068)	(0.066)
9.Task × A_*Recession*	0.033	0.030	0.090	0.080
	(0.022)	(0.024)	(0.067)	(0.065)
10.Task × A_*Recession*	0.002	0.014	0.002	0.037
	(0.023)	(0.024)	(0.068)	(0.065)
11.Task × A_*Recession*	0.013	0.049**	0.031	0.129**
	(0.023)	(0.024)	(0.070)	(0.064)
A_*PersIncomeFall*	-0.100***	-0.114***	-0.286***	-0.304***
	(0.016)	(0.017)	(0.048)	(0.046)
3.Task × A_*vPersIncomeFall*	-0.021	0.013	-0.062	0.034
	(0.023)	(0.024)	(0.067)	(0.064)
4.Task × A_*PersIncomeFall*	-0.001	-0.011	-0.009	-0.032
	(0.023)	(0.024)	(0.068)	(0.064)
5.Task × A_*PersIncomeFall*	-0.020	-0.034	-0.066	-0.092
	(0.022)	(0.024)	(0.066)	(0.064)
8.Task × A_*PersIncomeFall*	-0.021	-0.044*	-0.077	-0.115*
	(0.022)	(0.024)	(0.066)	(0.064)
9.Task × A_*PersIncomeFall*	-0.057**	-0.038	-0.190***	-0.098
	(0.022)	(0.024)	(0.068)	(0.064)
10.Task × A_*PersIncomeFall*	-0.011	0.006	-0.047	0.017
	(0.022)	(0.024)	(0.067)	(0.063)
11.Task × A_*PersIncomeFall*	-0.004	-0.044*	-0.032	-0.115*
	(0.022)	(0.024)	(0.068)	(0.063)
NonMale	-0.034***	0.000	-0.102***	0.001
	(0.008)	(0.002)	(0.023)	(0.005)
Age	-0.000	0.000	-0.001	0.000
	(0.000)	(0.000)	(0.001)	(0.000)
Education	-0.009***	-0.000	-0.027***	-0.000
	(0.003)	(0.001)	(0.009)	(0.002)
Class	-0.008	0.001	-0.023	0.001
	(0.005)	(0.001)	(0.014)	(0.003)
EconView	0.001	0.001	0.003	0.002
	(0.004)	(0.001)	(0.011)	(0.003)
LeftRight	0.020***	0.000	0.060***	0.000
	(0.005)	(0.001)	(0.015)	(0.004)

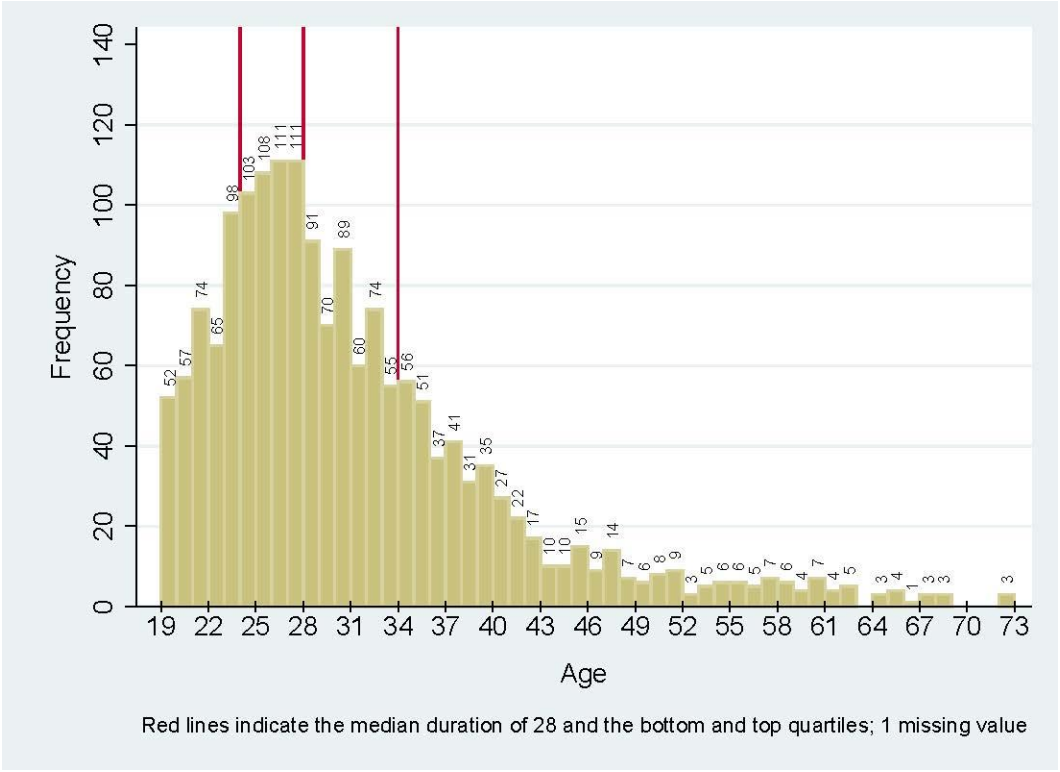
EUimage	0.004	-0.001	0.012	-0.004
	(0.006)	(0.001)	(0.017)	(0.004)
EUmembership	-0.001	-0.001	0.001	-0.002
	(0.009)	(0.002)	(0.026)	(0.007)
TrustInInstitutions	-0.000	-0.001	-0.004	-0.003
	(0.007)	(0.002)	(0.021)	(0.005)
_cons	0.603***	0.718***	0.330**	0.586***
	(0.052)	(0.024)	(0.151)	(0.066)
N	25,700	25,700	25,700	25,700
adj. R^2	0.081	0.076		
pseudo R^2			0.069	0.060

Results of OLS regressions in Columns 1-2, probit regressions in Columns 3-4. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. Other variables are based on pre-experiment survey. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

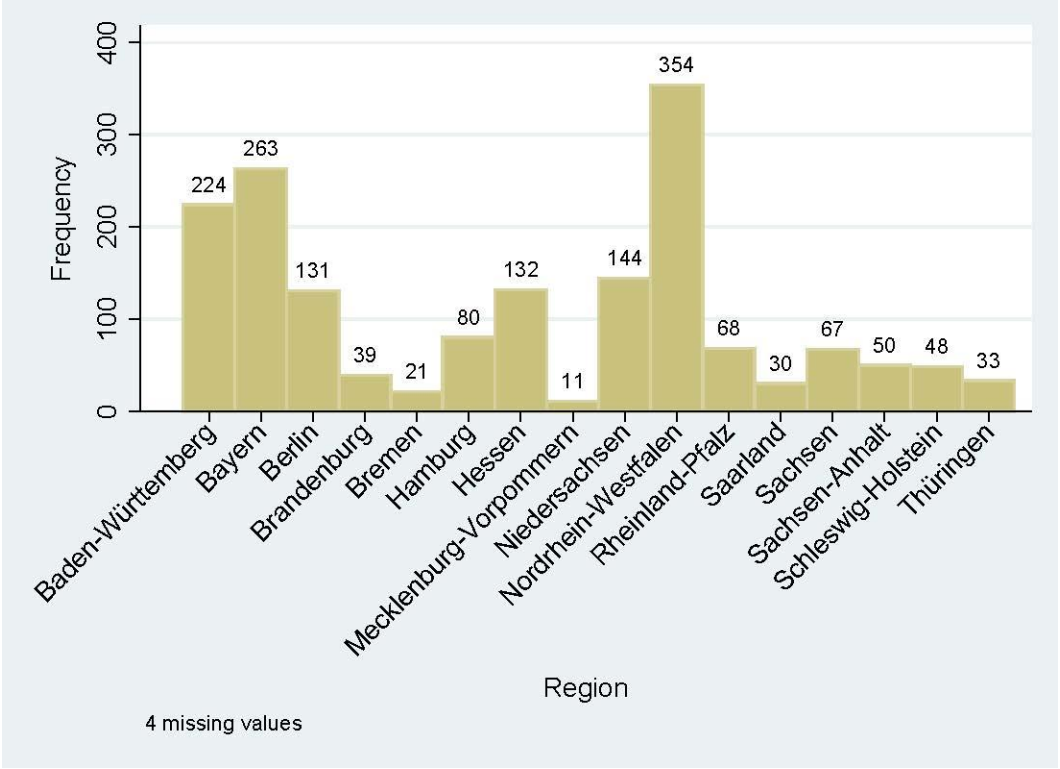
Socio-demographic profiling of the sample

The histograms below provide the distribution of completed responses to each of the questions on the pre-experiment survey.

