CESifo WORKING PAPERS

10875
2023

December 2023

Restrictions to Civil Liberties in a Pandemic and Satisfaction with Democracy

Daniel Graeber, Lorenz Meister, Panu Poutvaara

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 10875*

Restrictions to Civil Liberties in a Pandemic and Satisfaction with Democracy

Abstract

In times of crises, democracies face the challenge of balancing effective interventions with civil liberties. This study examines German states' response during the early stages of the COVID-19 pandemic, focusing on the interplay between civil liberties and public health goals. Using state-level variation in mobility restrictions, we employ a difference-in-differences design to show that stay-at-home orders notably increased satisfaction with democracy and shifted political support towards centrist parties. Stay-at-home orders increased satisfaction with democracy most among individuals who had been exposed to the authoritarian regime of the German Democratic Republic. A potential explanation is that these individuals had got used to more restrictive state interventions. Moreover, we find suggestive evidence that satisfaction with democracy increases more among individuals who are obese or have low vitality, possibly because their benefit from the mobility restrictions is higher. However, these differences are not statistically significant.

JEL-Codes: D720, H120, I120, I180. P260.

Keywords: perceptions of public policies, satisfaction with democracy, Covid-19.

Daniel Graeber
German Institute for Economic Research - DIW Berlin / Germany
dgraeber@diw.de

Lorenz Meister
German Institute for Economic Research - DIW Berlin / Germany
lmeister@diw.de

Panu Poutvaara
ifo Institute – Leibniz Institute for Economic Research at the University of Munich / Germany
poutvaara@ifo.de

The authors are grateful for valuable feedback from Joop Adema, Cevat Aksoy, Damien Bol, Clara Martínez-Toledano, Sascha Riaz, Chris Roth, Stefanie Stantcheva, and participants of the 2023 meeting of the European Public Choice Society (EPCS), the 36th Annual Conference of the European Society for Population Economics (ESPE) and the 2023 Annual Congress of the International Institute of Public Finance (IIPF). The Socio-Economic Panel data can be made available on-site via the research data center of the Socio-Economic Panel group at the DIW Berlin. Daniel Graeber and Lorenz Meister acknowledge funding from the project "Groups put at Particular Risk by COVID-19 (GaPRisk)," funded by the German Research Foundation (SCHR 1498/7-1). The authors declare they have no conflict of interest. Parts of this text were produced with the help of a generative AI model. Any remaining errors are our own.

1. Introduction

What determines citizens' acceptance of restrictive state interventions in a democracy? This question is particularly relevant when unexpected crises, such as armed conflicts, natural disasters or pandemics require immediate government decisions to protect the population. While government interventions can address inefficiencies and coordinate crisis responses, they may affect citizens' freedom of assembly, property rights, or right to privacy while being in place. Especially in democracies, inappropriate interventions might harm public trust and undermine the legitimacy of the government.

To answer this question, we explore the early setting of the COVID-19 pandemic, which was the worst sudden onset of a global crisis since the Second World War and strongly highlighted tension between civil liberties and public health objectives. At the onset of the pandemic, rapid spread of the disease and associated risks prompted many governments to intervene on a scale hardly imaginable before the pandemic. Since a vaccine or medication was not readily available at the beginning of the pandemic, governments resorted to nonpharmaceutical interventions (NPIs) to slow the spread of the SARS-CoV-2, the virus causing COVID-19. Many NPIs strongly interfered with civil liberties – most notably, stay-at-home orders interfered with the freedom of movement.

In the wake of the pandemic, governments were facing a clear trade-off: Delayed interventions could cost lives and cause irreversible health damage, with overcrowded hospitals forcing physicians to apply triage. Yet, also rash action could have adverse effects. For example, Fuchs-Schündeln et al. (2022) provide evidence on long-term harm done by very long school closures that may affect especially children from disadvantaged backgrounds throughout their lives. In addition to direct negative effects on those whose rights and opportunities are curtailed, disproportionate interventions may have negative long-term consequences on trust in institutions and satisfaction with democracy.

Clearly, for policy makers, the decision to implement NPIs to slow the progression of the pandemic was a difficult choice to make. We aim at informing such decisions by quantifying the effect of such an NPI on citizens' trust in democratic institutions. That is, we will estimate the effect of stay-at-home orders on individuals' satisfaction with democracy. The direction of the effect is not clear a priori. On one side, citizens concerned about their own and others' health and the risk

of an infection might respond to mobility restrictions with increased trust in democracy. On the other side, the order to stay at home interferes with people's freedom of movement and assembly. Citizens might perceive the intervention as disproportionate to the associated health risks and be concerned about losing some civic liberties that are closely linked with a liberal democracy.

Estimating a causal effect is challenging because policymakers do not randomize stay-at-home orders. For instance, the implementation of stay-at-home orders could be associated with either strong or weak democratic institutions which in turn might be positively or negatively correlated with residents' satisfaction with democracy. In this case, the effect of interest might be biased upwards or downwards. Similarly, if trends in satisfaction with democracy would have varied across treated and non-treated jurisdictions in absence of the policy, the estimates would be biased, too. In addition to methodological considerations, we require individual level data that provides us with (1) respondents' satisfaction with democracy, (2) respondents' location of residence and (3) information on respondents' pre-determined characteristics. Typically, this information is rarely available in other data, such as polls or surveys that were conducted during the pandemic to elicit individuals' attitudes.

In this paper, we exploit a unique setting to estimate the causal effect of stay-at-home orders on citizens' satisfaction with democracy at the beginning of the COVID-19 pandemic. In Germany, only a subset of the 16 federal states implemented mobility restrictions to slow down the spread of the virus, yielding a treatment and control group with balanced covariates. To identify a causal effect, we estimate difference-in-differences (DID) models and conduct DID-event study analyses. The outcome measure, satisfaction with democracy, is provided by the Socio-Economic Panel (SOEP). The SOEP is a representative household panel that also includes a large set of pre-treatment individual characteristics (Goebel et al., 2019). We link this survey with daily information on pandemic policies at the state level, provided by the Leibniz Institute for Psychology and Steinmetz et al. (2022). In our setting, stay-at-home orders are legal regulations, defined as the "prohibition to leave the apartment without reason (i.e. for nutritional reasons or doctoral visits)" by Steinmetz et al. (2022). As Germany was historically divided into democratic West Germany and socialist East Germany, we can also study whether responses to pandemic restrictions depended on in which type of society individuals were socialized in. Individuals who have lived in the German Democratic Republic might react differently to restrictive state interventions due to their exposure

to communist ideology and the experience of living in an authoritarian state.

Our results show that on average, mobility restrictions increased individuals' satisfaction with democracy by approximately 17% of a standard deviation, relative to the control group. The result indicates strong political approval of mobility restrictions in the affected overall population during the onset of the pandemic. This result is robust to a wide range of robustness tests that include placebo estimations in the pre-pandemic years and wild-cluster bootstrap procedures to account for the small number of clusters. To rule out the concern that infection incidences determine the intervention and changes in satisfaction with democracy at the same time, we show that at the time of the policy implementation, incidences in the treatment group were not higher than in the control group.

Our DID-event study analysis shows that the effect emerges directly in the first week after the implementation of the mobility restriction and remains constant over the whole duration of the intervention. Due to the incubation time of about a week and a lag of several days in incidence reporting, we interpret the event study results as evidence for reactions to the policy itself, and not its actual success in slowing down the spread of the virus. A possible explanation alludes to individuals' expectations at the beginning of the phase of mobility restrictions. Most experts and politicians were agreeing on the implementation of mobility restrictions in the very early phase of the pandemic. Hence, the increase in satisfaction with democracy could be due to the government acting in line with the public discourse on how to deal with the highly uncertain and threatening situation.

In the second part of the paper, we explore possible mechanisms behind our findings. We show that past exposure to an authoritarian regime is another strong determinant of an individual's response to the mobility restriction. The overall increase in satisfaction with democracy is driven by individuals whose location in 1989 was the former German Democratic Republic (GDR), a socialist state with strong surveillance. In this group, satisfaction with democracy increased by 31% of a standard deviation due to the intervention, while it increased only 11% for individuals who were located in the Federal Republic of Germany (FRG) in 1989. These findings suggest that there is a socialization effect: The perceived appropriateness of restrictive state interventions depends on past exposure to restrictive state interventions. This result aligns well with Alesina and Fuchs-Schündeln (2007), who show that living in East Germany makes individuals more supportive of

state interventions, a preference that takes two generations to fade out. While Alesina and Fuchs-Schündeln (2007) focus on welfare state interventions, we explore restrictive crisis interventions.

Next, we test whether the effects are larger for individuals with a higher risk of a severe progression of COVID-19 and individuals for whom the cost of staying at home is low. The evidence on vulnerable health groups is not conclusive across various domains, such as body mass index, vitality, self-reported health and health worries. Moreover, our estimates show that the overall positive effect cannot be found for households with children, possibly due to an increased childcare need and higher levels of stress.

Finally, we estimate the effect of the mobility intervention on party preferences. Using the same empirical strategy as in the main part, we find that the stay-at-home orders increased support for centrist parties by 7 percentage points, partly at the expense of the far-right parties including the populist Alternative for Germany (AfD), which lost 2 percentage points of support. This shift underscores the political impact of evidence-based crisis interventions on public opinion and party affiliation.

