



# Protest policing and public perceptions of police. Evidence from a natural experiment in Germany

Christof Nägel & Amy Nivette

To cite this article: Christof Nägel & Amy Nivette (2023) Protest policing and public perceptions of police. Evidence from a natural experiment in Germany, Policing and Society, 33:1, 64-80, DOI: [10.1080/10439463.2022.2064857](https://doi.org/10.1080/10439463.2022.2064857)

To link to this article: <https://doi.org/10.1080/10439463.2022.2064857>



© 2022 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group



[View supplementary material](#)



Published online: 29 Apr 2022.



[Submit your article to this journal](#)



Article views: 2899



[View related articles](#)



[View Crossmark data](#)



Citing articles: 2 [View citing articles](#)

# Protest policing and public perceptions of police. Evidence from a natural experiment in Germany

Christof Nägel and Amy Nivette

Department of Sociology, Utrecht University, Utrecht, The Netherlands

## ABSTRACT

Understanding when, to what extent, and in which contexts high-profile police actions influence trust in and legitimacy of the police is important because police perceptions are associated with cooperation, compliance and, eventually, trust in the state itself. The current study uses a quasi-experimental design to assess changes in public attitudes toward the police after the violent police dispersal of a protest movement against a new railway station project in Stuttgart on 30 September 2010. We found little to no change in several dimensions of perceptions of police and legitimacy, specifically measures of trust in police, moral alignment, procedural fairness, and obligation to obey the police. However, respondents interviewed after the event saw the police as more unduly influenced by political pressure. The results suggest that the impact of high-profile incidents of police violence may depend on institutional context, media response, and post-incident reconciliation strategies.

## ARTICLE HISTORY

Received 1 November 2021  
Accepted 7 April 2022


## KEYWORDS


Protest policing; natural experiment; European Social Survey; perceptions of police

## Introduction

Does excessively violent protest policing change public attitudes toward the police? Understanding when, to what extent, and in which contexts high-profile police actions influence attitudes towards the police is important because police perceptions are associated with public cooperation (Tyler and Huo 2002), compliance with the law (Tyler and Jackson 2014), criminal behaviour (Eisner and Nivette 2013, Dawson 2018) and, eventually, trust in the state itself (Jeong and Han 2020). The current study assesses changes in public attitudes toward the police after the so-called 'Black Thursday' in Germany; the violent police dispersal of a protest movement against a new railway station project in Stuttgart (known as Stuttgart 21) on 30 September 2010. We take advantage of the overlap between the fieldwork period of the European Social Survey [ESS] 2010 (15 September 2010–3 February 2011) and the protests. This special type of natural experiment, known as an 'unexpected event during survey design' [UESD] (Muñoz *et al.* 2019), allows us to estimate causal effects of incidents of police violence on public opinion.

This study aims to contribute to knowledge on protest policing and public attitudes in two ways. First, we examine the impact of 'vicarious experiences' (Weitzer 2017) on attitudes using a case study of police violence in the context of mass protests in Stuttgart, Germany. Given that the vast majority of research on perceptions of police, and high-profile police incidents in general, has been conducted in the United States (Lasley 1994, Jefferis *et al.* 1997, Tuch and Weitzer 1997, Weitzer 2002, White *et al.* 2018, Kochel 2019, Kochel and Skogan 2021, Reny and Newman 2021), (see: Hohl

**CONTACT** Christof Nägel  [c.r.e.nagel@uu.nl](mailto:c.r.e.nagel@uu.nl)

 Supplemental data for this article can be accessed <https://doi.org/10.1080/10439463.2022.2064857>.

© 2022 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group

This is an Open Access article distributed under the terms of the Creative Commons Attribution-NonCommercial-NoDerivatives License (<http://creativecommons.org/licenses/by-nc-nd/4.0/>), which permits non-commercial re-use, distribution, and reproduction in any medium, provided the original work is properly cited, and is not altered, transformed, or built upon in any way.

*et al.* 2012, Thomassen *et al.* 2014, Kääriäinen *et al.* 2016, Curtice 2021, Nägel and Lutter 2021), we lack a more general understanding of change in public attitudes towards the police in other national and institutional contexts (Roché and Oberwittler 2017, Sahin *et al.* 2017, Sato 2017). Compared to the US and other European countries, trust in police in Germany is generally high (Guzy and Hirtenlehner 2015). Previous research suggests that countries with a high average level of trust in the police are less likely to experience a severe legitimacy crisis even after a high-profile cases of police misconduct (Thomassen *et al.* 2014, Kääriäinen *et al.* 2016). The incident in Stuttgart, therefore, provides an important opportunity to evaluate changes in public attitudes towards the police in a 'high-trust' context.

Second, this study overcomes previous methodological limitations to estimate causal effects of incidents on public attitudes. Previous research on the effects of high-profile policing incidents on attitudes toward the police is either limited by (1) a lack of causal inference perspectives (Graziano 2018), (2) a lack of nationally representative survey data (Lasley 1994, Kaminski and Jefferis 1998, Hohl *et al.* 2012, White *et al.* 2018, Kochel 2019, Kochel and Skogan 2021) and/or (3) a lack of appropriate measuring instruments (Nägel and Lutter 2021). These issues call into question the internal, external and construct validity of previous findings. To our knowledge, the only papers to address all three issues are those by Curtice (2021) assessing violent police repression in Uganda and Reny and Newman (2021) assessing the impact of the killing of George Floyd and subsequent protests on public attitudes towards the police. The present study utilises a unique quasi-experimental design, high quality survey data, and extensive measurement instruments on perceptions of police in order to address these most common limitations. In addition, previous research suggests that opinion-mobilizing events tend to affect public opinion only briefly (Zaller 1992, Ares and Hernández 2017), including attitudes towards the police (Nägel and Lutter 2021, Reny and Newman 2021). The fact that the focal event occurred early in the fieldwork period means that we are also able to investigate the evolution of potential effects over time.