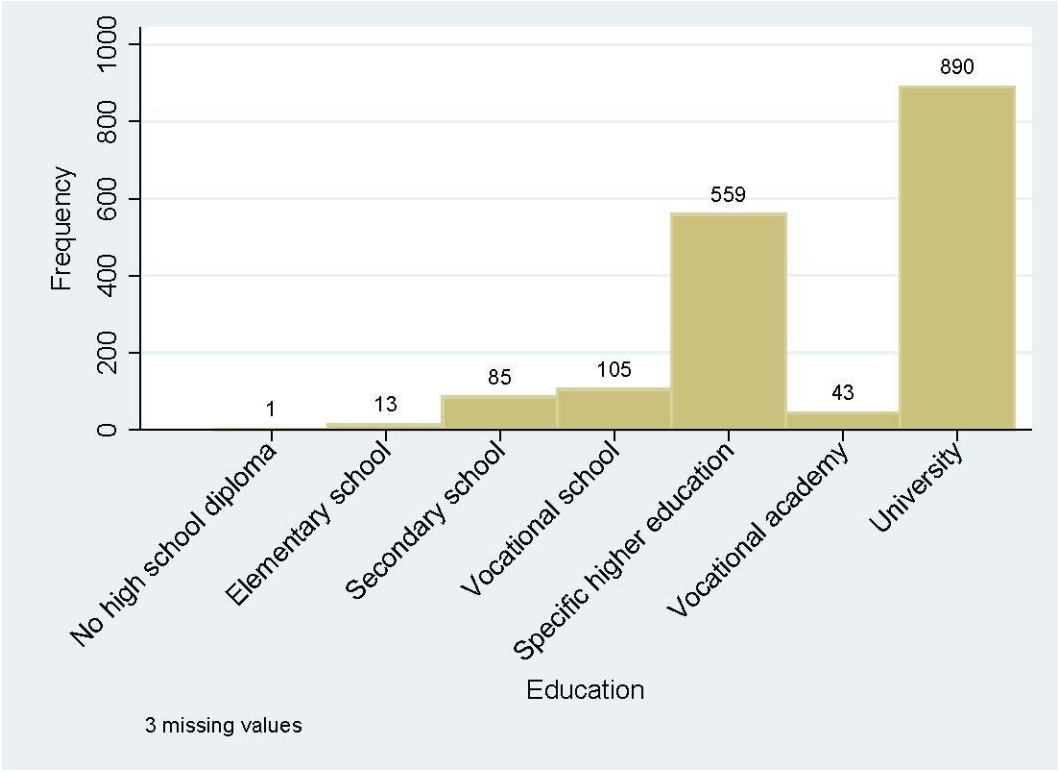


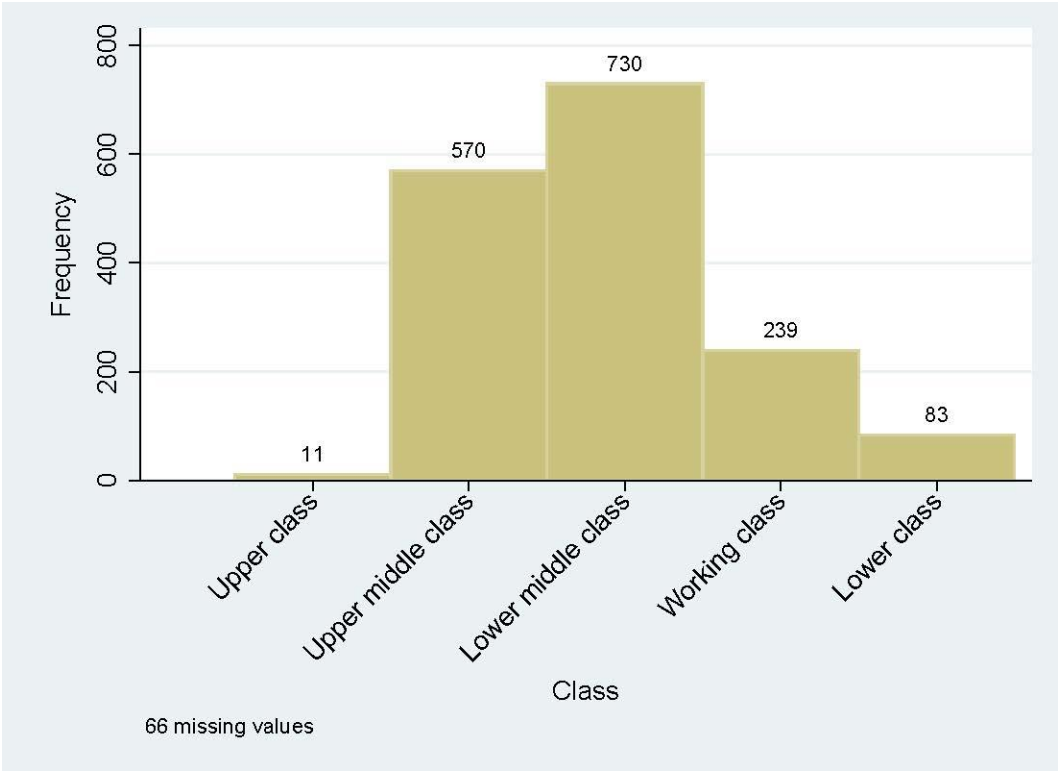


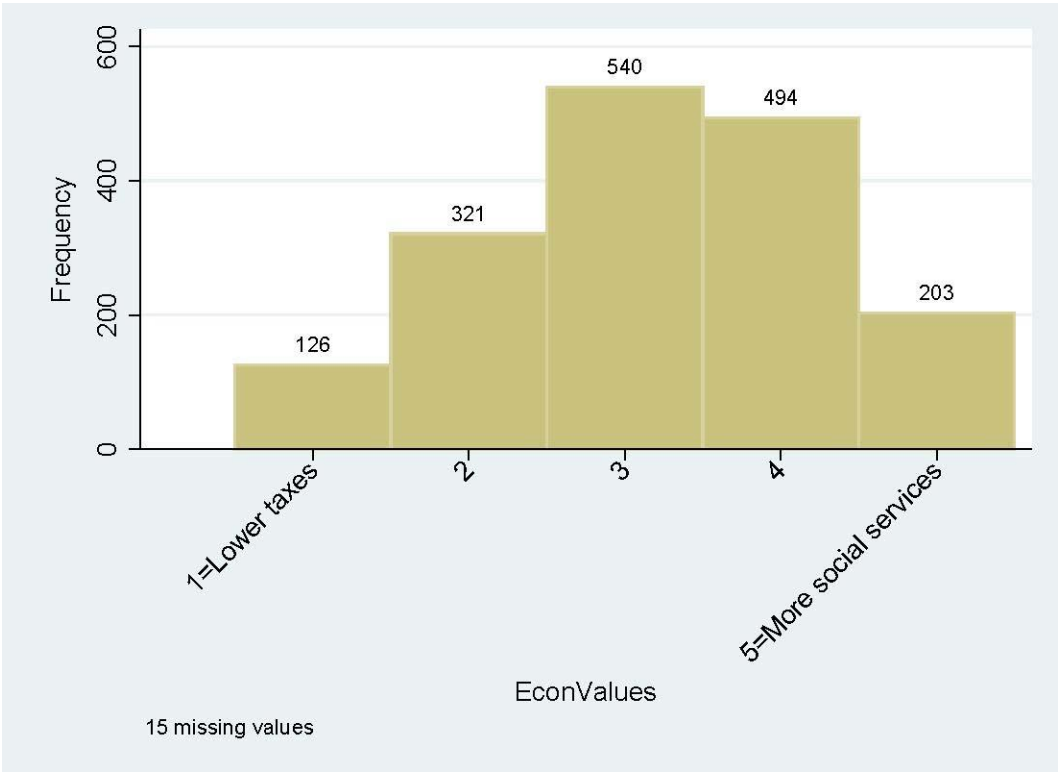
Red lines indicate the median duration of 28 and the bottom and top quartiles; 1 missing value