Our paper contributes to a burgeoning literature on the perception of restrictive policy interventions during a health crisis. Key contributions are based on survey experiments that were designed and implemented in Western democracies during the onset of the COVID-19 pandemic. Alsan et al. (2023a) investigate individuals' willingness to sacrifice civil liberties for public health. Building on variation in local COVID-19 related mortality and an information treatment, they find that individuals become more willing to sacrifice their own rights, give up free press, or suspend democratic procedures, as health insecurity rises. Algan et al. (2021) investigate how various measures of trust affect support for and compliance with NPIs during the first months of the pandemic. The study shows that trust in scientists has a positive effect, while trust in the government has more ambiguous effects and trust in others has a rather negative effect on compliance with NPIs, possibly because individuals who trust others expect other individuals to voluntarily distance and hence, may consider government administered NPIs unnecessary. Alsan et al. (2023b) investigate civil liberties concerns and reluctance to mobility and privacy restrictions. Using an information treatment on aggressive mobility restrictions and privacy infringements in China and South Korea, they do not find an effect on the willingness to sacrifice democratic procedures in general. However, they do observe more reluctance to giving up the freedom of movement or privacy specifically.

Our study is closest to Bol et al. (2021), who compare the effect of national lockdowns across Europe on individuals' support for the current prime minister as well as satisfaction with democracy. In line with our study, the authors find that the lockdown increased satisfaction with democracy by about 3% on the full response scale, which ranges from zero to eleven. They explain the increased support for the government by a retrospective performance evaluation, according to which citizens understand that the strict measures were necessary and thus, increased support for those responsible for this policy. We build on this important initial evidence along three lines. First, our estimate clearly allows for a causal interpretation, given the evidence on common pre-trends and institutional and demographic similarity between treatment and control group. Second, by the means of an event study, we can show how the effect evolves over time; it is visible right in the first week after the treatment and stays constant over the whole treatment period. Third, turning to the mechanism, we show that the effect strongly depends on the perceived proportionality of the intervention, which depends on local infection numbers, and that individuals who lived in socialist East Germany show higher approval of the restrictive intervention.

Baekgaard et al. (2020) investigated the effect of the announcement of the lockdown in Denmark on respondents' trust in the prime minister's administration and parliament. They exploit daily survey data of unemployed persons in a regression discontinuity in time design. They find that the lockdown increased citizens' political support for the government considerably. We build on the study of Baekgaard et al. (2020) by analyzing a representative sample of the German population, compared to unemployed persons in Denmark.

We also contribute to a wider literature on the rallying around the flag effect, which describes the fact that citizens display an increase in the support of their political leaders in time of international crises (Mueller, 1973). Since then, the phenomenon has been mainly observed in the context of international conflicts (Edwards and Swenson, 1997; Kriner, 2006; Parker, 1995; Shapiro, 1991) or terrorist attacks (Dinesen and Jæger, 2013; Perrin and Smolek, 2009; Wollebæk et al., 2012). Recently, the literature documented rallying around the flag at the onset of the COVID-19 pandemic in 2020, proving that rallying around the flag emerges not only as the result of a inter-group conflict, but also in the wake of public health crisis (Mazza and Scipioni, 2022; Merkley et al., 2020; Schraff, 2021; Yam et al., 2020). This literature is mainly centered on the descriptive analysis of approval ratings during the pandemic. One robust finding in this literature is the fact that approval ratings for

governments increase as the severity of the pandemic increases (Schraff, 2021; Yam et al., 2020, e.g.). We contribute to this literature by analyzing the effect of a specific NPI to slow the spread of COVID-19 on satisfaction with democracy.

Our study also relates to the economic literature on the change of attitudes and preferences in times of crises. A wide array of the literature is centered on eliciting the effect of experiencing conflicts (Callen et al., 2014; Jakiela and Ozier, 2019), recessions (Malmendier and Nagel, 2011) and inequality (Roth and Wohlfart, 2018) on attitudes or preferences. We add to this by estimating the change in attitudes as a response to the introduction of a mobility restriction during the onset of the COVID-19 pandemic.

The paper proceeds as follows: In Section 2., we provide some institutional details and details about the COVID-19 pandemic in Germany in 2020. In Section 3. we present our data, Section 4. introduces our empirical strategy and 5. displays our results. Section 6. discusses and concludes.

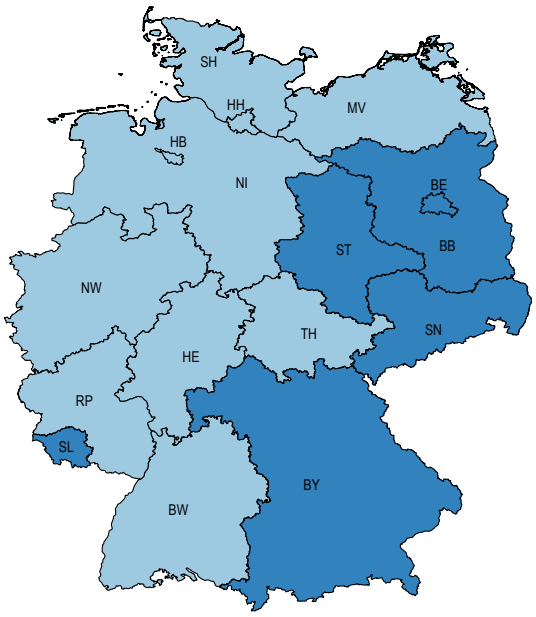
2. Covid-19 pandemic in Germany

The first COVID-19 infection in Germany was detected on 27 January 2020. Although the initial cluster was contained rapidly, soon other cases of COVID-19 surfaced. Figure 2 displays the seven day incidence per 100,000 residents at the state level over time. The first wave of the pandemic was widespread in all states in Germany. The pandemic quickly became the most prominent topic in German media. On 11 March 2020, the World Health Organization (WHO) declared COVID-19 a pandemic (World Health Organization, 2020). Politicians and health experts tried to cope with the unexpected health crisis, while only little was known about SARS-CoV-2, the virus that causes COVID-19, at that point in time. During that time, high levels of uncertainty in the population about transmission channels, medical treatment options, and the criteria for a high susceptibility for a severe progression of COVID-19 existed. Pharmaceutical interventions, such as vaccinations or medicine, were not available in 2020 such that politicians had to rely on NPI to restrict the spread of SARS-CoV-2.

The NPIs included stay-at-home orders, physical distancing rules, the duty to wear a mask, closure of retail stores, zoos, and theaters, the prohibition of demonstrations, as well as mandatory homeschooling and workplace restrictions. However, the most heatedly debated measures were the stay-at-home orders, which constituted arguably the strongest intervention into people's civic

liberties.

Figure 1: Map of Germany that shows treated states.



Notes: The map depicts the geographic distribution of states that passed a strict stay-at-home order in March 2020 (treatment group, dark shade) and states that did not (control group, light shade). Source: ZPID and Steinmetz et al. (2022). Illustration: own.

An important detail of the legislation for our study is the fact that the containment of commutable diseases was the states' responsibility at that time in Germany. While most of the measures were implemented uniformly across all states, the stay-at-home orders were implemented in a subset of states only. Figure 1 displays the geographical distribution of states that implemented mobility restrictions (treatment group) and those that did not (control group). The treatment group consists of three states that were fully in the former GDR, two states that were fully in the former FRG, and Berlin that was partly in the GDR and partly in the FRG. This balanced distribution rules out concerns in the subsequent analysis that the observed effect is driven by the historical German division. The stay-at-home orders were implemented almost simultaneously across states in the treatment group. Exact dates range from 21 of March to 28 of April and are listed in appendix Table 8. Table 1 shows that individuals in the treatment and control group are similar with respect to a wide range of demographic characteristics. Similar groups support the counterfactual existence of common trends in satisfaction with democracy among treatment and control group in absence of the treatment. This unique setting allows us to exploit exogenous variation across states and time

to evaluate the effect of the mobility restriction on individuals' satisfaction with democracy.

3. Data

We combine survey data from the SOEP with the ZPID Lockdown Measures Dataset for Germany and data on infection rates from the Robert Koch Institute (RKI) to estimate the effect of a mobility restriction on individuals' satisfaction with democracy. Our sample covers the period from February 26 to April 19, 2020. This allows us to include observations about one month prior and after to the beginning of the stay-at-home orders. The SOEP is uniquely suited for this analysis since it provides us with information on individuals' satisfaction with democracy and a wide range of predetermined information on individuals' socio-demographic characteristics, together with the respondents' household location. The SOEP is a representative panel of households and their members in Germany. It started in 1984 and surveys the respondents about a wide range of topics such as individuals' satisfaction with a wide range of domains, socio-demographics, labor market experience, health and attitudes, among others, on a yearly basis. Today, the SOEP surveys about 30,000 individuals in 15,000 households (Goebel et al., 2019).[1]

Most importantly for our analysis, the SOEP surveys their respondents about their satisfaction with democracy. To be precise, the respondents are asked "How satisfied are you with democracy as it exists in Germany?" Responses are given on an eleven-point Likert scale ranging from zero "completely dissatisfied" to ten "completely satisfied." Respondents in the SOEP are asked about their satisfaction with democracy since 2005 about every five years. Throughout, we standardize the outcome by subtracting the mean of the control group and dividing this difference by the standard deviation of the outcome of the control group. For this transformation, we take both of these figures from the pre-treatment phase. Hence, effect sizes can be interpreted as measured in standard deviations of the pre-treatment control group.