## High-profile incidents and attitudes toward the police

Historically, event-based studies on perceptions of police first gained traction after the Rodney King beating in Los Angeles in March 1991. The incident not only sparked an extensive discussion about police violence against members of ethnic minorities, but also motivated researchers to assess the effect of police violence on public opinion (Lasley 1994, Jefferis *et al.* 1997, Kaminski and Jefferis 1998, Weitzer 1999, 2002, Weitzer and Tuch 2004, Graziano *et al.* 2010). Interest in these 'indirect effects' of police misconduct and violence, or so-called 'vicarious experiences' (Weitzer 2017), was renewed following the death of Michael Brown in August 2014 (Kochel 2019). Most recently, the killing of George Floyd in May 2020 further fuelled the debate both from a societal and academic point of view (Reny and Newman 2021). The underlying question was whether and to what extent these incidents resulted in lasting effects on public attitudes towards the police. Prior research suggests that attitudes towards the police depend on the quality of treatment during interactions, whereby unfair, disrespectful, and unequal treatment can lead to more negative perceptions of police legitimacy (Mazerolle *et al.* 2013, Tyler *et al.* 2014, Walters and Bolger 2019). Perceptions of police legitimacy concern 'whether a power-holder is justified in claiming the right to hold power over other citizens' (Bottoms and Tankebe 2012). However, some events may result in little change in public opinion as they are filtered differently through (social) media outlets, resulting in varying degrees of public awareness and reactions (Cheng 2021). In addition, incidents can be closely intertwined with protests, which can amplify public awareness, contestation, and ultimately polarisation (Reny and Newman 2021).

The results from these American-based studies told a largely similar story: in general, incidents of police violence led to significant but short-lived decreases in trust in police. However, the results were not unambiguous. For example, one study found little evidence that the death of Freddie Gray was associated with more negative attitudes toward police in Baltimore (White *et al.* 2018).

Likewise, studies on police misconduct and public opinion outside of the United States tend to show more heterogeneous effects. On the one hand, a study on the impact of the 2011 London disorder showed little to no effect on attitudes towards the police (Hohl *et al.* 2012), whereas analyses of police misconduct in Finland (Kääriäinen *et al.* 2016) and police failures during a terrorist attack in Norway (Thomassen *et al.* 2014) suggest that trust may even increase following certain controversial incidents. On the other hand, Nägel and Lutter (2021) found that trust in police significantly declined following a serious incident of police violence coupled with protests in France. Similar to studies in the US, the authors found that the negative effect was relatively short-lived for the general population, but more persistent for ethnic minorities.

## Protest policing and attitudes toward the police

Several incidents coincide with large-scale protests, where police are often under close public and media scrutiny (Rosie and Gorringer 2009, Greer and McLaughlin 2010, Maguire and Oakley 2020). Protest policing refers to police treatment and control of protesters (Della Porta 1999) which may involve a range of peaceful (e.g. communication, differentiation) or more aggressive (e.g. arrest, assault, tear gas, water cannons) tactics (Maguire and Oakley 2020). While the public tends to be more supportive of peaceful policing tactics, recent research suggests that public perceptions of protest policing depends on situational characteristics and the goals of the protest (e.g. Black Lives Matter, pro-confederate monuments, see Metcalfe and Pickett 2021). Specifically, in situations where protestors were armed, committed violence and property damage, and interrupted traffic, respondents were more likely to fear protestors and support aggressive policing tactics.

Public perceptions of protest policing also depend on police conduct and treatment during interactions with protestors (Lai Hang Hui and Chi Yan Au 2014, Perry *et al.* 2017, Newburn *et al.* 2018, Curtice 2021). Analysing the effects of protest policing on perceptions of police legitimacy in Hong Kong, Au and Hui proposed three mechanisms by which protest policing might influence attitudes: (1) degree of fairness and expediency in dealing with public processions; (2) the interaction between the police and protestors on site and the responsiveness in accommodating demands by protestors; and the (3) degree of neutrality displayed towards groups with different political views. (2014, p. 225). Generally, these mechanisms coincide with notions of fairness, voice, and neutrality or impartiality, which comprise key elements of procedural justice policing (Tyler and Blader 2003, Mazerolle *et al.* 2013, Trinkner and Tyler 2016). The use of excessive force or repressive tactics against protestors violates these principles and can damage the trustworthiness of police (Perry *et al.* 2017, Curtice and Behlendorf 2021, Nägel and Lutter 2021). Research that has used disaggregated measures of perceptions of police and legitimacy has found that the use of excessive force and violence during protests is associated with declines in perceptions of police fairness, feelings of obligation to obey, and normative alignment with police (Curtice 2021, Metcalfe and Pickett 2021).

In addition, the use of excessive force during protests can lead to political backlash among observers (Curtice and Behlendorf 2021), as police are often considered representatives of the state there to uphold the status quo (Earl *et al.* 2003, Maguire and Oakley 2020, Wang *et al.* 2020, Wood 2020, Channing 2021). In interviews conducted with participants of the 2012 'Occupy' protests in Israel, one protester stated that 'when the PPU's [paramilitary policing units] are involved in policing the protest event, they give you this feeling – we do not work for you. You are the enemy [and] we have to keep you quiet' (quoted in Perry *et al.* 2017, p. 620). Police presence at protests, and in particular the use of force against protestors, can therefore damage perceptions of impartiality alongside trustworthiness.

However, there is a crucial difference to be made between the attitude of protestors themselves and the general public who were not physically involved in the protests. Thus, while it is plausible that attitudinal dynamics might spill over to the population as a whole when the public identifies with the protestors or the incidents can be considered to be 'high-profile', in other, more specific cases, it is possible that this relates only to the participants of protests themselves.

Taken together, previous research suggests that high-profile incidents of police violence during protests should result in more negative perceptions of police, and that these effects are typically short-lived. More specifically, we expect that the use of violence will lead to lower perceptions of police fairness, moral alignment, obligation to obey, trust in the police, and political impartiality (Research Question 1). With the exception of political impartiality, these perceptions (or very similar items) were found to decrease after protests and arrests in Uganda (Curtice 2021) or protests following police misconduct in France (Nägel and Lutter 2021). Since the potential interference of politicians in the police triggered a great controversy after the incident in Stuttgart (the focal case), which was even followed by the implementation of an official committee of enquiry investigating who could be held attributable for the police violence (Feltus and Schnepfer 2011), we included political impartiality in this analysis as well.<sup>1</sup>

In addition, we expect that any changes in attitudes towards the police will return to pre-event levels in the long-term (Research Question 2). This study examines these questions using a case study of excessive police violence during protests in Stuttgart, Germany.