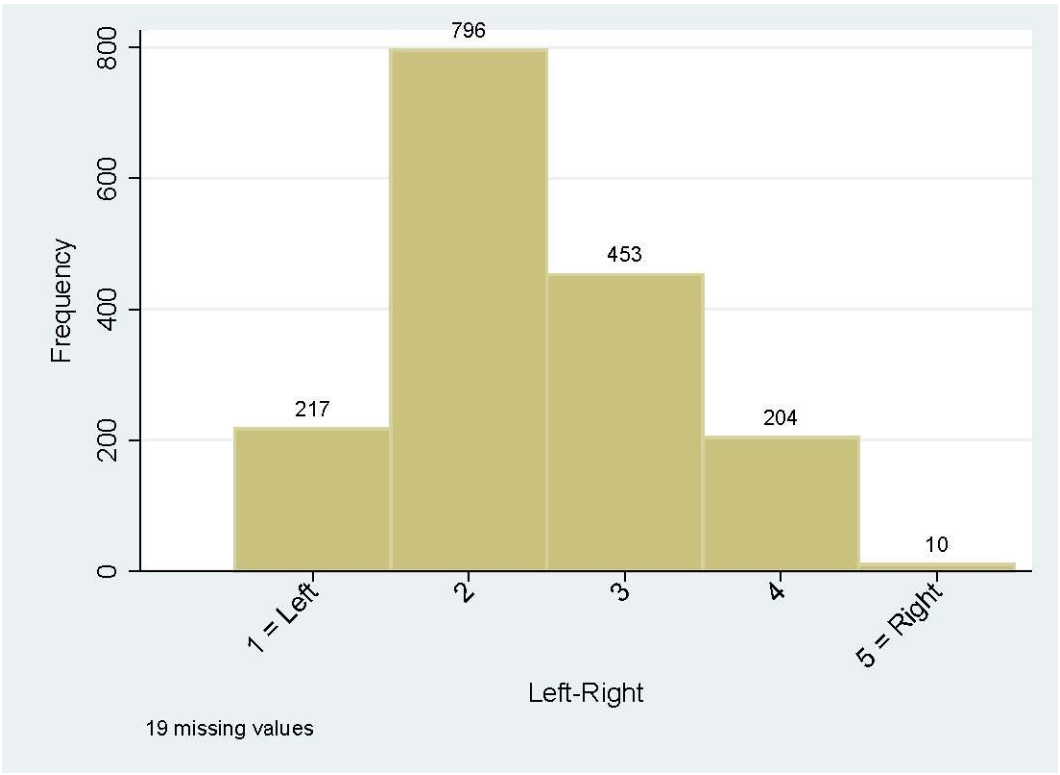


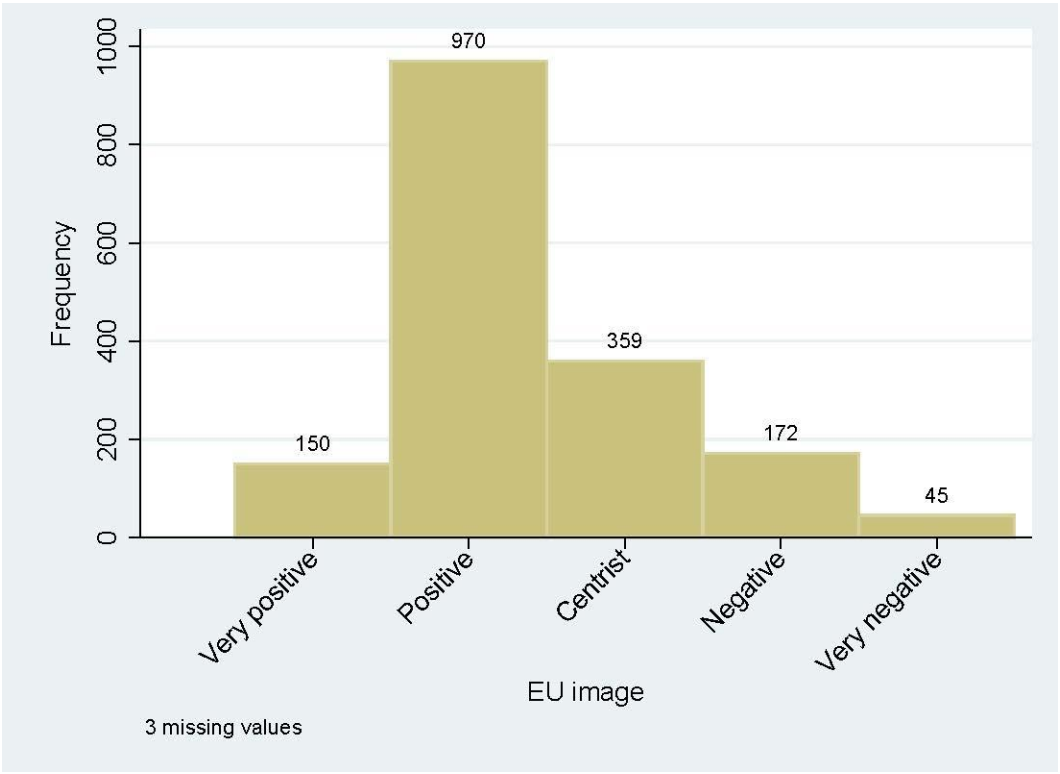
4 missing values

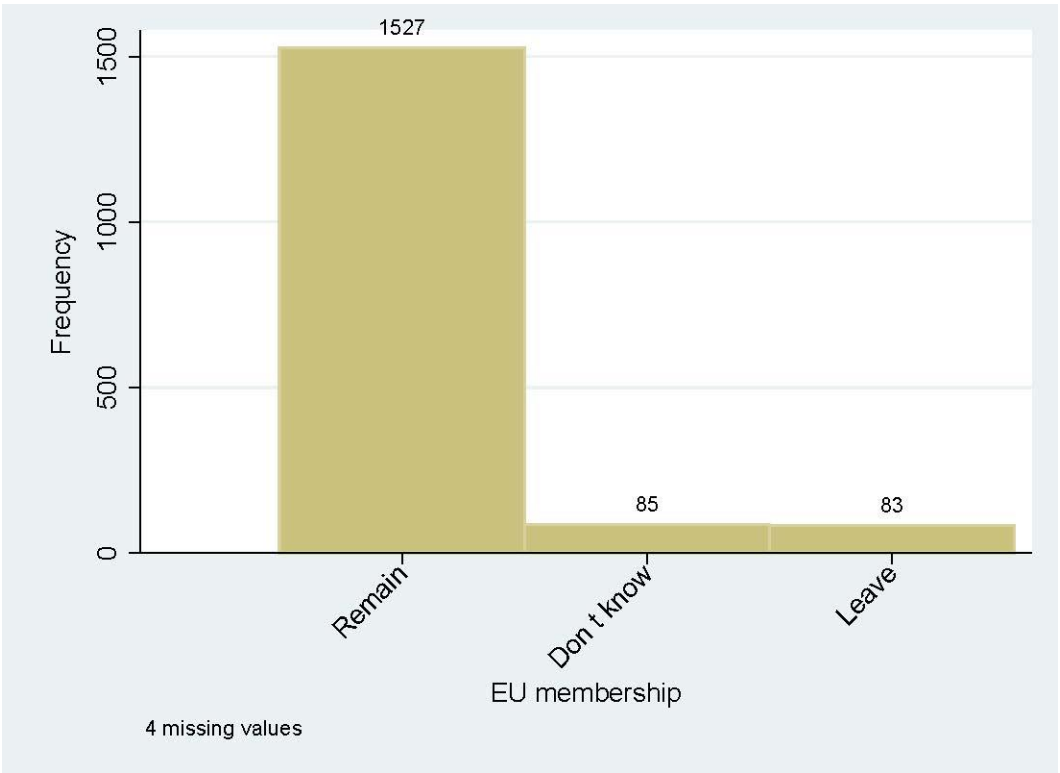

Frequency
0
200
400
600
800
1000
1
13
85
105
559
43
890
No high school diploma
Elementary school
Secondary school
Vocational school
Specific higher education
Vocational academy
University
Education
3 missing values

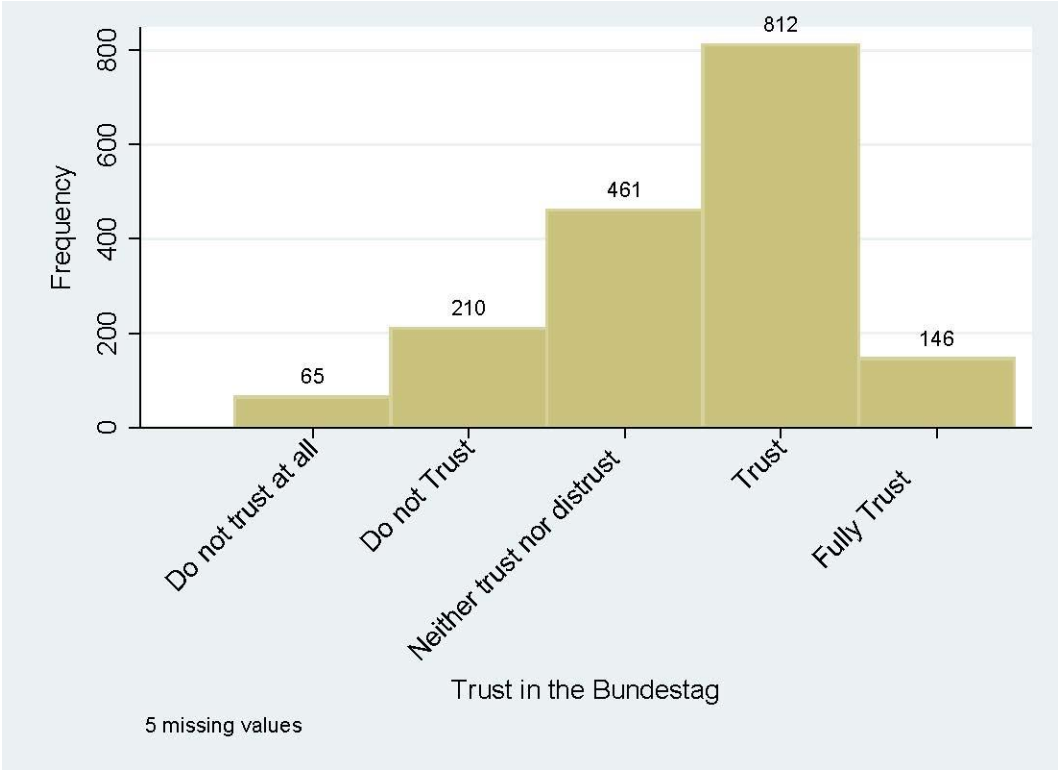

Frequency
0
200
400
600
800
11
570
730
239
83
Upper class
Upper middle class
Lower middle class
Working class
Lower class
Class
66 missing values