Due to the concern that mobility restrictions could be correlated with COVID-19 incidence which in turn could be correlated with satisfaction with democracy, we present in Table 9 an analysis on the predictors of state-level COVID-19 incidence rates on five days before and five days after the introduction of stay-at-home orders. Column 1 shows that states in former East Germany had considerably lower infection rates, and higher household income was associated with higher

[1] We use the SOEP v37.eu (DOI: 10.5684/soep.core.v37eu).

Table 1: Summary statistics by treatment status.

	Control Group		Treatment Group	
	Mean	S.d.	Mean	S.d.
Age	51.03	17.57	51.62	17.20
Female	0.53	0.50	0.52	0.50
Married	0.63	0.48	0.60	0.49
Parent	0.46	0.50	0.41	0.49
Migration Background	0.22	0.42	0.17	0.37
University Graduate	0.29	0.46	0.31	0.46
HH Net Income	4,000	4,275	3,602	3,765
Urban	0.68	0.47	0.65	0.48
March 20 incidence	20.46	8.56	15.72	5.55
Observations	6,315		3,298	

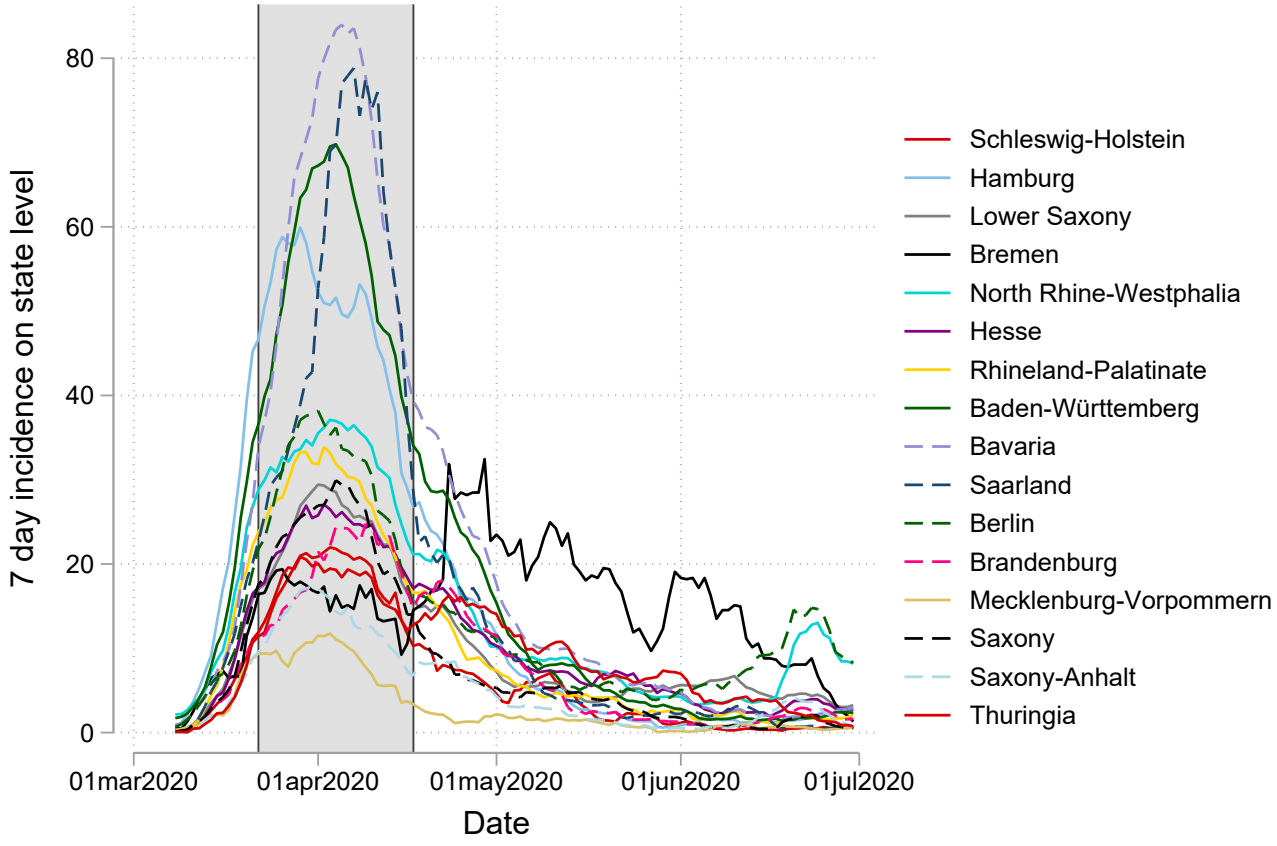
Notes: The table shows average characteristics in 2020 for individuals in the treatment group and individuals in the control group, respectively. Source: SOEPv37.

infection rates. Importantly, the difference between the treatment group and the control group is small and not even statistically significant at the 5 percent significance level. This suggests that differences in incidence rates between treatment and control group before stay-at-home orders are unlikely to explain subsequent differences in satisfaction with democracy.

In a separate analysis, we are interested in how individuals adjust their party preferences. For this, we rely on the individuals' responses to the consecutive items: "Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party?" and "Which party do you lean toward?" The response options to the first item are "Yes" and "No". If individuals responded "Yes", they are presented with the second of these two items. The response options to the second item are the "Social Democratic Party of Germany (SPD)", the "Christian Democratic Union of Germany (CDU)", the "Christian Social Union in Bavaria (CSU), the "Free Democratic Party (FDP)", "Alliance 90/The Greens", "The Left", "Alternative for Germany (AfD)", or "National Democratic Party of Germany/The Republicans/The Right – Party for Referendum, Sovereignty and Homeland Protection" (NPD)[2]. There is also the option "Other". We build three indicators, one for "Far-left party preference" if respondents are indicating that they have a preference for the party The Left, one for "Far-right

[2]The acronym only refers to the first of these three parties. However, for the sake brevity, we just subsume all the three parties in the acronym NPD.

Figure 2: Seven day incidence at the state level



Notes: The graph shows the evolution of 7-day incidences of COVID-19 infections across states. Dashed lines mark states in the treatment group, solid lines mark states in the control group. The 7-day incidence is defined as the number of new infections per 100,000 over the past 7 days. The area shaded in grey marks the period of the lockdown. Source: RKI. Illustration: own.

party preference" if they respond that they are preferring the AfD or NPD. We also construct an indicator for "Mainstream party preference" if individuals prefer either mainstream right/center-right parties CDU, CSU, and FDP, or mainstream left/center-left parties SPD and Alliance 90/The Greens. Lastly, we construct an indicator for "No or other party preferences" if individuals indicate they have none of the aforementioned preferences.[3]

We rely on a rich set of predetermined individual and regional-level characteristics to account for potential imbalances in our data across states and time. For example, this could be the case

[3]Note that, for a subset of observations, individuals indicated that they have more than one party preference. For this, we coded the individual leaning towards the party The Left or NPD according to the aforementioned procedure. The remainder observations were allocated to one of the other classifications.

if individuals with higher education respond systematically later to the survey and if this dynamic varies between treatment and control states. In particular, we control for education, gender, migration background, age, degree of urbanization, type of household, and net household income. Note that if these measures are not time-invariant, we use the information from 2019, the year before the COVID-19 pandemic. This way, we ensure that these covariates are exogenous, *i.e.*, not affected by the treatment. Our categorical measure of education corresponds to primary, secondary, and tertiary education. Our gender indicator is one for female respondents and zero otherwise. To control for age, we use a second-order polynomial in age. For the migration background, we distinguish between individuals who do not have a migration background and individuals who migrated to Germany or have at least one parent who was born outside of Germany. To determine whether individuals live in urban areas or not, we rely on the classification provided by the company BIK Aschpurwis + Behrens GmbH, the so called BIK regions. BIK regions are a widely used classification, used by administrative authorities. Here, we use information from the individuals' household location in 2019. Conceptually, these BIK regions are designed to capture commuting patterns in Germany. We also add the household type in 2019 as controls. For that, we distinguish between single households, childless couples, single parents and parents with children. In addition, we control for the household net income from 2019.

For the heterogeneity analysis, we rely on additional individual-level characteristics. We construct an indicator that categorizes individuals into those who were living in the former GDR, West Germany, and abroad in 1989, the year of Germany's reunification. This indicator is only available for the subsample that has been alive in 1989. We also rely on a wide range of biometric, and subjective health information for our heterogeneity analysis.[4] Our first biometric indicator is the body mass index (BMI), surveyed in 2018. We distinguish between individuals who belong to the top decile in the BMI distribution in our sample and individuals who do not belong to the top decile.