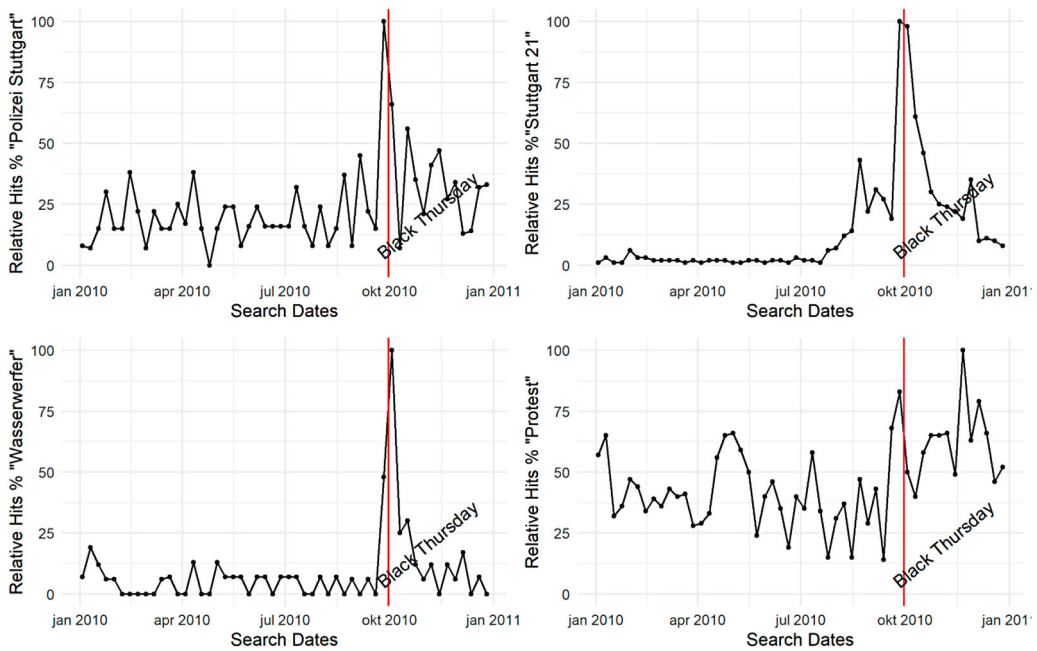
## Contextual setting

In order to understand the effects of the so called 'Black Thursday' it is necessary to explain the broader political context of the planned railway station in Stuttgart and the motivation for its opposition. The broader aim of the infrastructure project is to make changes to the Stuttgart railroad station. Among other things, Stuttgart's main station is to be converted from an overground terminus to an underground transit station. Since planning for Stuttgart 21 began, there have been protests against the project. Major criticisms concern for example the high costs, low economic viability, a lack of democratic legitimation and citizen participation, the difficulty in access for travellers, endangerment of the environment (through possible water contamination), the lack of protection of historical monuments, and general deficiencies in planning.

Although the planning of the project began in 1996, it was not until the police operation in the Stuttgart Schlossgarten on 30 September 2010, that the project came to the attention of the broader German public. On the morning of 30 September 2010, many protesters occupied the Schlossgarten in Stuttgart. Around 600 police officers acted with unusual force against allegedly over 1000 demonstrators who represented a broad cross-section of society (including for example pensioners and school children).<sup>2</sup> According to information from citizens' initiatives and park protectors, more than 100 people were injured, including minors. Four demonstrators had serious eye injuries, and according to the protest organisers, more than 400 protesters suffered eye irritation. A 66-year-old man almost completely lost his eyesight as a consequence of being hit head-on in the eyes with a water cannon.<sup>3</sup> Following the events, the police justified their actions by claiming that the aggression had originated from the demonstrators. However, this account was refuted by police videos which included details on the timing of events. Later that day, work began on felling the first trees in the park to make way for the new station.

30 September 2010 was hence remembered as 'Black Thursday'.<sup>4</sup> On 20 November 2010, about 10,000 people protested to demand clarification regarding the background and legal reappraisal of the police operation. Every year around September 30th, rallies, demonstrations, and similar events commemorate those affected by the police action. Thousands of people gather for these events.<sup>5</sup>

Figure 1 complements the qualitative characterisation of the political circumstances surrounding the event with a quantitative examination of related relative Google search trends for the year 2010 in Germany. Internet search data is considered a valuable resource to gain insights into public problem awareness (Muñoz *et al.* 2019). Google trends in particular are considered valid indicators of issue salience (Mellon 2014) and have been analysed alongside traditional survey data in many previous UESD studies (Ares and Hernández 2017, Nägel and Lutter 2021, Reny and Newman 2021, Thompson 2021). Google trends data can also serve as a proxy for political interest in the absence of survey data (Barrie 2020).



**Figure 1.** Evolution of weekly Google search trends before and after the event.

The search trends demonstrate an interesting pattern: while the theme ‘Wasserwerfer’ (water cannon) peaks in the week after the event, the more general search themes ‘Polizei Stuttgart’ (‘police Stuttgart’) and ‘Stuttgart 21’ and reaches the highest relative frequency in the week of the event. ‘Protest’ searches are elevated after the event, but also show considerable movement before. In fact, except for ‘water cannon’, every search term shows some interest even in the weeks before the event. This provides an important clue for the present study, as it shows that the police were already associated with the infrastructure project before 30 September 2010, but the connection to excessive violence (water cannon) only appeared to be the focus of public interest after the event.

While interest in ‘Protest’ appears to be more persistent, interest in the other three search themes quickly decreases. This pattern suggests that even though police involvement was a very salient issue after 30 September 2010, the uptick of interest quickly faded. This trend is in line with the notion that the police are a ‘Teflon Service’ (Reiner 2010) where even major scandals resolve quickly. This exploratory analysis furthermore highlights the importance of examining the existing survey data more closely with respect to interview timing. Nevertheless, these Google trends demonstrate that ‘Black Thursday’ can be considered a significant event in the German public ‘psyche’.