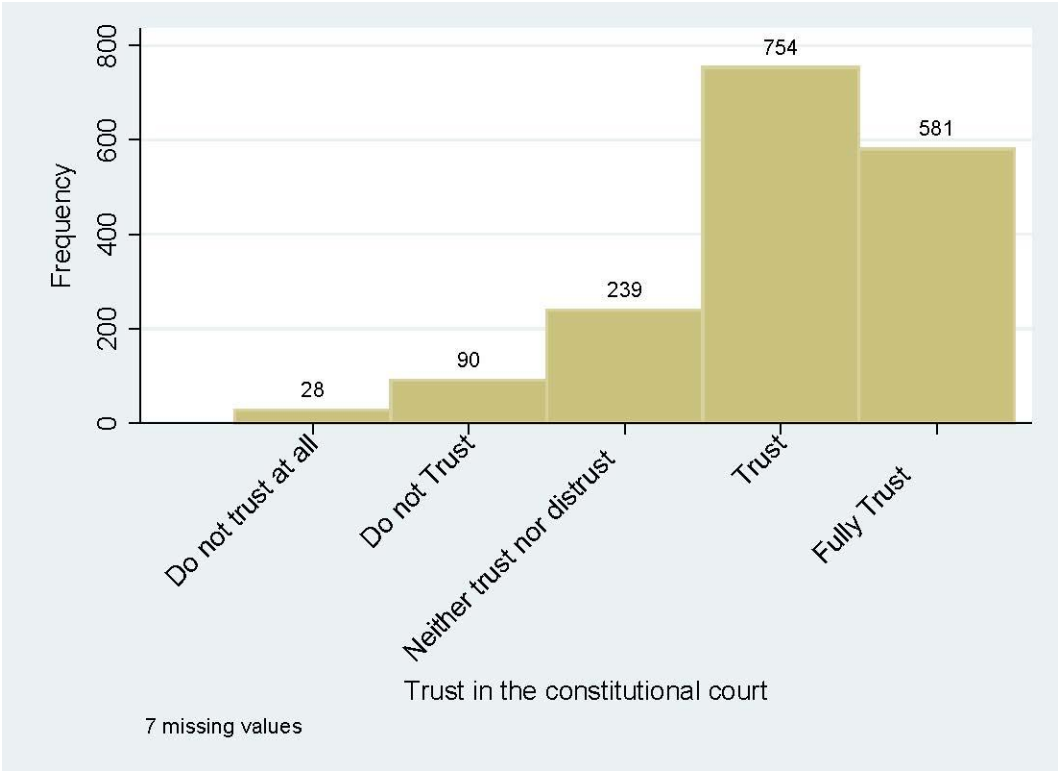

Frequency
0
200
400
600
126
321
540
494
203
1=Lower taxes
2
3
4
5=More social services
EconValues
15 missing values

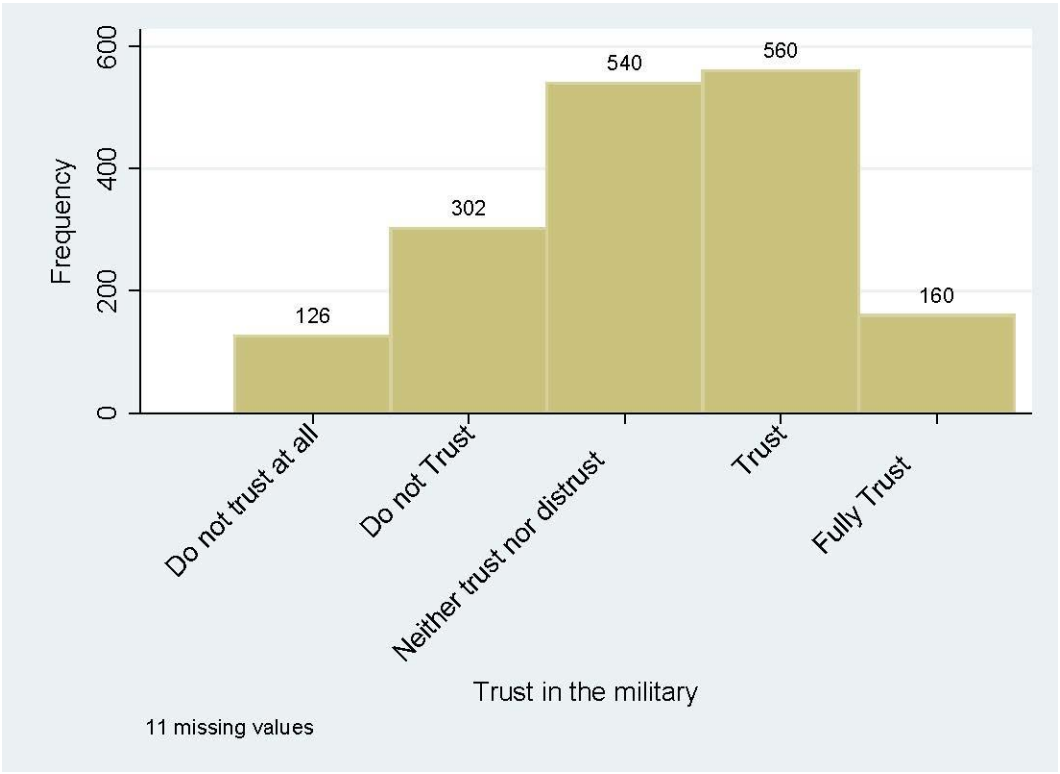

Frequency
0
200
400
600
800
217
796
453
204
10
1 = Left
2
3
4
5 = Right
Left-Right
19 missing values

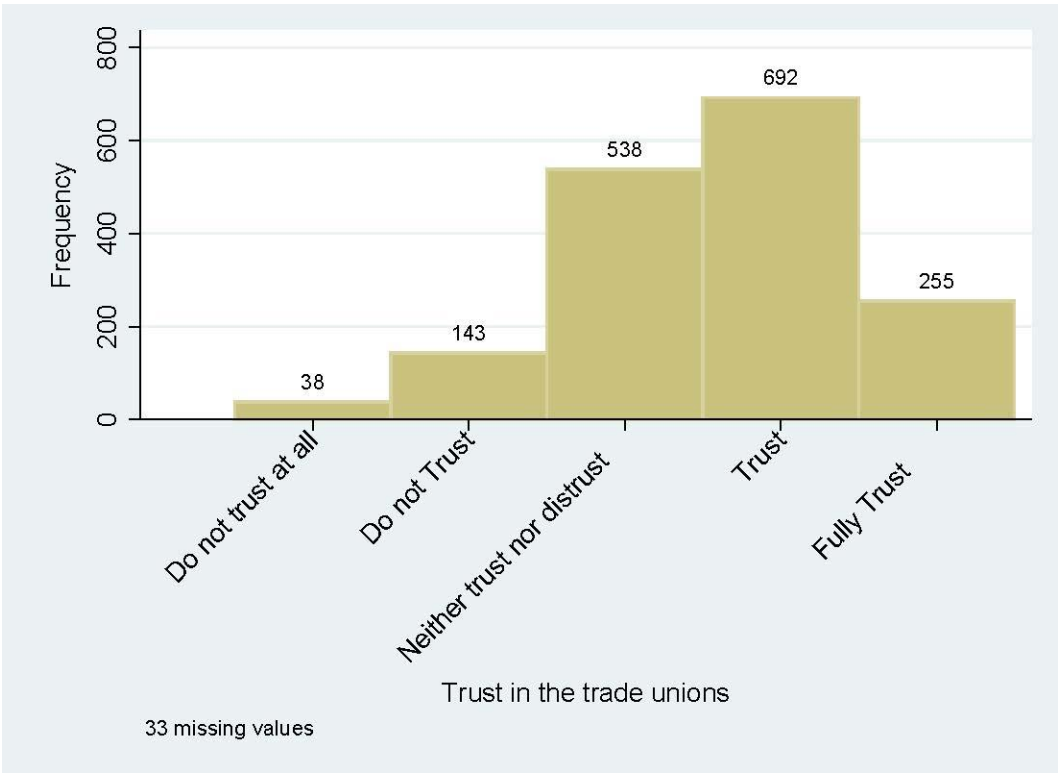

Frequency
0
200
400
600
800
1000
970
359
150
172
45
Very positive
Positive
Centrist
Negative
Very negative
EU image
3 missing values