Our indirectly revealed health condition is the vitality of individuals. Vitality is inferred by the question "During the last four weeks, how often did you feel energetic?" Responses are given on a four point Likert-scale ranging from one "Always" to four "Never." We aggregate the categories three "Sometimes" and four "Never" to the category "Low to medium vitality" and the categories

[4]We do not use the self-reported doctor diagnosis since the question asks whether individuals were ever diagnosed with a certain health condition. Therefore, in many cases, it is unclear when individuals have been diagnosed with these health conditions and whether they have ever recovered from these health conditions.

one "Always" and two "Often" to "High vitality." Vitality is inferred biannually since 2002. Thus, our indicator is based on information from 2018.

Our information about respondents' subjective health is self-rated health status (SRHS) and worries about one's own health. SRHS is inferred by responses to the single item question "How would you describe your current health?" Responses are given on a five point Likert-scale ranging from one " Very good" to five "Bad." We aggregate the responses to "Very good to satisfactory" and "Poor or Bad." This information comes from responses in 2019.

Concerns about one's own health are provided by responses to the single-item question "How concerned are you about the following issues? Your health..." Responses to this item range from one "Very concerned" to three "Not concerned at all". For our analysis, we distinguish between individuals who are "Not concerned at all" or "Somewhat concerned" and individuals who are "'Very concerned."

We combine the SOEP with daily information on NPIs at the state level from the ZPID Lockdown Measures Dataset for Germany (Steinmetz et al., 2022) that contains detailed information on 14 different governmental measures to slow down transmission rates. Lastly, we add daily information on the incidence, that is the number of COVID-19 cases per 100,000 residents per seven days on the state level (as a control) and on the district level (to study the mechanism). This information is directly reported by the Robert Koch Institute (RKI), the central German public health authority.[5]

4. Empirical strategy

Estimating the causal effect of a policy restricting residents' mobility on individuals' satisfaction with democracy consistently is not trivial. A simple OLS regression of satisfaction with democracy on our indicator for the regional implementation of a mobility restriction would result in inconsistent estimates. The main reason is that unobserved factors, that are jointly correlated with the implementation of mobility restrictions and individuals' satisfaction with democracy, could bias our estimate of interest. For instance, one could hypothesise that one such factor are strong regional institutions that increase the propensity of state governments to implement mobility restrictions. These strong institutions could cause individuals' satisfaction with democracy to be higher than in states with weak regional institutions, on average. Alternatively, time trends in individuals' satis-

[5]The data is made available under `https://npgeo-corona-npgeo-de.hub.arcgis.com/`.

faction with democracy can complicate a simple before and after comparison in the group of states that implement the mobility restriction. In both cases, our estimates of the causal effect of mobility restrictions on individuals' satisfaction with democracy would be biased. This is why we rely on a DID-strategy to estimate the causal effect of the mobility restriction on individuals' satisfaction with democracy. For this, we estimate a DID event study-model of the following form:

$$y_{its} = \gamma_0 + \sum_{\tau \in T} I[t = \tau] * (\gamma_{1\tau} + \gamma_{2\tau} * I[s \in Treat]) + \sum_{\theta \in S} \gamma_{3\theta} * I[s = \theta] + \gamma_4 * \mathbf{X}_{is} + \eta_{its}. \quad (1)$$

In Equation 1, the dependent variable is the satisfaction with democracy, y_{its}, of individual i in treatment week t and state s. Note that we anchor the time in the event-time dimension. In Equation 1, we allow the time trend to differ between the treatment and control states. Therefore, the estimates of $\gamma_{1\tau}$ reflect the baseline trend of the control group in the event-time weeks $\tau \in T$ with $T = \{-4, -3, \ldots, 3, 4\}$. We set the last pre-treatment week 0 to be our baseline week. $I[t = \tau]$ is the event-time indicator for the mobility restriction having been implemented since τ weeks. $I[s \in Treat]$ is an indicator equal to one if state s is in the group of treatment states $Treat = \{BB, BE, BY, SA, SL, ST\}$. The estimates of $\gamma_{2\tau}$ are our coefficients of interest. They summarize the different trajectories of the satisfaction with democracy of individuals in states that implement mobility restrictions and states that do not implement mobility restrictions, relative to the last pre-treatment week. $I[s = \theta]$ are indicators that are equal to one if the state under consideration is equal to θ, where $\theta \in S$ with S being the space of all 16 states under consideration. Hence, the estimates of $\gamma_{3\theta}$ reflect permanent differences in individuals' satisfaction with democracy between states. In some specifications, we also control for a wide range of predetermined individual level controls X_{its}. These characteristics are either time-invariant or are taken from 2019. These characteristics consist of gender, education, migration background, net household income, household composition, a second-order polynomial in age, and a sample indicator[6] and the degree of urbanity of the municipality in which the respondent lives. γ_4 is the vector of coefficients associated with these characteristics. η_{its} is the error term. Throughout, we cluster the standard error at the state level.

[6]The SOEP sample is composed by many different samples, some of them representing widely different populations. As a result of organizational reasons, these samples are fielded in a staggered way over the year. As a result, some sample imbalances can occur.

To estimate the average treatment effect in the treatment period, we also estimate a standard DID specification of the following form:

$$y_{its} = \beta_0 + \beta_1 I[t > 0] * I[s \in Treat] + \sum_{\tau \in T} \beta_{2\tau} * I[t = \tau] + \sum_{\theta \in S} \beta_{3\theta} * I[s = \theta] + \beta_4 * \mathbf{X}_{its} + \varepsilon_{its}. \quad (2)$$

In Equation 2, the indicator $I[t > 0]$ is an indicator equal to one if we observe an individual in the treatment periods and zero otherwise. The estimate of β_1 in Equation 2 is our coefficient of interest and captures the average causal effect of the implementation of the policy restriction on individual's satisfaction with democracy in the treatment weeks. Throughout, we cluster our standard errors at the level of the states (Bertrand et al., 2004). While the event-study design can be used to analyze how the effect develops over time, the standard DiD specification yields the average effect for the post-treatment period and can be easily extended for some heterogeneity analyses.

Identification. The estimates of β_1 and $\gamma_{2\tau}$, with $\tau \in \{1,2,3,4\}$, are consistently estimated if the common trend assumption is valid. The common trend assumption requires that states that did and did not implement mobility restrictions would display a common trend in satisfaction with democracy in absence of the implementation of the mobility restrictions. Clearly, this is a counterfactual situation and, hence, not testable. However, the estimates of $\gamma_{2\tau}$ in Equation 1 are informative about differential pre-trends between the treatment and control group. And the absence of differential pre-trends can make us confident that the common trend assumption is valid. In Section 5., we find strong support for the validity of the assumption of common trends. In consequence, we are confident that our estimates of the effect of mobility restrictions on individuals' satisfaction with democracy are consistent.

5. Results

A. Main results

The implementation of the mobility restriction increased individuals' satisfaction with democracy significantly. In Figure 3, we show the evolution of the effect in the weeks after the treatment. Each point in Figure 3 corresponds to the coefficient estimate on the interactions between the treatment group indicator and the indicators for the calendar weeks, as displayed in Equation 1.

The coefficient estimates for the weeks before the implementation of the mobility restriction are all close to zero and statistically indistinguishable from zero. Thus, we are confident that the common trend assumption is not violated. Further, the estimates for the coefficients in Figure 3 suggest that the mobility restriction increased individuals' satisfaction with democracy by 12% to 19% of a standard deviation, when controlling for individual characteristics.

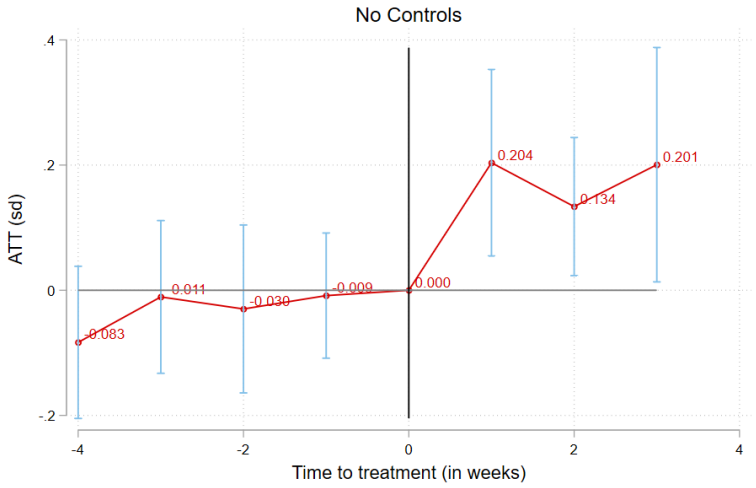
The average effect of the mobility restriction over the three weeks of the mobility restriction is 16.9% of a standard deviation. Table 2 displays the results that are associated with the estimation of Equation 2. Column (1) of Table 2 contains only week and state indicators as controls, column (2) also includes our full set of individual level controls and column (3) includes the contemporaneous seven day-incidence of COVID-19 at the state level. Column (4) of Table 2 additionally includes sample indicators. The effect of the mobility restriction, including no additional controls beside week and state indicators show that the effect of the mobility restriction on satisfaction with democracy is about 16.3% standard deviations of the control group in the pre-treatment period. Including our full set of individual level controls causes the estimand to increase by about 1.4 percentage points of a standard deviation, displayed in column (2) of Table 2. Controlling for the contemporaneous seven-day incidence of COVID-19 causes the estimate of the treatment effect to increase to 17.8% of a standard deviation, displayed in column (3) of Table 2. In our most demanding specification, displayed in column (4) of Table 2, with additional sample controls included, the result indicates that the mobility restrictions during the first wave of the COVID-19 pandemic in Germany increased satisfaction with democracy by about 16.9% of a standard deviation. We conclude that the coefficient of interest stays very robust to the inclusion of various controls.