## Methods<sup>6</sup>

### Identification strategy

To answer the research questions, we make use of the UESD identification strategy, which is a quasi-experimental research design that can analyse shifts in public opinion after unexpected events that occur during the fieldwork of survey programmes. Although this research design purports to permit causal inferences, these are subject to two key identification assumptions that need to be examined in detail: excludability and temporal ignorability (Muñoz *et al.* 2019). The first one assumes that the interview date, as an instrument  $Z$ , exclusively affects the outcome  $Y$  through the event  $X$ . The occurrence of other events influencing  $Y$  must therefore be excluded. The temporal ignorability

assumption is concerned with the assignment to pre- or post-treatment groups, which should be as good as random. Muñoz *et al.* (2019) provide good practice advice for UESD that we adhere to as closely as possible in order to obtain unbiased estimates and efficient standard errors. Since these robustness checks are technical in nature, they are discussed in detail in the accompanying supplemental material.

### **Data, descriptive statistics, and model specification**

The European Social Survey [ESS]<sup>7</sup> is a cross-national survey programme that measures respondents' attitudes, beliefs and behaviour patterns in European countries on the basis of probability samples gathered through face-to-face interviews (European Social Survey 2011). We use the German subset of the ESS5 which includes respondents interviewed in all parts of Germany (not just within Stuttgart) between 15 September 2010, and 3 February 2011. Inferences are there for valid for general German population. Detailed information concerning the ESS can be found in the supplementary materials (section A1.1).

The current study examines several operationalizations of attitudes towards the police that variously reflect public perceptions of fairness, moral alignment, obligation to obey, trust in police, and neutrality or impartiality. The variable *procedural fairness* was measured using three items that asked respondent how often police in their country (1) treat people with respect, (2) make fair, impartial decisions and (3) explain their decisions when asked ( $\alpha = .60$ ). Responses were measured using a 4-point Likert-type scale ranging from 1 'not at all often' to 4 'very often'. *Moral alignment* was measured using three items that capture to what extent the police (1) 'have the same sense of right and wrong as me', (2) 'stand up for values that are important to people like me' and whether the respondent (3) 'generally support[s] how the police act' ( $\alpha = .78$ ). Responses were measured on a 5-point Likert-type scale ranging from 1 'agree strongly' to 5 'disagree strongly'. The obligation to obey scale consists of 3 items measuring the respondent's felt duty to (1) 'back decisions made by the police, even if disagree', (2) 'do what police say, even when don't understand or agree' and (3) 'do what police say even if treated badly' ( $\alpha = .86$ ). Responses are measured on a scale ranging from 0 'not at all my duty' to 10 'completely my duty'. Factor analyses confirm a one-factor solution for each construct (see Appendix A1.2, Table A1). The variable *trust in police* was measured using a single item that asks respondents to what extent they trust the following institutions (the police) on a scale from 0 'no trust at all' to 10 'complete trust'. Finally, the measure of political impartiality was measured using a single item that asks to what extent the respondents agree or disagree with the statement that 'the decisions and actions of the police are unduly influenced by pressure from political parties and politicians'. Responses ranged from 1 'agree strongly' to 5 'disagree strongly'. Our choice of variables is depended on the composition of the ESS5. The latent constructs (i.e. procedural fairness, moral alignment, and obligation to obey) are based on the methodological suggestions put forward by Jackson *et al.* (2010).<sup>8</sup>

Five socio-demographic variables were included in order to assess and compare the composition of control and treatment groups: age (years), gender (1 = female), education years, income, and ethnic minority. Education was measured years of completed education. Income was measured as household's total net income in deciles. We applied the natural logarithm to account for the skewed distribution. Ethnic minority captures whether the respondent is a member of an ethnic minority group (1 = yes). Except for income, we only included observations with complete cases on all variables to keep the number of observations constant between different analyses. Results were virtually the same when we follow a listwise missing value strategy (see Tables A5 and A6 in the Supplementary Materials). Following previous research using the same design (Minkus *et al.* 2019), we avoided survey weights since they might further bias results in this particular design. Still, results were robust to applying the two possible ESS weights (design weights and post-stratification weights, see Tables A7 – A10 in the appendix).

**Table 1.** Descriptive statistics.

Statistic	Control group					Treatment group						Mean $\Delta$
	<i>N</i>	Mean	St. Dev.	Min	Max	Statistic	<i>N</i>	Mean	St. Dev.	Min	Max	
Political Impartiality	371	2.431	0.946	1	5	Political Impartiality	1,875	2.294	0.94	1	5	-0.137***
Trust in the Police	396	6.644	2.219	0	10	Trust in the Police	1,979	6.797	2.227	0	10	0.153
Procedural Fairness	350	2.938	0.432	1	4	Procedural Fairness	1,710	2.957	0.499	1	4.333	0.019
Moral Alignment	384	3.924	0.595	1.667	5	Moral Alignment	1,909	3.856	0.645	1	5	-0.068*
Obligation to Obey	386	6.631	2.293	0	10	Obligation to Obey	1,937	6.649	2.275	0	10	0.018
Age	396	52.571	18.127	15	90	Age	1,987	48.623	17.36	15	97	-3.948***
Female	396	0.48	0.5	0	1	Female	1,987	0.484	0.5	0	1	0.004
Education	396	12.571	3.494	2	23	Education	1,987	13.406	3.352	0	28	1.237***
Income	396	4.987	2.833	1	10	Income	1,987	5.582	2.711	1	10	0.595***
Ethnic Minority	396	0.975	0.157	0	1	Ethnic Minority	1,987	0.957	0.204	0	1	-0.018*

\*\*\* $p < .01$ , \*\* $p < .05$ , \* $p < .1$



As can be seen in [Table 1](#), the data appear to be rather unbalanced between the treatment and control group: the treatment group was on average significantly younger, more educated, had a higher income, and was less likely to belong to an ethnic minority. As Muñoz *et al.* (2019) argue, controlling for these covariate imbalances can reduce bias. Furthermore, a narrower bandwidth is beneficial to achieve conditional independence since observations and their outcomes are more likely to be independent from treatment assignment. This, however, is a trade-off since a narrow bandwidth increases variance, allows exclusively for estimation of very local effects, and may even lead to type two errors since the treatment effect might need some time to evolve. The best strategy, therefore, was to choose a bandwidth with maximal balance between the groups. For this study, this meant a range of 15 days before and after the event. The only remaining imbalances were a younger ( $M_c = 52.12$  years,  $M_t = 48.54$  years;  $p = .008$ ) and more educated ( $M_c = 12.55$  education years,  $M_t = 13.31$  education years;  $p = .003$ ) post-treatment sample. To further reduce model dependence, we present findings from a ‘full bandwidth sample’ in the appendix (Supplementary Materials A1.3, Table A4). This specification again did not change our main results.