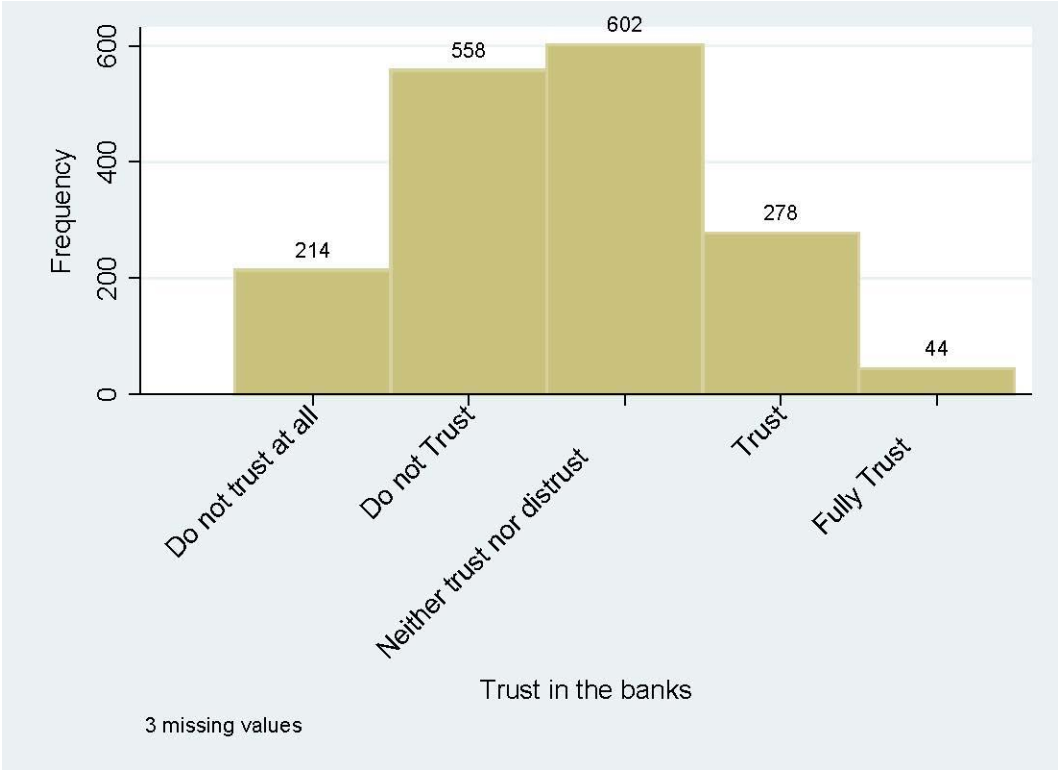

Frequency
0
500
1000
1500
1527
85
83
Remain
Don t know
Leave
EU membership
4 missing values

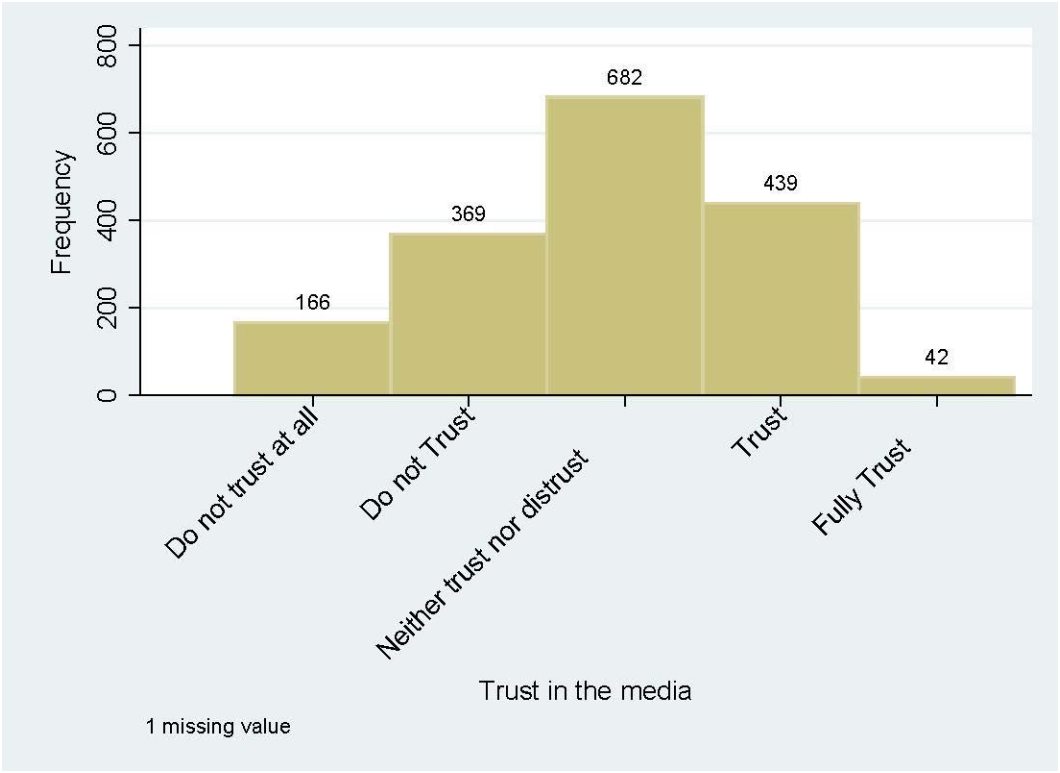

Frequency
0
200
400
600
800
65
210
461
812
146
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the Bundestag
5 missing values

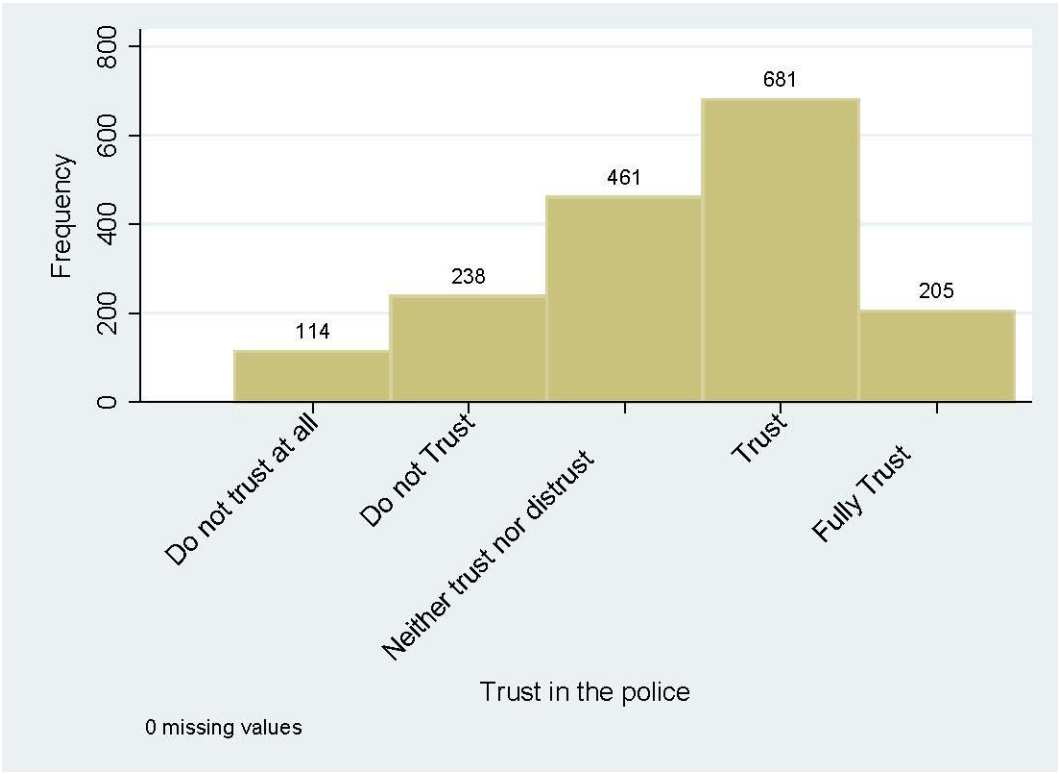

Frequency
0
200
400
600
800
28
90
239
754
581
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the constitutional court
7 missing values

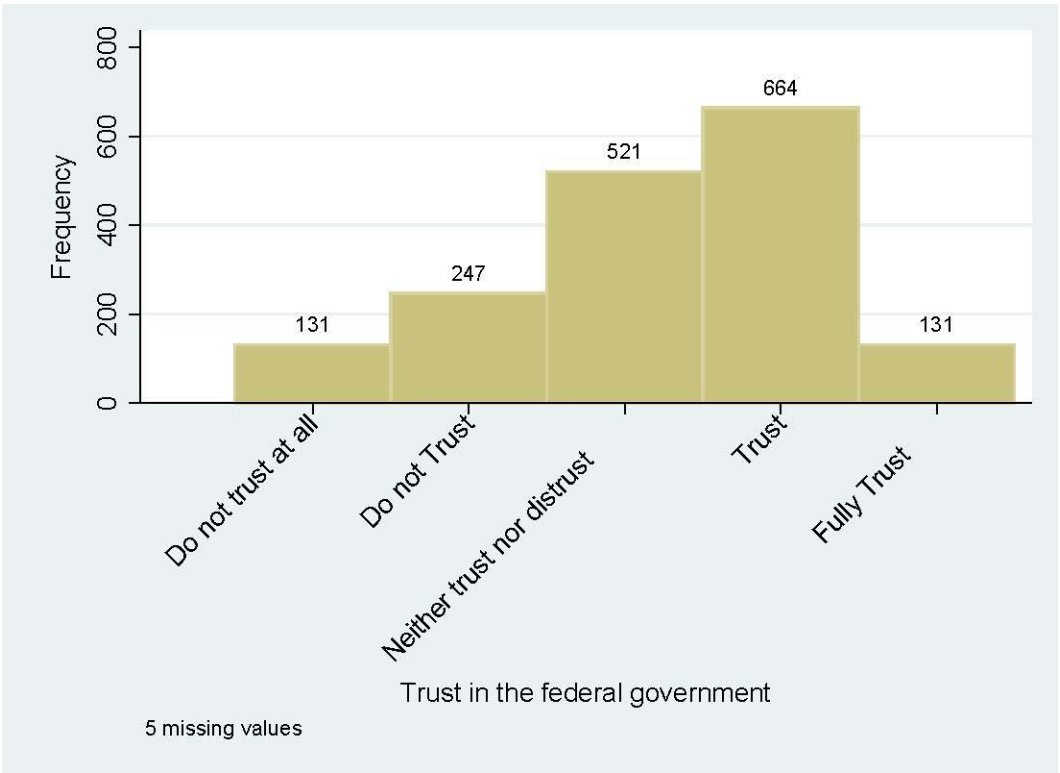

Frequency
0
200
400
600
126
302
540
560
160
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the military
11 missing values