B. Mechanisms: Exposure to socialism and the household composition matter

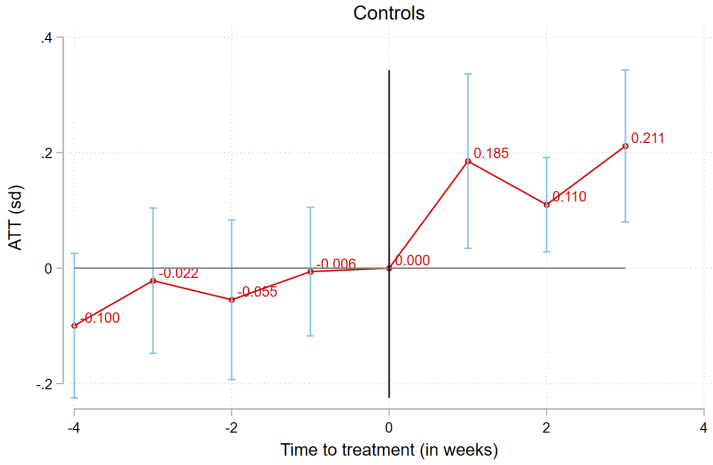
Why does the mobility restriction increase satisfaction with democracy? Economic theory suggests that citizens support mobility restrictions if expected benefits exceed expected costs. Expected benefits plausibly depend on individual's health status and family composition. Also expected costs may depend on household composition, with parents taking into account also the effects on their children, and on psychological costs of government restriction of civic liberties.

Exposure and adaptation to authoritarian regimes might reduce push-backs against restricting civil liberties. However, one could also expect that people who made negative experiences with

Figure 3: Event study analysis



(a) Without controls



(b) With controls

Notes: Figure 3a and 3b display the estimates for the event study-analysis of the effect of mobility restrictions on satisfaction with democracy depicted in Equation 1 without and with our full set of individual and regional level controls, respectively. The blue vertical bars correspond to associated 95% confidence intervals, based on standard errors that are clustered at the state level.

Table 2: Main results

	Satisfaction with Democracy			
Variables	(1)	(2)	(3)	(4)
Policy × Post	0.163	0.177	0.178	0.169
	(0.053)	(0.053)	(0.043)	(0.045)
State FE	yes	yes	yes	yes
Week FE	yes	yes	yes	yes
Individual controls		yes	yes	yes
Incidence			yes	yes
Sample control				yes
Observations	9,496	9,496	9,496	9,496
R-squared	0.087	0.120	0.121	0.127

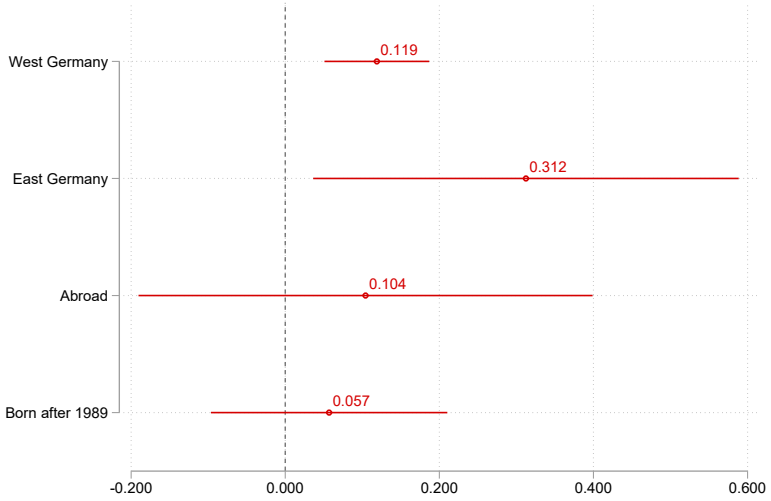
Notes: Table 2 displays the results associated with the estimation of Equation 2. The table displays the coefficient estimates and the associated standard errors on the interaction of the treatment and post-policy indicator. Column (1) includes a full set of week and state indicators. Column (2) builds on column (1) and includes our full set of individual control variables. Column (3) builds on column (2) and includes a full set of sample indicators. Column (4) builds upon column (3) and includes also the local COVID-19 incidence. The associated standard errors, clustered at the state level.

Table 3: Heterogeneities with respect to location in 1989

	(1)
Baseline	0.119
	(0.0347)
Location 1989 (Bl: West Germany	
East Germany	0.194
	(0.135)
Abroad	-0.0147
	(0.145)
Born after 1989	-0.0619
	(0.0938)
State FE	yes
Week FE	yes
Individual controls	yes
Incidence	yes
Sample control	yes
Observations	9,069
R-squared	0.160

Notes: Table 3 displays the results associated with the estimation of β_1 in Equation 2, whereby the term is interacted with group indicators. The results thus show heterogenous effects of the lockdown on satisfaction with democracy across locations of respondents in 1989. Baseline refers to West Germany. The associated standard errors are clustered at the state level.

Figure 4: Effects for various locations in 1989



Notes: The coefficient plot shows point estimates and 95%-confidence bands from estimating Equation 2, fully interacted with the three locations of respondents in 1989. The results show that individuals who lived in East Germany significantly increase their satisfaction with democracy due to the mobility restrictions by 35.1%, while the effect is smaller in size (8.7%) and for individuals who lived in West Germany. As the sub-samples for those who lived abroad and those who were born after 1989 are small, confidence intervals are too large to allow for any conclusions on these groups.

authoritarianism would be even more sensitive to civil liberty violations. The unique German history and its division into East and West Germany after the Second World War allows us to investigate how exposure to an authoritarian regime moderates our effect of interest.

Individuals in East Germany were living in a socialist state with high surveillance. Individuals in West Germany, i.e, the region which always belonged to the FRG, were living in a liberal democracy. Therefore, individuals in East and West Germany were exposed to highly different institutions until German reunification in 1989. We measure this exposure by forming four sub-samples: Individuals who were living in East Germany, West Germany, and abroad in 1989, and individuals who were born after 1989. We continue our heterogeneity analysis along this dimension. Table 3 shows that the effect is only 11% of a standard deviation for individuals who lived in West Germany in 1989. The effect for individuals who resided in East Germany is 19 percentage points larger, and overall amounts to 31% of a standard deviation, as depicted in Figure 4. Due to the small sample of individuals who grew up in the former GDR, the effect difference is not significant. The effect for individuals born after German reunification is similar to those who were socialized in West Germany. Clearly, our main finding is driven by individuals who were living in East Germany. It

appears that this group does not get so easily concerned about restrictive crisis interventions and even strongly approves them in the context of an emerging global health crisis. This result extends the findings of Alesina and Fuchs-Schündeln (2007), who explored how the GDR regime formed individuals' preferences for the size of the state. The authors find that exposure to the socialist regime changed individuals' preferences towards more pro-state and interventionist attitudes.

In the remaining heterogeneity analysis we explore different effect sizes for various demographic groups. In Table 4, we distinguish by gender, age, education, migration background, household composition, and employment type. We find no gender differences in the effect of mobility restrictions on satisfaction with democracy. Turning to age, we find suggestive evidence that older individuals show a larger effect of the mobility restriction on satisfaction with democracy. This can be seen in column (2) of Table 4, in which we distinguish between individuals who are younger than 30, between 31 and 60, and older than 60 years. However, the age disparity mostly stems from individuals who were raised in the former GDR. Regarding educational differences of the effect of mobility restrictions on satisfaction with democracy, displayed in column (3) of Table 4, we find no differences in the effects between individuals who have no academic degree and individuals with an academic degree. Similarly, we also find no differences in the effect sizes between individuals who have no migration background and a migration background, as displayed in column (4) of Table 4.

We find significant differences between individuals who live in a household with children and those individuals who do not live in households with children. This is displayed in column (5) of Table 4. For individuals who do not live in households with children, the coefficient on the interaction and the respective group indicator indicates a difference of about 15 percentage points of a standard deviation, which is statistically different from zero at a ten percent level of significance. For individuals who live in a household with children, we observe that the corresponding effect size is close to zero and not statistically significantly different from zero. The lack of an effect of the mobility restriction among individuals who live in households with children could be caused by the disproportional burden families were carrying during the first lockdown at the onset of the COVID-19 pandemic in Germany.

Further, we find suggestive evidence that the occupational status is associated with the effect size of mobility restrictions on satisfaction with democracy. This result is displayed in column (6) of Table 4. In column (6) of Table 4, we distinguish between individuals who are self-employed

and individuals who are employees. Clearly, among self-employed individuals, we observe a null-effect. In contrast, the coefficient estimate on the interaction of our treatment indicator and the indicator for being employee suggests that the effect difference to about 12.7 percentage points of a standard deviation, compared to self-employed individuals. However, we cannot reject the null hypothesis of no differential effect among employees. A null effect among self-employed individuals is consistent with the observation that self-employed individuals were disproportionately affected by the COVID-19 pandemic and associated policy measures compared to employees (Caliendo et al., 2022; Graeber et al., 2021). In consequence, we would expect the self-employed to be less satisfied with the intervention, compared to employees.