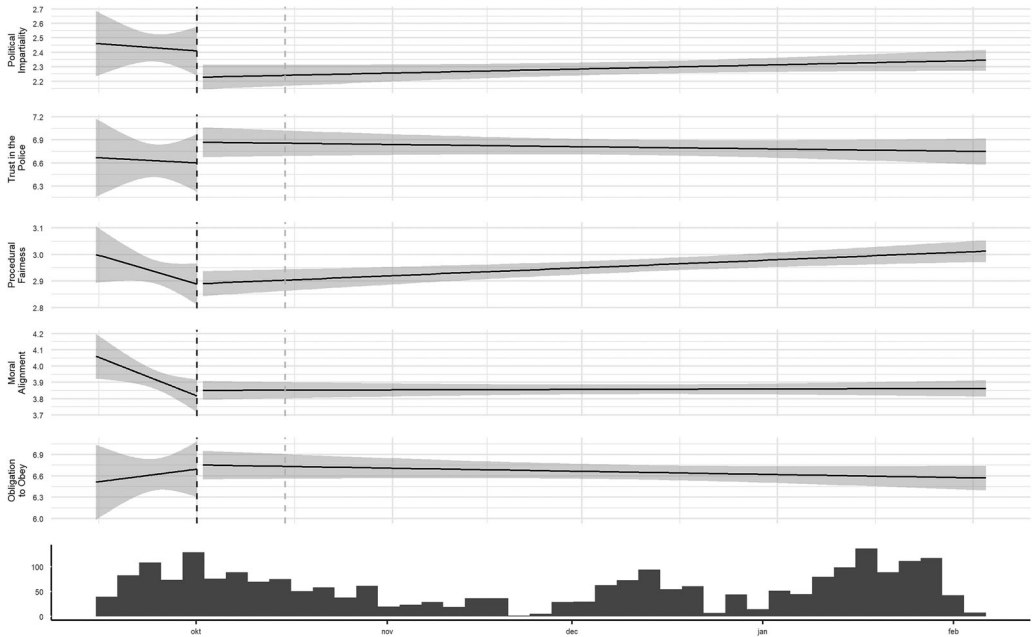
The  $\pm 15$ -day sample period can be used to answer RQ 1, namely that people interviewed after the event generally reported less positive attitudes towards the police. To address the second research question, we ran  $5 \times 18$  regressions for each outcome by subsequently adding one week of observations to the post-treatment time-window (starting with a time window of one week). The control group stayed the same across all models (15 days before the event). This way, we could track the trajectories of potential treatment effects over time (see Ares and Hernández 2017 for a similar approach).

## Results

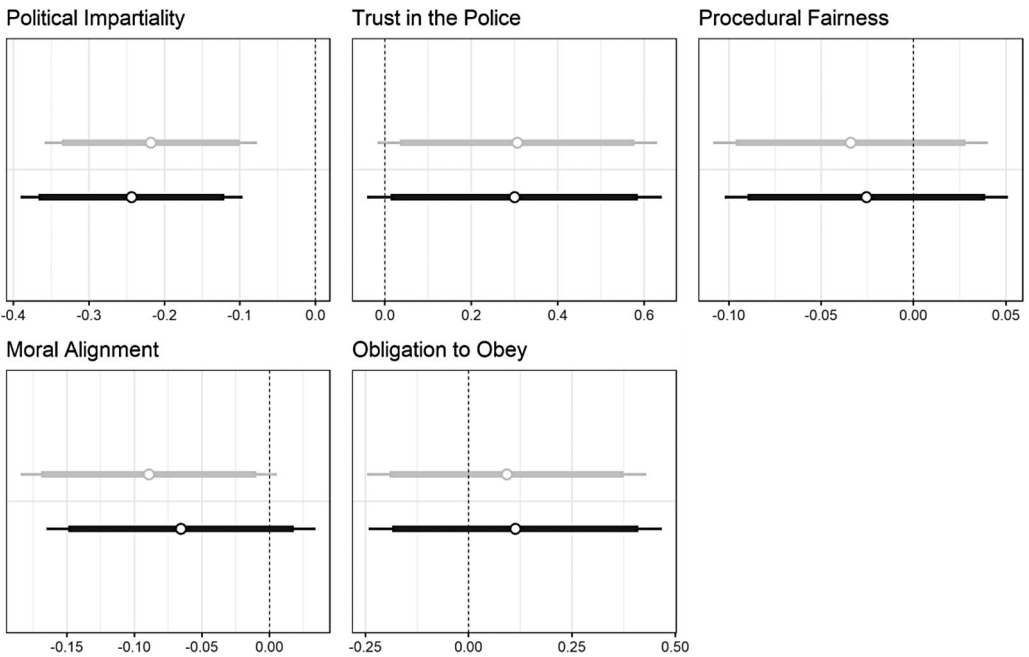
Our analysis section is structured as follows: First, in order to answer RQ 1, we will provide a graphical depiction of the main outcomes over time. We will then present results of models regressing each outcome only on the binary event indicator (treatment effect) and on the treatment indicator as well as all variables used in the analysis (control variables and state fixed effects). To address RQ 2, we will provide a visualisation of the development of the treatment effect on each outcome that is significantly affected by the event in the fashion outlined above. Finally, we will present those results from the sensitivity analysis that do not clearly underline the robustness of our results (all results from sensitivity analysis can be accessed in the appendix).