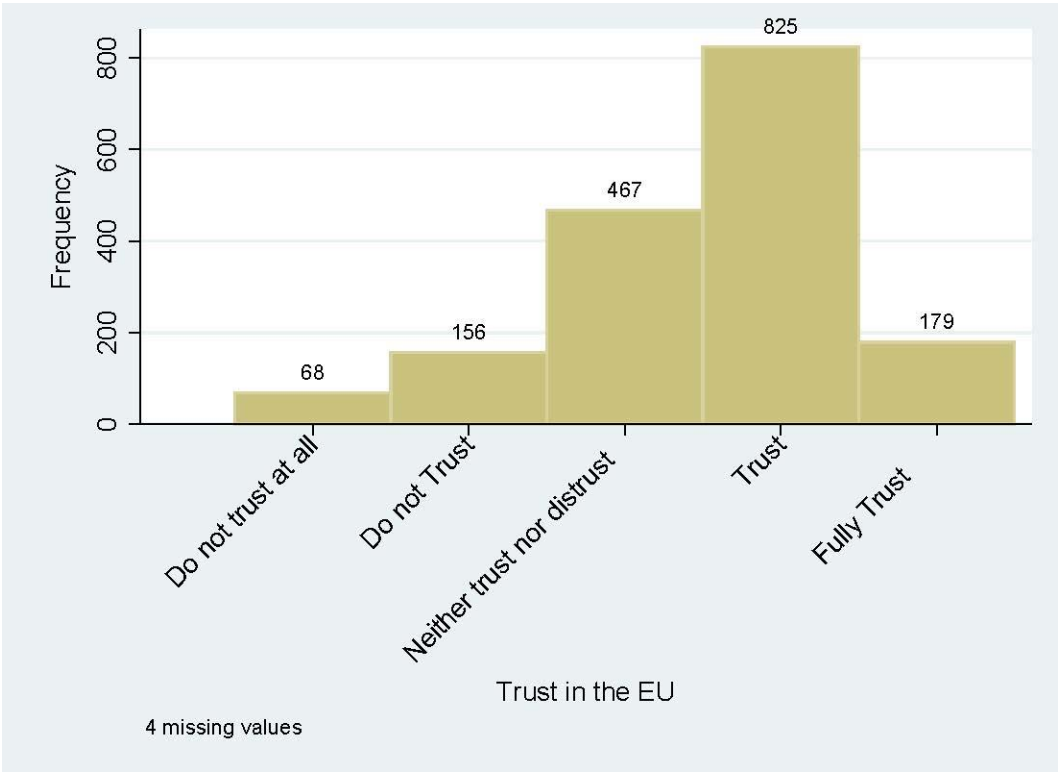

Frequency
0
200
400
600
800
38
143
538
692
255
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the trade unions
33 missing values

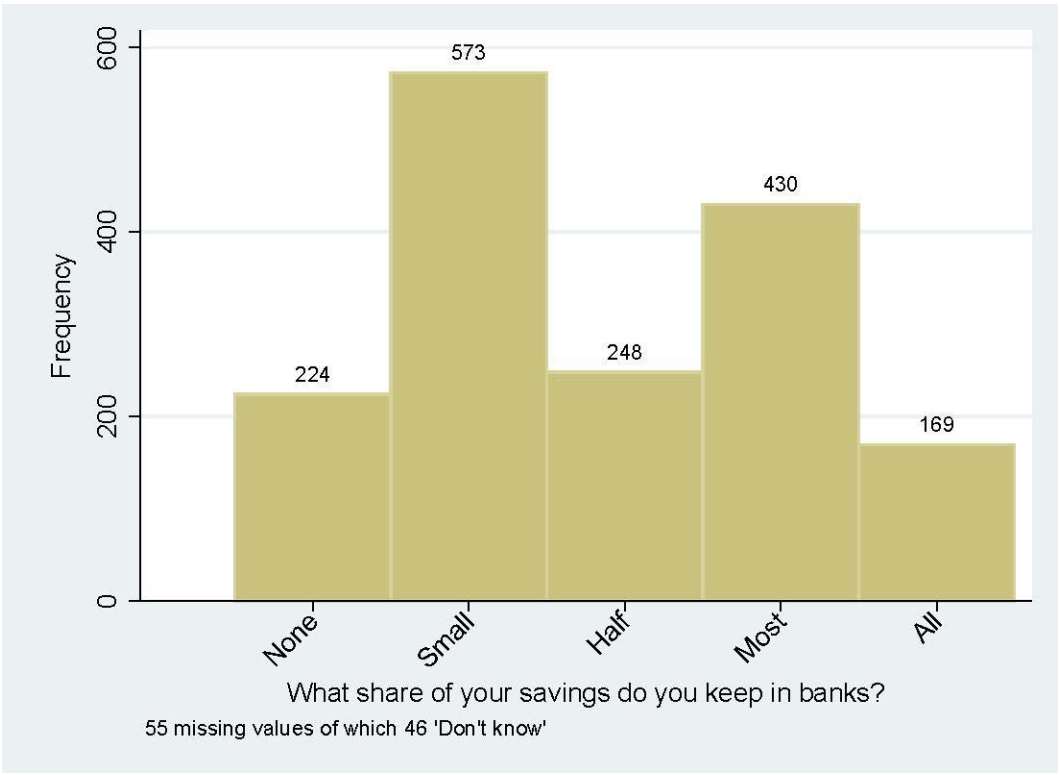

Frequency
0
200
400
600
214
558
602
278
44
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the banks
3 missing values

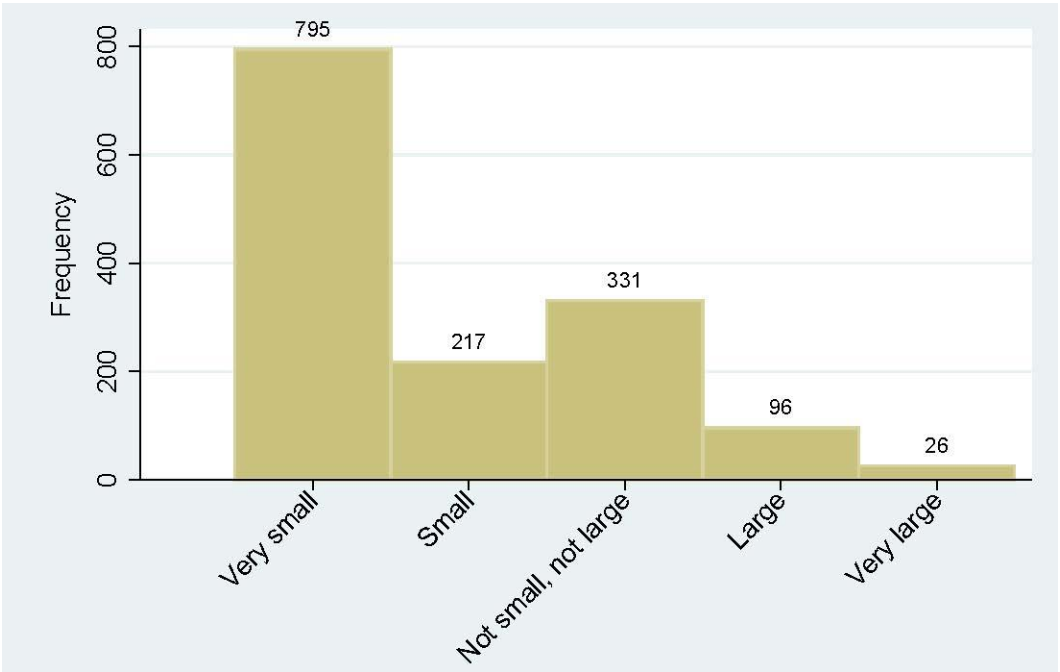

Frequency
0
200
400
600
800
166
369
682
439
42
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the media
1 missing value


Frequency
0
200
400
600
800
114
238
461
681
205
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the police
0 missing values


Frequency
0
200
400
600
800
131
247
521
664
131
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the federal government
5 missing values