As the pandemic went on, evidence emerged that individuals with some preconditions face a higher risk of a severe progression of COVID-19. However, vulnerable health groups had not yet been clearly identified at the very beginning of the pandemic, the period of our study. Indeed, we do not find conclusive evidence that individuals in worse health before the pandemic consistently display larger estimates for the effect of the mobility restriction on satisfaction with democracy, as shown in Table 5. There is some suggestive evidence that individuals who are obese show larger effect sizes compared to individuals who are not obese, as displayed in column (1) of Table 4. Turning to vitality, we find evidence that individuals who have lower vitality display larger effect sizes than individuals who do have at least a medium level of vitality, as depicted in column (2) of Table 5. In contrast, for self-rated health and worries about one own's health, as depicted in columns (3) and (4) of Table 5, we fail to find any differences in the effect sizes.

Overall, the heterogeneity analysis has shown that in times of high insecurity and a diffuse health threat, individuals did not show much differences in their reaction to the enforced mobility restrictions based on their demographic and health characteristics. Instead, it seems that the effect mostly varies across individuals depending on their preferences for state intervention that have been formed during the postwar period when Germany was split into two distinct states with contrasting political systems in place.

C. Party preferences

The alarming situation at the beginning of the pandemic demanded competent crisis management and reliance on science and experts. According to Guriev and Papaioannou (2022), populist leaders

Table 4: Heterogeneities with respect to demographic characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline	0.165	0.0569	0.172	0.147	0.0730	0.0191
	(0.0604)	(0.0781)	(0.0782)	(0.0587)	(0.0533)	(0.166)
Gender (Bl: Male)						
Female	0.0135					
	(0.0506)					
Age (Bl: 18 to 30)						
31 to 60 years		0.00191				
		(0.0619)				
Above 60 years		0.246				
		(0.195)				
Education (Bl: No academic degree)						
Academic degree			-0.00106			
			(0.109)			
Migration Background (Bl: No)						
Migration background				0.0426		
				(0.0941)		
HH Composition (Bl: Children in hh)						
Childless household					0.151	
					(0.0835)	
Employment (Bl: Self-employed)						
Employers, Workers, Civil Servants						0.127
						(0.184)
State FE	yes	yes	yes	yes	yes	yes
Week FE	yes	yes	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes	yes	yes
Incidence	yes	yes	yes	yes	yes	yes
Sample control	yes	yes	yes	yes	yes	yes
R-squared	0.131	0.151	0.112	0.135	0.137	0.134
N	9,496	9,496	8,649	9,496	9,496	5,783

Notes: Table 4 depicts heterogenous effects of the lockdown on satisfaction with democracy across various demographic groups. In the order of columns, baseline refers to male, age between 18 and 59, no academic degree, no migration background, household with children, self-employed. The overall sample includes individuals that are at least 18 years old. The sample in column (6) only includes employed and self-employed individuals and is thus a sub-sample. The associated standard errors are clustered at the state level.

Table 5: Heterogeneities with respect to health characteristics

	(1)	(2)	(3)	(4)
Baseline	0.0969	0.0498	0.178	0.166
	(0.0570)	(0.0852)	(0.0424)	(0.0505)
BMI (Bl: Not top decile)				
Top decile	0.417			
	(0.209)			
Vitality (Bl: High vitality)				
Low vitality		0.150		
		(0.0799)		
Self-rated health (Bl: Very good or good)				
Poor or bad			-0.0358	
			(0.105)	
Health worries (Bl: At most some)				
Lots				0.0250
				(0.0827)
State FE	yes	yes	yes	yes
Week FE	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes
Incidence	yes	yes	yes	yes
Sample control	yes	yes	yes	yes
R-squared	0.144	0.138	0.150	0.147
N	7,638	7,651	8,860	8,841

Notes: Table 4 depicts heterogenous effects of the lockdown on satisfaction with democracy across various health groups. In the order of columns, baseline refers to not top decile BMI, high vitality, very good or good safe-rated health, and no or some health worries. The associated standard errors are clustered at the state level.

caused more polarization and handled the situation worse. To study the case of Germany, we investigate how the stay-at-home orders affected the support for parties at the center and the extreme ends of the political spectrum. So far, our analysis was centered on satisfaction with democracy, a measure of support with the prevailing institutions. In the following analysis, we focus on the individuals' change in the party preferences in response to the stay-at-home orders. For this, we apply our main specification, displayed in Equation 2, to individual level indicators for one's party preference, as described in Section 3.. The results are displayed in Table 6. The introduction of the stay-at-home orders increased the preference for the center, or mainstream, parties by about 7 percentage points. The results suggest that about 2.2 percentage points of this change stem from far-right parties, including the populist "AfD". Moreover, 4.4 percentage points stem from other parties or individuals who did not indicate any party affiliation.[7] The stay at home orders did not change the individuals' affiliation with the party "Die Linke", the far left party.[8]
These findings suggest that approval of mainstream parties increased due to the evidence-based crisis interventions. Of course, the pandemic lasted way longer than our study period of the initial weeks of the pandemic and large political polarization on topics such as mask-wearing or vaccinations followed in the months after.

The pandemic has caused strong partisan divides also in other countries. For example, several studies show for the US that partisanship is the strongest predictor of health behavior, approval of pandemic-related policies, and personal beliefs about the risk of an infection (Allcott et al., 2020; Canes-Wrone et al., 2020; Gadarian et al., 2021).

D. Robustness

Too few clusters. The estimate of our coefficient of interest remains statistically different from zero if we rely on the wild cluster bootstrap-procedure for our inference. In Table 2, we cluster our standard errors at the state level, of which we have 16 in Germany. Since the asymptotic behavior of clustered variance-covariance matrices relies on the number of clusters converging towards infinity, statistical inference based on the standard errors in Table 2 could be misleading (Cameron et al., 2008; Cameron and Miller, 2015). The wild cluster bootstrap procedure has been

[7] Since 53% in this category correspond to individuals that do not have any clear party preference.

[8] Note that these are only suggestive results. In fact, since these changes correspond to net flows, more complicated changes of affiliations are imaginable.

Table 6: Party affiliation

	Party affiliation			
Variables	Far Left	Far Right	Mainstream	Other
Policy × Post	-0.005	-0.022	0.070	-0.044
	(0.006)	(0.007)	(0.013)	(0.012)
State FE	yes	yes	yes	yes
Week FE	yes	yes	yes	yes
Individual controls		yes	yes	yes
7-day lagged incidence			yes	yes
Sample control				yes
Observations	9,606	9,606	9,606	9,606
R-squared	0.037	0.027	0.130	0.109

Notes: The table shows estimates from a difference-in-differences estimation. The treatment group consists of individuals who lived in states that passed stay-at-home orders around March 23, 2020, and the control group consists of individuals who lived in states without this intervention. Far left refers to "Die Linke", far right to AfD and NPD, mainstream to FDP, Greens, SPD, and CDU/CSU, and other to not having a clear preference. Individual controls include gender, migration background, urban/rural, household income, age and age^2.

shown to converge faster than other procedures and thus delivers cluster-robust standard errors (Cameron et al., 2008). Results of the wild cluster bootstrap estimation corresponding to our main estimation, are shown in Figure 5. According to Figure 5, the 95% confidence interval, based on the wild-cluster bootstrap procedure, does not include the zero.

Other policy measures. We also find that our results do not change when we control for other policy measures implemented around the same time as stay-at-home orders. In Section 2., we argued that all other policies to stop the spread the disease COVID-19 were implemented mostly simultaneously across all states in Germany. To show that those other policies indeed to not threaten our identification strategy, we repeat our main analysis as displayed in Section A., but augment our model in Equation 2 with a full set of indicators indicating the implementation and strength of the policies based on the data base provided by Steinmetz et al. (2022). Considering all the other policies causes our estimate to increase to 25% of a standard deviation, as depicted in Appendix Table 12, Column (5).

Placebo analysis. Another endogeneity concern could be that seasonal variation in satisfaction with democracy correlates with the intervention. To address this point, we conduct a placebo analysis in the years before 2020. Table 10 displays the results if we estimate Equation 2 in the

year 2005, 2010 and 2016. For this, we simulate the policy at exact the same date in the previous years. The results in column (1) to (3), *e.g.*, the placebo years, are never larger than four percent of a standard deviation and insignificant throughout. Column (4) in Table 10 displays our main result for comparison; the original estimate amounts to 16.9% of a standard deviation, as displayed in column (4) of Table 10.

Controlling for the pre-policy incidence of COVID-19. The estimates remain qualitatively unchanged when we control for the pre-policy COVID-19 incidence. Hypothetically, the level of the incidence could have determined the timing and strength of the set of policies to contain the spread of the virus. At the same time, Yam et al. (2020) showed that support for the government was increasing with the daily confirmed total number of COVID-19 cases across 11 developed economies, including Germany and the US. Therefore, we replicate our main analysis, but control for the pre-policy COVID-19-incidence. The results show that controlling for the incidence does not alter our results.