[Figure 2](#) provides a visual representation of this quasi-natural experiment, resembling a classic regression discontinuity (rd) plot.<sup>9</sup> The dashed line represents the 15-day bandwidth of the ‘maximum balanced local sample’. The histogram at the bottom of [Figure 2](#) represents the average number of interviews per day. Fluctuations in interview frequency could influence outcomes, but the results suggest that the number of interviews around the ‘cut-off’ were relatively stable and should provide enough power to run the analyses. The control group (before the event) consists of 424 (17.8%) respondents, whereas there are 1959 (82.2%) respondents in the treatment group. The outcome evolution is represented by two simple linear regression lines, one before and one after the event for each of the five outcomes, the predictor being the date of the interview. While there appears to be little level or trend change for the outcomes procedural fairness, moral alignment or obligation to obey, there are visible discontinuities after September 30 in the outcomes political impartiality and trust in the police. While the former drops right after the ‘cut-off’, the latter increases. It is important to note that there exist considerable differences between the pre- and post-event groups which might influence results. The naïve before-after-comparisons in [Figure 2](#) may therefore give biased estimates of a causal effect.

To achieve a more reliable assessment of effects of the event on attitudinal outcomes, we first regressed each outcome on the treatment variable in a baseline model and then included the covariates presented in [Table 1](#) as well as federal state fixed effects. Although this is not a panel data set, the fixed effects take into account that the same regions were surveyed before and after the event.



**Figure 2.** Outcome evolution after 'Black Thursday'. Histogram represents average number of interviews per day. Black dotted line is 1 October 2010 (One day after the event). Grey dotted line is 15 October 2010 and represents the shortened time window.



**Figure 3.** Effect of 'Black Thursday' on outcomes.

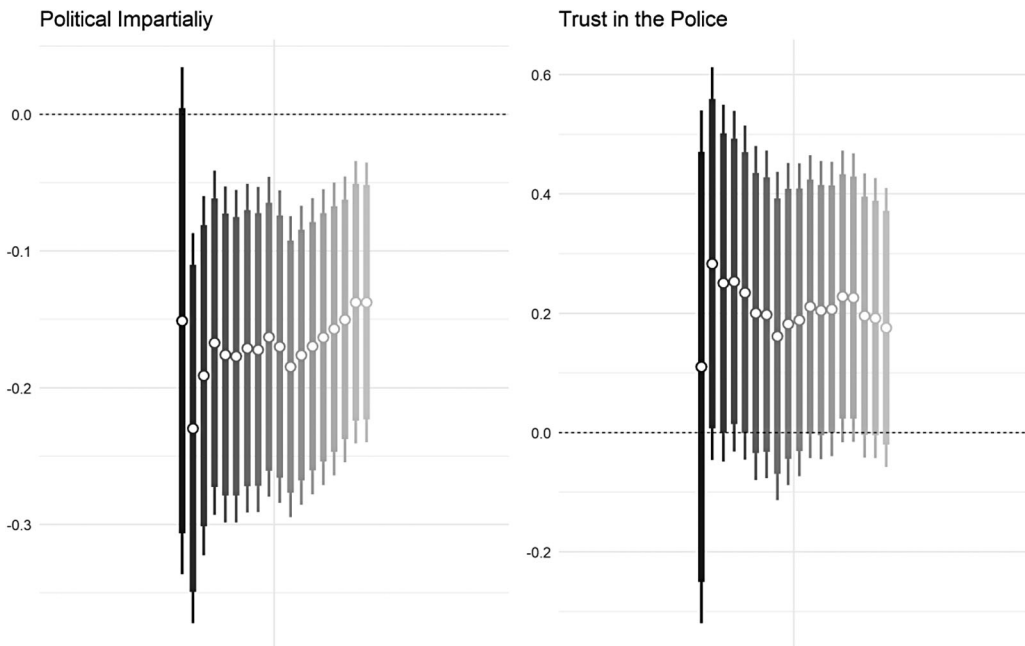
Note: Grey models summarise results from regressions that only include the treatment indicator. Black models summarise the results of models adjusting for all covariates listed in Table 1 as well as federal state fixed effects. Thick and thin lines are 90% and 95% confidence intervals, respectively.

This allows us to control for the effect of omitted variables that are constant over time but vary between states.<sup>10</sup>

Figure 3 summarises our findings for RQ 1. Both the baseline model and the model with controls and fixed effects show a significant decrease in perceived political impartiality of the police after the event with  $B = -.24$  and  $p = .001$  [95% CI =  $-.390, -.097$ ] in the long regression. This effect is substantial as it suggests a change of more than  $\frac{1}{4}$  of a standard deviation of the variable's pre-treatment distribution. The results thus suggest that respondents interviewed after the event were significantly more likely to perceive the police as being under unjust political pressure. Regressing the second outcome, trust in the police, on the treatment, the controls and state dummies revealed a treatment effect of  $B = 0.299$  ( $p = .084$ ), however, the 95% confidence interval does include zero [95% CI =  $-.041, .641$ ]. This result is noteworthy since it opposes the relationship we hypothesised. Concerning the outcomes procedural fairness, moral alignment and obligation to obey, none of the long regressions showed any effect that is significantly different from zero. Moral alignment appears to be negatively affected in the baseline model ( $B = -0.089$ ) but the effect was small, statistically significant only at the 10% level and mediated by inclusion of covariates and fixed effects. Hence, apart from the event's effect on political impartiality, none of our findings support the hypothesised relationships.

To address RQ2 we focus on the two outcomes that show some over-time variation which might be attributable to the event (political impartiality and trust in the police).<sup>11</sup> For both outcomes, we did so by re-running the regressions on the treatment effect and subsequently adding one week of observations at a time starting with a model of 7 days post-treatment and ending with a model 18 weeks after the event. Diminishing effect sizes could be an indication that any changes in public perceptions of police will return to pre-event levels in the long-term, especially when standard errors do not inflate at the same time. This specification also provides a convenient way of analysing whether findings could merely be driven by sample bandwidths. Results are summarised in Figure 4.

While the treatment effect on political impartiality is not significant in week one after the event (which might be a result of very limited power), it is greatest two weeks after the event and sharply



**Figure 4.** Regression models run on each weekly independent cross section for the outcomes 'Political Impartiality' and 'Trust in the Police'.

drops in the next two weeks while remaining significant at  $p < .05$ . This effect appears to stay relatively stable but significant for another seven weeks until it again decreases in the last weeks of the fieldwork period. As such, it appears as though the effect on political impartiality indeed diminishes over time, and that this decrease is non-linear.