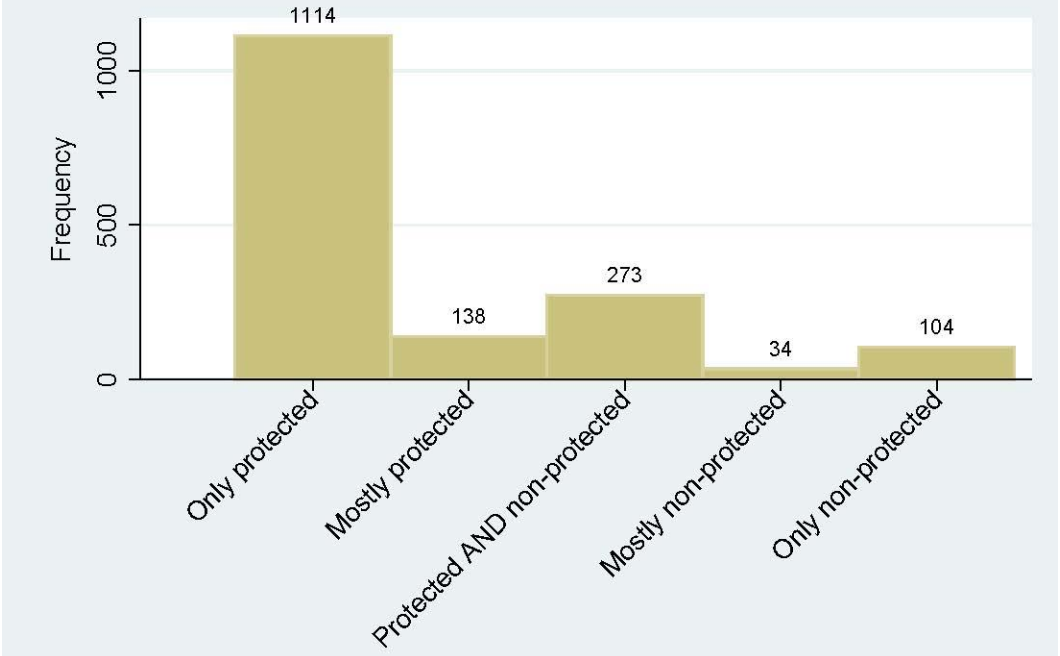

Frequency
0
200
400
600
800
68
156
467
825
179
Do not trust at all
Do not Trust
Neither trust nor distrust
Trust
Fully Trust
Trust in the EU
4 missing values


Frequency
0
200
400
600
224
573
248
430
169
None
Small
Half
Most
All
What share of your savings do you keep in banks?
55 missing values of which 46 'Don't know'



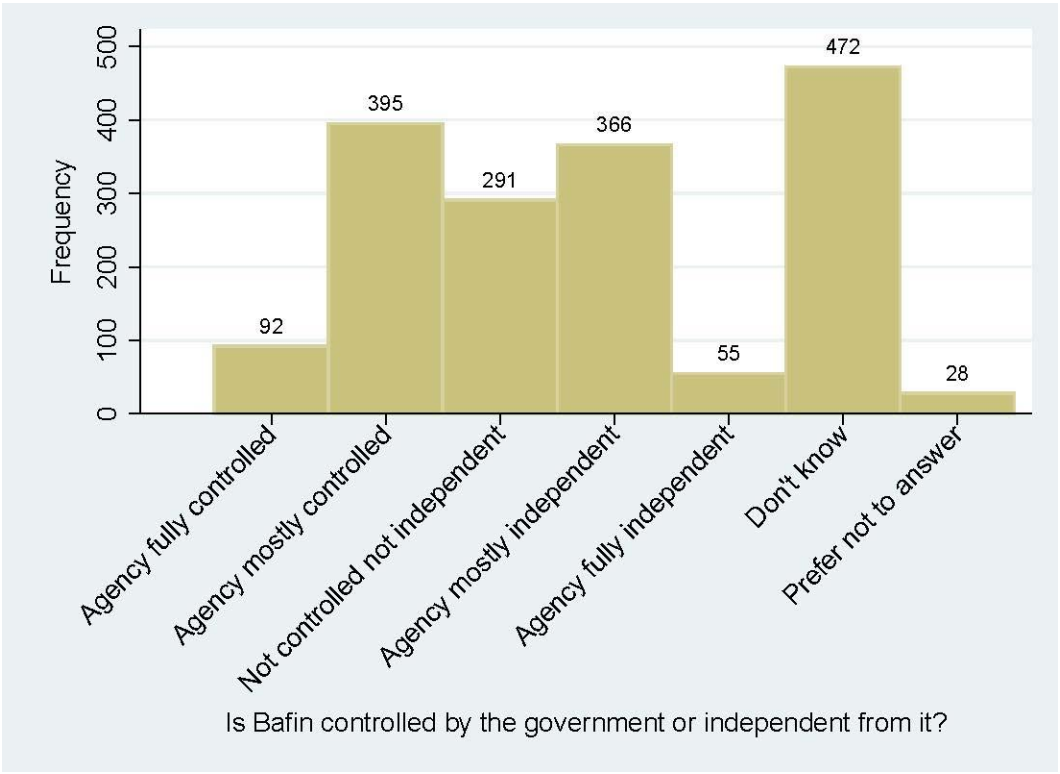
Realtive to your income how large is your debt to banks?

234 missing values of which 146 'Don't know'



In whay type of bank do you manage your personal accounts?

246 missing values of which 207 'Don't know'


Frequency
0
100
200
300
400
500
92
395
291
366
55
472
28
Agency fully controlled
Agency mostly controlled
Not controlled not independent
Agency mostly independent
Agency fully independent
Don't know
Prefer not to answer
Is Bafin controlled by the government or independent from it?

Appendix C: blame attribution for negative economic shocks – heterogeneous effect

Table C1: Heterogeneous attribution to large government parties - gender, age, Länder and class

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A_*LGovParty*	-0.026	0.045	0.063	-0.008	0.009	0.016	0.024	0.041
	(0.058)	(0.053)	(0.076)	(0.046)	(0.047)	(0.069)	(0.065)	(0.043)
A_*BailoutTax*	-0.246***	-0.180***	-0.197***	-0.214***	-0.196***	-0.244***	-0.212***	-0.202***
	(0.029)	(0.028)	(0.040)	(0.023)	(0.024)	(0.035)	(0.034)	(0.022)
A_*LGovParty* × A_*BailoutTax*	0.120**	0.098	0.070	0.124**	0.123**	0.078	0.092	0.127***
	(0.060)	(0.060)	(0.084)	(0.049)	(0.051)	(0.076)	(0.070)	(0.048)
A_*BadBankPers*	-0.194***	-0.110***	-0.120***	-0.160***	-0.166***	-0.111***	-0.135***	-0.139***
	(0.029)	(0.026)	(0.040)	(0.022)	(0.024)	(0.034)	(0.032)	(0.021)
A_*LGovParty* × A_*BadBankPers*	-0.064	-0.044	-0.135	-0.032	-0.034	-0.106	-0.093	-0.077
	(0.063)	(0.060)	(0.086)	(0.050)	(0.051)	(0.082)	(0.072)	(0.048)
A_*Recession*	-0.292***	-0.243***	-0.227***	-0.280***	-0.248***	-0.306***	-0.229***	-0.254***
	(0.028)	(0.028)	(0.039)	(0.023)	(0.024)	(0.036)	(0.034)	(0.022)
A_*LGovParty* × A_*Recession*	0.050	0.021	0.064	0.020	0.045	0.015	0.078	0.054
	(0.061)	(0.060)	(0.084)	(0.050)	(0.052)	(0.076)	(0.072)	(0.048)
A_*PersIncomeFall*	-0.325***	-0.343***	-0.355***	-0.328***	-0.359***	-0.279***	-0.325***	-0.315***
	(0.030)	(0.028)	(0.041)	(0.024)	(0.025)	(0.037)	(0.035)	(0.023)
A_*LGovParty* × A_*PersIncomeFall*	0.038	-0.162***	-0.154*	-0.034	-0.074	-0.044	-0.071	-0.092**
	(0.061)	(0.060)	(0.088)	(0.049)	(0.053)	(0.074)	(0.069)	(0.047)
A_*EconParty*	-0.134***	-0.285***	-0.276***	-0.192***	-0.251***	-0.125***	-0.307***	-0.252***
	(0.027)	(0.028)	(0.040)	(0.023)	(0.024)	(0.036)	(0.032)	(0.022)
A_*LCParty*	0.332***	0.204***	0.199***	0.287***	0.247***	0.302***	0.291***	0.269***
	(0.028)	(0.026)	(0.038)	(0.022)	(0.023)	(0.034)	(0.030)	(0.021)
A_*EUParty*	-0.582***	-0.430***	-0.489***	-0.506***	-0.491***	-0.526***	-0.587***	-0.540***
	(0.029)	(0.030)	(0.042)	(0.024)	(0.025)	(0.038)	(0.037)	(0.024)
A_*EUSuper*	-0.023	-0.019	-0.030	-0.018	-0.011	-0.049	-0.000	-0.010
	(0.025)	(0.024)	(0.035)	(0.020)	(0.021)	(0.031)	(0.029)	(0.019)
N	12,624	13,076	6,526	19,174	17,774	7,926	9,180	20,730
pseudo R^2	0.077	0.060	0.068	0.069	0.069	0.069	0.081	0.072