Split sample analysis for East and West Germany. One more concern could be that belonging to the treatment group tends to overlap with being one of the "new states of Germany" that belonged to the former German Democratic Republic. In this case, the effect could be driven by German political history rather than by the mobility intervention. Therefore, we run the main DiD design separately for East German and West German states only, leveraging the fact that both parts have states in the treatment and the control group. Appendix Table 13 shows that coefficients remain close to the ones from the main analysis. However, the sample from East Germany is comparatively small and the estimate is thus not significant.

Placebo tests for party preferences. Table 14 displays the results for a placebo analysis on party approval. For this, we implemented placebo stay at home-orders in the years 2016 to 2019, basically replicating our empirical design without actual stay at home orders happening in these years. For most estimations, the point estimates are significantly smaller in magnitude and show opposite signs. Exceptions are the years 2019 and 2018 for the far right parties and 2019 and 2016 for the mainstream parties. However, the estimates are relatively small in comparison to or main results, displayed in the first row of Table 14.

6. Conclusion

When a government faces a severe crisis, it may need to implement policies that would be unpopular or even unacceptable in normal times to protect the population. Interventions should strike a balance between effectiveness and respect for civil liberties. We find that in the early moments of the COVID-19 pandemic, implementing mobility restrictions at the state level increased satisfaction with democracy. This suggests a clear overall popular approval of the restrictions, despite their interference with the freedom of movement. In a more fine-grained analysis, we show that support for the intervention depended on whether the respondent had lived in the socialist East Germany before the German reunification. Our results relate to Alesina and Fuchs-Schündeln (2007), who find that individuals adapt their preferences for state intervention when being exposed to socialism. Furthermore, we observe that the intervention increased support for mainstream parties and reduced support for the far-right.

The increase of the satisfaction with democracy that we observe in response to the introduction of mobility restrictions is best explained by theories alluding to individuals' beliefs or attitudes towards the state's capacity to mitigate the threat. Two theories that are consistent with the empirical evidence we provide are system justification theory and cultural evolutionary models (Yam et al., 2020). System justification theory states that individuals are motivated to justify the way things are, even if the societal system affects their self-interest. Threats are important triggers of system justification beliefs, leading to increased support towards government or authorities. This in turn reduces individuals' uncertainty and the perceived threat (Yam et al., 2020). Alternatively, cultural evolutionary models posit that adherence to group norms and support for group leaders can preserve group unity and prosociality in times of threat (Yam et al., 2020). Both theories are consistent with the timing of the effects, as suggested by our event study.

It is important to highlight that we analyzed early stages of the pandemic, and our results should not be extrapolated to public support for mobility restrictions at later stages, when vaccination became widely available. At the beginning, uncertainty was very high and ambiguity widespread. At the same time, the government and the opposition agreed on how to address the pandemic. Once vaccinations became available, the trade-off facing societies changed, and the case for restrictions became considerably weaker. Applied to other crises, our findings suggest that at the time of

unknown threats, citizens may not only passively accept but actively expect their leaders to pursue policies to protect them, even at considerable short-term costs.

References

Alesina, A. and N. Fuchs-Schündeln (2007). Good-bye Lenin (or not?): The effect of communism on people's preferences. *American Economic Review 97*(4), 1507–1528.

Algan, Y., D. Cohen, E. Davoine, M. Foucault, and S. Stantcheva (2021). Trust in scientists in times of pandemic: Panel evidence from 12 countries. *Proceedings of the National Academy of Sciences 118*(40), e2108576118.

Allcott, H., L. Boxell, J. Conway, M. Gentzkow, M. Thaler, and D. Yang (2020). Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic. *Journal of Public Economics 191*, 104254.

Alsan, M., L. Braghieri, S. Eichmeyer, M. J. Kim, S. Stantcheva, and D. Y. Yang (2023a). Civil liberties in times of crisis. *American Economic Journal: Applied Economics 15*(4), 389–421.

Alsan, M., L. Braghieri, S. Eichmeyer, M. J. Kim, S. Stantcheva, and D. Y. Yang (2023b). The health of democracies during the pandemic: Results from a randomized survey experiment. *AEA Papers and Proceedings 113*, 572–576.

Baekgaard, M., J. Christensen, J. K. Madsen, and K. S. Mikkelsen (2020). Rallying around the flag in times of COVID-19: Societal lockdown and trust in democratic institutions. *Journal of Behavioral Public Administration 3*(2).

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Bol, D., M. Giani, A. Blais, and P. J. Loewen (2021). The effect of COVID-19 lockdowns on political support: Some good news for democracy? *European Journal of Political Research 60*(2), 497–505.

Caliendo, M., D. Graeber, A. S. Kritikos, and J. Seebauer (2022). Pandemic depression: COVID-19 and the mental health of the self-employed. *Entrepreneurship Theory and Practice*, 10422587221102106.

Callen, M., M. Isaqzadeh, J. D. Long, and C. Sprenger (2014). Violence and risk preference: Experimental evidence from Afghanistan. *American Economic Review 104*(1), 123–148.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Cameron, A. C. and D. L. Miller (2015). A practitioner's guide to cluster-robust inference. *The Journal of Human Resources 50*(2), 317–372.

Canes-Wrone, B., J. T. Rothwell, and C. Makridis (2020). Partisanship and policy on an emerging issue: Mass and elite responses to covid-19 as the pandemic evolved. *Available at SSRN 3638373*.

Dinesen, P. T. and M. M. Jæger (2013). The effect of terror on institutional trust: New evidence from the 3/11 madrid terrorist attack. *Political Psychology 34*(6), 917–926.

Edwards, G. C. and T. Swenson (1997). Who rallies? The anatomy of a rally event. *Journal of Politcs 59*(1), 200–212.

Fuchs-Schündeln, N., D. Krueger, A. Ludwig, and I. Popova (2022). The long-term distributional and welfare effects of covid-19 school closures. *The Economic Journal 132*(645), 1647–1683.

Gadarian, S. K., S. W. Goodman, and T. B. Pepinsky (2021). Partisanship, health behavior, and policy attitudes in the early stages of the covid-19 pandemic. *Plos one 16*(4), e0249596.

Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schröder, and J. Schupp (2019). The German socio-economic panel (SOEP). *Journal of Economics and Statistics 239*(2), 345–360.

Graeber, D., A. S. Kritikos, and J. Seebauer (2021). COVID-19: a crisis of the female self-employed. *Journal of Population Economics 34*(4), 1141–1187.

Guriev, S. and E. Papaioannou (2022). The political economy of populism. *Journal of Economic Literature 60*(3), 753–832.

Jakiela, P. and O. Ozier (2019). The impact of violence on individual risk preferences: Evidence from a natural experiment. *Review of Economics and Statistics 101*(3), 547–559.

Kriner, D. L. (2006). Examining variance in presidential approval: The case of FDR in World War II. *Public Opinion Quarterly 70*(1), 23–47.

Malmendier, U. and S. Nagel (2011). Depression babies: Do macroeconomic experiences affect risk taking? *Quarterly Journal of Economics 126*(1), 373–416.

Mazza, J. and M. Scipioni (2022). The brief rally around the flag effect of COVID-19 in europe. Technical report, European Commission.

Merkley, E., A. Bridgman, P. J. Loewen, T. Owen, D. Ruths, and O. Zhilin (2020). A rare moment of cross-partisan consensus: Elite and public response to the COVID-19 pandemic in canada. *Canadian Journal of Political Science/Revue canadienne de science politique 53*(2), 311–318.

Mueller, J. E. (1973). War, presidents, and public opinion. *University Press of America*.

Parker, S. L. (1995). Toward an understanding of rally effects: Public opinion in the persian gulf war. *Public Opinion Quarterly 59*(4), 526–546.

Perrin, A. J. and S. J. Smolek (2009). Who trusts? Race, gender, and the September 11 rally effect among young adults. *Social Science Research 38*(1), 134–145.

Roth, C. and J. Wohlfart (2018). Experienced inequality and preferences for redistribution. *Journal of Public Economics 167*, 251–262.

Schraff, D. (2021). Political trust during the Covid-19 pandemic: Rally around the flag or lockdown effects? *European Journal of Political Research 60*(4), 1007–1017.

Shapiro, C. R. (1991). The rally phenomenon in public opinion. In *Assessing the President*, pp. 45–80. Stanford University Press.

Steinmetz, H., V. Batzdorfer, J. Scherhag, and M. Bosnjak (2022). The ZPID lockdown measures dataset for Germany. *ZPID Science Information Online 20*(1).

Wollebæk, D., B. Enjolras, K. Steen-Johnsen, and G. Ødegård (2012). After utøya: How a high-trust society reacts to terror—trust and civic engagement in the aftermath of July 22. *PS Political Science & Politics 45*(01), 32–37.

World Health Organization (2020). WHO Director-General's opening remarks at the media briefing on COVID-19 - 11 March 2020. {https://www.who.int/director-general/speeches/detail/who-director-general-s-opening-remarks-at-the-media-briefing-on-covid-19—11-march-2020}, Accessed: 2022-11-15.