The results on trust in the police are somewhat more difficult to interpret. While the figure clearly shows a decrease in the positive treatment effect, the treatment dummy falls in and out of significance even for the most liberal threshold of 10% in a seemingly random pattern. This suggests that any changes in trust over time are apparently little more than noise.

Summarising our results, it seems that the events of 'Black Thursday' only had a substantial and consistent (in terms of statistical significance) effect on perceptions of political impartiality. It is also the only treatment effect that shows a clear decreasing pattern over time, although it does not reach pre-event levels during the period we can observe in the survey. Trust in the police is, on average, somewhat elevated after the event and this effect also diminishes over time. However, this result is not robust to all model specifications. The analyses on the other outcomes clearly point in one direction: the event did not alter perceptions of procedural fairness, moral alignment or obligation to obey.

To increase the reliability of our estimates and our overall research design, we ran virtually all sensitivity analyses recommended in the literature (Muñoz *et al.* 2019), the results of which can be found in the Supplemental Material. In addition to general analytical choices like handling of missing values or survey choices, we addressed the assumptions related to UESD design. Specifically, we assessed placebo outcomes and pre-existing time trends, evaluated the UK as a placebo comparison, examined placebo-treatments at the empirical median of the control group, and analysed non-response patterns. While the results show that our results are in general robust, we want to highlight three sources of potential bias we encountered while running our sensitivity analyses. As has been discussed above, (1) our pre-post samples suffer from several imbalances in socio-demographic variables. To address this issue, we controlled for those covariates and chose an analysis window with minimal imbalances between those groups (as well as 18 time windows in total). However, in this sample, the post-treatment group still is slightly younger and more educated than the pre-treatment group. Additionally, (2) we identified a significant negative pre-existing time trend in the moral alignment outcome. This negative trajectory might also be the reason, why we see a small negative effect in this outcome in some specification that is significant at  $p < .1$ . Finally, (3) we identified a negative effect on both moral alignment and procedural fairness when splitting the control group at its empirical median (which is recommended in the rd literature) at both  $p < .1$  (the  $p$ -values exceed  $p < .1$  when covariates are included). While it is possible that those effects are a function of running many robustness analysis (i.e. type 1 error, or alpha error cumulation), we also consider non-linearities in those outcomes as a potential reason for these effects. A more detailed discussion on those issues, as well as some graphical representations can be found in the technical appendix (see sections on pre-existing time trends and placebo treatment at median of control group).

## Discussion

This study aimed to assess potential change in public attitudes toward the police after a major policing controversy during protests against a planned railway station in Stuttgart, Germany. We found little to no change in several dimensions of perceptions of police and legitimacy, specifically measures of moral alignment, procedural fairness, and obligation to obey the police. However, respondents interviewed after the event saw the police as more unduly influenced by political pressure. This effect was robust to various placebo specifications, to the temporal bandwidth of the analysis, and sizable across all modelling strategies. Surprisingly, people in the post-intervention group reported more trust in the police than respondents in the control group, but this result was not robust to all alternative time windows and model specifications. In the following discussion, we

address two main implications for research on high-profile events and public attitudes towards the police.

The results suggest that, by and large, the events of 'Black Thursday' had little impact on public perceptions of police in Germany. On the one hand, this contradicts previous research that has found significant changes in attitudes towards the police following high-profile incidents of police violence (Weitzer 2002, Kochel 2019, Reny and Newman 2021). On the other hand, the findings are consistent with research on high-profile events in 'high trust' societies (e.g. Finland, Norway, and to a lesser extent UK; Hohl *et al.* 2012, Thomassen *et al.* 2014, Kääriäinen *et al.* 2016), which found that attitudes showed little or even positive change following controversial incidents. This heterogeneity in effects may be due to differences in institutional contexts or event characteristics. For example, in 'high trust' societies where the trust in police is also high, public reactions to police misconduct may be more tolerant or only short-term. In these contexts, incidents of police violence also tend to be less common compared to countries like the United States or Brazil (Ceccato *et al.* 2018, Lawson 2019). By contrast, where incidents of police misconduct are situated in polarised contexts where the legitimacy of police is highly contested, high-profile incidents can have a lasting impact particularly among members of affected minority groups (Nägel and Lutter 2021, Reny and Newman 2021). However, in contexts where police violence is more frequent and support for the police is already low, these incidents may have relatively smaller or null effects (see e.g. White *et al.* 2018). Another explanation for the null effects on most outcomes could be the fact that police in Germany are characterised by a pronounced decentralisation. In the federalist state structure, there are 16 state police forces (Laenderpolizeien) in addition to the two national police forces, the Bundespolizei (Federal Police) and the Bundeskriminalamt (Federal Criminal Police Office). Even though the police units in Baden-Wuerttemberg were reinforced on the 'Black Thursday' by units from Bavaria, Rhineland-Palatinate, Hesse, and North Rhine-Westphalia as well as the federal police, it is quite possible that the respondents made a distinction between these police forces and the police in their own federal state and consequently remained broadly unchanged in their opinion towards the police. However, since we included a proximity moderator yielding null results (see Table A2 in the appendix), we believe that this interpretation is unlikely to hold as the effects should, in this case, have been more pronounced for people in Baden-Wuerttemberg.

Effects (or the lack thereof for most outcomes) may not be due to the protests themselves, but due to the subsequent media discussion that focused on the political implications of 'Black Thursday'. Shortly after the incident, there was an investigation by a committee of enquiry on whether the (conservative) CDU and (economically liberal) FDP-led state government could be held responsible for the excessive police violence, which helped to frame the incident as a political rather than a police scandal (Feltus and Schnepfer 2011). This development might even have influenced the subsequent federal state elections in the upcoming year, in which the CDU had to hand over government responsibility to a coalition of the social ecological Green party (Die Grünen) and the SPD (social democrats) for the first time after 58 years (Brettschneider and Schuster 2013).