Dependent variable is *B3way*. Observations are restricted to participants who self-identify as non-male (1) male (2), are old (3), young (4), live in (5) rich Länder, (6) poor Länder, and self-identify as (7) high-class (8) low-class. Results of probit regressions. Standard errors in parentheses are clustered on participant IDs. The prefix A denotes dummies for randomized conjoint attributes. *ALGovParty* is a dummy for a large party in government. The constant, taskcount, and socio-demographic variables are suppressed for presentation. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Table C2: Heterogeneous attribution to large government parties - education, trust in institutions, Euroscepticism and economic values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A_*LGovParty*	0.017	0.000	-0.048	0.072	0.032	0.016	0.034	-0.001
	(0.054)	(0.057)	(0.056)	(0.054)	(0.087)	(0.044)	(0.064)	(0.049)
A_*BailoutTax*	-0.207***	-0.214***	-0.248***	-0.176***	-0.195***	-0.219***	-0.225***	-0.201***
	(0.027)	(0.029)	(0.029)	(0.027)	(0.046)	(0.022)	(0.032)	(0.026)
A_*LGovParty* × A_*BailoutTax*	0.058	0.171***	0.120**	0.107*	0.137	0.107**	0.060	0.143***
	(0.058)	(0.062)	(0.058)	(0.062)	(0.092)	(0.048)	(0.071)	(0.053)
A_*BadBankPers*	-0.149***	-0.155***	-0.142***	-0.160***	-0.085*	-0.166***	-0.133***	-0.162***
	(0.028)	(0.027)	(0.028)	(0.027)	(0.045)	(0.022)	(0.030)	(0.026)
A_*LGovParty* × A_*BadBankPers*	-0.031	-0.081	-0.041	-0.071	-0.242**	-0.022	-0.159**	-0.001
	(0.061)	(0.063)	(0.064)	(0.060)	(0.098)	(0.049)	(0.070)	(0.056)
A_*Recession*	-0.291***	-0.242***	-0.277***	-0.260***	-0.308***	-0.259***	-0.273***	-0.258***
	(0.028)	(0.027)	(0.028)	(0.028)	(0.041)	(0.022)	(0.032)	(0.025)
A_*LGovParty* × A_*Recession*	0.086	-0.017	0.106*	-0.042	0.106	0.007	0.047	0.019
	(0.058)	(0.063)	(0.063)	(0.059)	(0.099)	(0.048)	(0.071)	(0.054)
A_*PersIncomeFall*	-0.339***	-0.331***	-0.360***	-0.309***	-0.362***	-0.330***	-0.258***	-0.395***
	(0.029)	(0.029)	(0.030)	(0.028)	(0.048)	(0.023)	(0.031)	(0.027)
A_*LGovParty* × A_*PersIncomeFall*	-0.055	-0.079	-0.056	-0.080	-0.104	-0.055	-0.049	-0.055
	(0.059)	(0.063)	(0.064)	(0.059)	(0.097)	(0.049)	(0.067)	(0.057)
A_*EconParty*	-0.246***	-0.176***	-0.164***	-0.262***	-0.258***	-0.207***	-0.035	-0.335***
	(0.028)	(0.028)	(0.029)	(0.028)	(0.046)	(0.022)	(0.030)	(0.026)
A_*LCParty*	0.280***	0.250***	0.234***	0.297***	0.021	0.330***	0.425***	0.158***
	(0.026)	(0.027)	(0.028)	(0.026)	(0.044)	(0.021)	(0.029)	(0.024)
A_*EUParty*	-0.589***	-0.408***	-0.300***	-0.700***	-0.013	-0.627***	-0.662***	-0.396***
	(0.030)	(0.029)	(0.030)	(0.028)	(0.043)	(0.023)	(0.032)	(0.027)
A_*EUSuper*	-0.051**	0.014	0.008	-0.046*	-0.043	-0.015	-0.057**	0.004
	(0.024)	(0.025)	(0.024)	(0.025)	(0.040)	(0.019)	(0.027)	(0.022)
N	13,620	12,080	12,210	13,490	4,792	20,908	10,804	14,896
pseudo R^2	0.079	0.057	0.052	0.092	0.042	0.083	0.090	0.063

Dependent variable is B3way. Observations are restricted to participants who self-identify as (1) university graduates, (2) non-university graduates, (3) NOT trusting institutions, (4) trusting institutions, (5) having anti-EU views, (6) having pro-EU views, (7) having left-wing economic views, (8) NOT having left-wing economic views. See Table C1 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C3: Heterogeneous attribution to large government parties - political values, exposure to banks, protected banks and *Bafin* knowledge

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A_*LGovParty*	0.038	-0.025	-0.009	0.032	0.043	-0.026	0.135*	-0.032
	(0.051)	(0.061)	(0.063)	(0.050)	(0.049)	(0.067)	(0.079)	(0.045)
A_*BailoutTax*	-0.230***	-0.195***	-0.238***	-0.194***	-0.224***	-0.189***	-0.182***	-0.222***
	(0.027)	(0.030)	(0.033)	(0.025)	(0.025)	(0.035)	(0.039)	(0.023)
A_*LGovParty* × A_*BailoutTax*	0.093*	0.144**	0.139**	0.102*	0.116**	0.110	0.087	0.121**
	(0.056)	(0.066)	(0.066)	(0.055)	(0.053)	(0.072)	(0.089)	(0.048)
A_*BadBankPers*	-0.154***	-0.138***	-0.172***	-0.137***	-0.157***	-0.135***	-0.154***	-0.150***
	(0.026)	(0.029)	(0.031)	(0.025)	(0.024)	(0.036)	(0.038)	(0.023)
A_*LGovParty* × A_*BadBankPers*	-0.049	-0.089	-0.085	-0.049	-0.060	-0.058	-0.134	-0.033
	(0.058)	(0.068)	(0.073)	(0.055)	(0.055)	(0.074)	(0.085)	(0.051)
A_*Recession*	-0.258***	-0.279***	-0.297***	-0.251***	-0.247***	-0.311***	-0.264***	-0.269***
	(0.026)	(0.030)	(0.032)	(0.025)	(0.024)	(0.036)	(0.042)	(0.022)
A_*LGovParty* × A_*Recession*	-0.035	0.129*	0.035	0.041	-0.028	0.120	0.058	0.024
	(0.057)	(0.066)	(0.069)	(0.056)	(0.054)	(0.073)	(0.081)	(0.051)
A_*PersIncomeFall*	-0.317***	-0.368***	-0.356***	-0.315***	-0.334***	-0.347***	-0.351***	-0.328***
	(0.026)	(0.033)	(0.033)	(0.026)	(0.025)	(0.036)	(0.040)	(0.024)
A_*LGovParty* × A_*PersIncomeFall*	-0.036	-0.093	-0.108	-0.054	-0.070	-0.061	-0.185**	-0.025
	(0.056)	(0.068)	(0.068)	(0.056)	(0.054)	(0.074)	(0.084)	(0.050)
A_*EconParty*	-0.070***	-0.419***	-0.216***	-0.209***	-0.212***	-0.208***	-0.261***	-0.196***
	(0.025)	(0.030)	(0.030)	(0.027)	(0.025)	(0.034)	(0.041)	(0.023)
A_*LCParty*	0.392***	0.088***	0.285***	0.253***	0.237***	0.312***	0.251***	0.270***
	(0.025)	(0.028)	(0.030)	(0.025)	(0.023)	(0.033)	(0.037)	(0.022)
A_*EUParty*	-0.642***	-0.315***	-0.561***	-0.459***	-0.470***	-0.558***	-0.572***	-0.478***
	(0.027)	(0.032)	(0.032)	(0.028)	(0.025)	(0.038)	(0.039)	(0.025)
A_*EUSuper*	-0.027	-0.018	-0.024	-0.012	-0.001	-0.058*	-0.050	-0.012
	(0.023)	(0.026)	(0.027)	(0.023)	(0.021)	(0.031)	(0.035)	(0.020)
N	15,478	10,222	10,434	14,914	16,914	8,314	6,404	19,296
pseudo R^2	0.085	0.060	0.081	0.059	0.065	0.078	0.079	0.065

Dependent variable is *B3way*. Observations are restricted to participants who self-identify as (1) having left-wing political views, (2) or not, (3) highly exposed to banks, (4) or not, (5) preferring to work with protected banks, (6) or not, (7) being aware that Bafin is independent from the government, (8) or not. See Table C1 for more notes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C4: Pairwise correlations between heterogeneity dummies

	NonMale	*Old*	*RichState*	*University*	*Rich*	*LeftEcon*	*LeftPol*	*Eurosceptic*	*NoTrustInInstitutions*	*HighBankExp*	*ProtectedBanks*
Old	-0.09										
RichState	-0.01	-0.05									
University	0.05	0.14	-0.01								
Rich	-0.04	0.04	0.12	0.22							
LeftEcon	0.12	-0.01	-0.08	-0.00	-0.01						
LeftPol	0.25	-0.08	-0.11	0.01	-0.04	0.41					
Eurosceptic	-0.13	0.09	0.02	-0.09	-0.12	-0.17	-0.27				
NoTrustInInstitutions	0.00	0.01	-0.02	-0.10	-0.14	-0.11	-0.13	0.38			
HighBankExp	0.15	-0.02	-0.01	-0.00	-0.01	0.02	0.08	-0.07	-0.04		
ProtectedBanks	0.03	0.01	0.02	-0.06	-0.02	-0.00	-0.03	-0.01	0.01	0.04	
NotAwareBafin	0.10	-0.07	0.00	-0.11	-0.11	-0.05	-0.05	0.11	0.12	0.00	0.08
N	27,584										