Yam, K. C., J. C. Jackson, C. M. Barnes, J. Lau, X. Qin, and H. Y. Lee (2020). The rise of COVID-19 cases is associated with support for world leaders. *Proceedings of the National Academy of Sciences 117*(41), 25429–25433.

A Data appendix

Table 7: Variable description

Variable	Description
Satisfaction with democracy	Responses on eleven point Likert-scale ranging from zero "completely dissatisfied" to ten "completely satisfied."
Education (2019)	Contains the categories of primary, secondary, and tertiary education.
Gender	Equal to one if respondent is female and zero if respondent is male.
Age	Difference between survey year and year of birth.
Migration background	Indicator that is equal to one if respondent either migrated themselves or has parents who migrated to Germany.
Urbanity (2019)	Equal to one if a BIK region has 50,000 inhabitants or more. BIK regions are based on a classification provided by the company BIK Aschpurwis + Behrens GmbH and commonly used by administrative authorities, designed to capture commuting areas.
Household type (2019)	Zero if children in respondent's household, one if childless household.
Household net income (2019)	Total household net income from 2019.
COVID-19 incidence	Number of COVID-19 cases, reported by RKI, normalized by the state population.

Table 8: Stay-at-home order schedules

State	Lockdown in 2020
Bavaria	21 March - 28 April
Berlin	23 March - 21 April
Brandenburg	23 March - 22 April
Saarland	21 March - 28 April
Sachsen	23 March - 19 April
Sachsen-Anhalt	23 March - 28 April

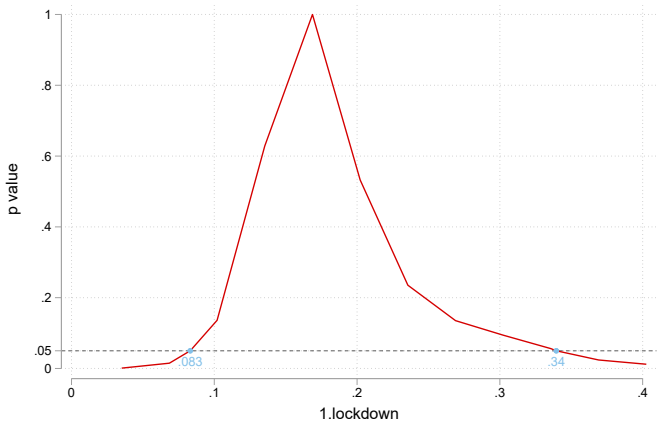
Notes: The sample covers the period from Feb 26, 2020 to April 19, 2020. Source: ZPID.

Table 9: Predictors of incidence around March 20

Variables	(1) Incidence	(2) Incidence
Treated state		1.480
		(0.821)
East	-12.007	-12.690
	(0.571)	(0.740)
Migration background	-0.636	-0.618
	(0.843)	(0.844)
Age	0.015	0.015
	(0.020)	(0.020)
Female	0.508	0.486
	(0.641)	(0.640)
Children in household	0.497	0.498
	(0.732)	(0.734)
Secondary education	0.090	0.047
	(1.808)	(1.798)
Tertiary education	1.019	0.937
	(1.892)	(1.881)
Household income	0.000	0.000
	(0.000)	(0.000)
Urban	0.932	0.759
	(0.699)	(0.715)
Constant	16.459	16.275
	(2.001)	(1.991)
Observations	1,505	1,505
R-squared	0.146	0.148

Notes: Column (1) shows the results of regressing the state-level incidence within 5 days prior to and after March 20, 2020, on various individual characteristics and a dummy for a state to belong to the former East, whereby Berlin is counted towards the West. Column (2) adds a dummy for belonging to the treatment group (a state that implemented a stay-at-home order).

Figure 5: Wild Cluster Bootstrap Test



Notes: Figure **??** displays the confidence curve based on the Wild-Cluster Bootstrap procedure following the estimation of Equation 2.

Table 10: Placebo results

	Satisfaction with Democracy			
Variables	2005	2010	2016	2020
Policy × Post	0.002	-0.032	0.012	0.169
	(0.096)	(0.044)	(0.075)	(0.045)
State FE	yes	yes	yes	yes
Week FE	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes
Incidence	yes	yes	yes	yes
Sample control	yes	yes	yes	yes
Observations	6,823	6,446	7,015	9,496
R-squared	0.122	0.127	0.105	0.127

Notes: Table 10 displays the results associated with the estimation of Equation 2, performed in various years. The table displays the coefficient estimates and the associated standard errors on the interaction of the treatment and post-policy indicator. All columns include a full set of week and state indicators as controls, whereby the pre-pandemic incidence is set to zero. Column (1) displays the result for the year 2005, column (2) displays the result for 2010, column (3) displays the result for 2015 and column (4) displays the result for 2020. The associated standard errors, clustered at the state level, are displayed in parentheses.

Table 11: Other Outcomes in 2020

	Satisfaction with ...							
Variables	Life	Family	Leisure	Income	Work	Sleep	Health	Democ.
Policy × Post	0.088	0.048	0.010	0.099	0.022	0.084	0.057	0.169
	(0.046)	(0.045)	(0.038)	(0.049)	(0.051)	(0.036)	(0.033)	(0.045)
State FE	yes	yes	yes	yes	yes	yes	yes	yes
Week FE	yes	yes	yes	yes	yes	yes	yes	yes
Individual contr.	yes	yes	yes	yes	yes	yes	yes	yes
Incidence	yes	yes	yes	yes	yes	yes	yes	yes
Sample control	yes	yes	yes	yes	yes	yes	yes	yes
Observations	9,595	9,484	9,575	9,399	6,167	9,591	9,588	9,496
R-squared	0.037	0.024	0.065	0.095	0.024	0.028	0.099	0.127

Notes: Table 11 displays the results associated with the estimation of Equation 2 on satisfaction with respect to various domains. The table displays the coefficient estimates and the associated standard errors on the interaction of the treatment and post-policy indicator. All coefficients are in standard deviations of the pre-treatment period in the control group. The specification includes all controls that have been used for the main results. The associated standard errors, clustered at the state level, are displayed in parentheses.

Table 12: Controlling for other pandemic policies

	Satisfaction with Democracy				
Variables	(1)	(2)	(3)	(4)	(5)
Policy × Post	0.169	0.164	0.182	0.235	0.240
	(0.045)	(0.045)	(0.039)	(0.080)	(0.078)
Child-related restrictions		yes	yes	yes	yes
Consumption restrictions			yes	yes	yes
Social restrictions				yes	yes
Other restrictions					yes
State FE	yes	yes	yes	yes	yes
Week FE	yes	yes	yes	yes	yes
Individual controls	yes	yes	yes	yes	yes
Incidence	yes	yes	yes	yes	yes
Sample control	yes	yes	yes	yes	yes
Observations	9,496	9,496	9,496	9,496	9,496
R-squared	0.127	0.128	0.129	0.129	0.130

Notes: Table 12 displays the results associated with the estimation of Equation 2, while step-wise including other non-pharmaceutical measures as controls. Column (1) includes the full specification from the main estimation. Column (2) adds child-related restrictions (closure of kindergartens and daycare, schools, and playgrounds). Column (3) adds consumption restrictions (closure of non-essential shops, i.e. gastronomy, petrol stations, banks, barbershops, bookstores, and zoo among others. Column (4) adds social restrictions such as entry bans to Germany, travel restrictions, the prohibition of political demonstrations, and the prohibition to meet someone in public who does not belong to the same household. Column (5) adds other measures, including the obligation to wear a mask in public, closure of churches, mosques, synagogues, and temples. The table displays the coefficient estimates and the associated standard errors on the interaction of the treatment and post-policy indicator. The associated standard errors, clustered at the state level, are displayed in parentheses.

Table 13: Split samples by former East and West Germany

	Satisfaction with Democracy	
	West	East
Policy × Post	0.136	0.193
	(0.029)	(0.136)
State FE	yes	yes
Week FE	yes	yes
Individual controls	yes	yes
Incidence	yes	yes
Sample control	yes	yes
Observations	7,671	1,825
R-squared	0.086	0.149

Notes: The left column depicts the result for running the main estimation from Equation 2 on the sub-sample of individuals who are living in former West Germany (including Berlin), the right column depicts the results for individuals in former East Germany. West and East Germany refer to the territories of the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR), respectively.

Table 14: Placebo lockdown coefficients in non-pandemic years

	Party Preference			
Variables	Far Left	Far Right	Mainstream	Other
2020	-0.005	-0.022	0.070	-0.044
	(0.006)	(0.007)	(0.013)	(0.012)
2019 (placebo)	-0.003	-0.016	0.040	-0.021
	(0.011)	(0.008)	(0.016)	(0.020)
2018 (placebo)	0.008	-0.018	-0.008	0.019
	(0.010)	(0.005)	(0.019)	(0.020)
2017 (placebo)	-0.001	-0.004	-0.026	0.031
	(0.006)	(0.005)	(0.023)	(0.027)
2016 (placebo)	0.000	-0.001	0.044	-0.043
	(0.006)	(0.011)	(0.023)	(0.026)

Notes: Far left refers to "Die Linke", far right to AfD and NPD, mainstream to FDP, Greens, SPD, and CDU/CSU, and other to not having a clear preference. Individual controls include gender, migration background, urban/rural, household income, age and age^2.