In contrast to null effects on most outcomes, we found that the event changed public opinion about police impartiality, whereby respondents reported that the police were more likely to be unduly influenced by politics. This is in line with previous research that shows that police are often seen as tools of the state when present at protests (Newburn *et al.* 2018, Curtice and Behlendorf 2021). In particular, more aggressive actions taken during protests, such as the use of force and paramilitary-style policing, can lead to feelings of alienation among protesters (Perry *et al.* 2017). From a methodological standpoint, the significant effect on political impartiality could also be considered a 'manipulation check' (see Muñoz *et al.* 2019), to test whether respondents were exposed to the event in the sense of a natural experiment. Such an interpretation would underline reliability of null- or even contrary effects on the other outcomes.

Interestingly, while some argue that perceived political impartiality is a key element of police legitimacy (Tankebe 2013), the current findings show that changes in neutrality did not 'spill over' to influence changes in correlated constructs, such as procedural fairness. This, again, suggests

that the incident was perceived as a political rather than police crisis, and that the public likely makes some distinction between police actions in different contexts (i.e. protests vs on the street) and how these actions reflect on the police more generally. These effects may also depend on the extent and content of media coverage, which can play a role in shaping narratives about police misconduct and violence (Cheng 2021). More research is needed to understand how police actions during protests are differentially filtered through (social) media outlets, and to what extent this influences broader political and legal attitudes.

It is, of course, noteworthy to highlight a few important differences between our study and previously published research. Obviously, the institutional, cultural, political and media context in our study differs in many regards from the circumstances of George Floyd's murder in 2020 (Reny and Newman 2021), or even the shooting of Michael Brown in Ferguson in 2014 (Kochel 2019). This is also true for findings from Uganda that investigated how state repressions translates into public perceptions of police in an Eastern African country (Curtice 2021). All those studies find robust evidence of a negative effect on public perceptions. Differences in political and cultural context demonstrate why it is important that our study adds evidence from a European high trust setting. Indeed, our (null-) results mirror findings from countries with a (to a certain extent) comparable cultural and political background (See studies from Scandinavia; Thomassen *et al.* 2014, Kääriäinen *et al.* 2016)

Taken together, the impact of incidents of police violence on public attitudes may be moderated variously by media attention, the type of event, and/or the societal context in which police are embedded. For example, high-profile incidents of police violence may have an inverse-U relationship with trust in police, whereby effects are smaller in societies with very low or high trust in police. These processes are important to dissect in order to understand how and under what circumstances the public responds to such incidents, and what strategies police must take to effectively repair public opinion and mitigate losses of legitimacy over time (e.g. reconciliation, apologies, structural reform see O'Brien *et al.* 2019, McLean 2021). Future research should move beyond single-incident case studies to comparatively assess how and when high-profile incidents lead to changes in attitudes towards the police, accounting for prior societal characteristics as well as post-incident organisational and media responses.

### **Limitations and conclusions**

While there are several strengths to this study, notably the utilisation of a quasi-experimental design and multiple dimensions of perceptions of police, there are also a number of limitations to address. First, natural experiments are often praised for 'maximising' external validity, but the current study is nevertheless a single case study concerning a particular event in time on attitudes in Germany. More comparative research using natural experiments is needed in order to assess and establish general mechanisms that contribute to changes in attitudes. In addition, controlled experiments and qualitative research can complement existing research to describe the evolution of media coverage and police reactions and reconciliation following different events (see Cheng 2021).

Other limitations include those inherent to natural experiments in general and UESD in particular. Although we discuss and test the excludability and temporal stability assumption in close detail in the supplementary materials, it is still possible that our results could be driven or levelled by unobserved heterogeneity. Finally, as has been discussed in the UESD literature (Muñoz *et al.* 2019), there is an obvious one-sided compliance problem in UESD studies, since it is more difficult in this particular setting to be certain that respondents have effectively been exposed to the treatment, thus the effect should be treated as an Intent-to-treat, rather than an Average-treatment-effect. The interpretation of the political impartiality item as a pseudo-manipulation check can lend more confidence that respondents were aware of the events of the 'Black Thursday'.

Overall, the current paper demonstrates that public perceptions of police can be responsive to police actions, however, the results likely depend on societal and situational contexts. It is important to note that, although opinion-mobilizing effects might be short-lived (Nägel and Lutter 2021, Reny and Newman 2021), high-profile policing incidents can threaten the validity of measuring instruments. Researchers should pay close attention to fieldwork dates and overlap with potentially highly salient events that can influence attitudinal measures. In order to avoid bias stemming from these sources, it may therefore be advisable to choose short fieldwork periods or to examine the survey periods in close detail for possible confounding events.

## Notes

1. Police legitimacy is an elusive term, defined and assessed in a variety of ways by many different theoretical approaches (for an overview, see for example Worden and McLean 2017). Since measurement concepts differ, we want to stress that the outcome of this study should be considered *public perceptions of police*, rather than police legitimacy. However, we acknowledge that we use the concepts somewhat interchangeably, as previous research has done as well (see for example Curtice 2021, Nägel and Lutter 2021).
2. As described by Feltes and Schnepper (2011), the use of extreme violence (e.g. pepper spray, water cannons, etc.) by police officers against this broad cross-section of the population was completely unanticipated, unlike in previous clearly left-wing or right-wing demonstrations. This might be one of the reasons why the events caused such an immense political and media discussion.
3. See [https://en.wikipedia.org/wiki/Dietrich\\_Wagner](https://en.wikipedia.org/wiki/Dietrich_Wagner)
4. A major newspaper from Stuttgart provides a slide show of the events on the 'Black Thursday': <https://www.stuttgarter-zeitung.de/gallery.chronologie-zu-stuttgart-21-schwarzer-donnerstag-im-schlossgarten-param~1~0~0~13~false.114e1e4e-5f61-4523-8f8c-9a387733c0ed.html>.
5. A more detailed description of the events – but only in German – can be found on the Wikipedia article [https://de.wikipedia.org/wiki/Protest\\_gegen\\_Stuttgart\\_21#30.\\_September\\_2010:\\_R%C3%A4umung\\_des\\_Schlossgartens\\_%E2%80%93\\_%E2%80%9ESchwarzer\\_Donnerstag%E2%80%9C](https://de.wikipedia.org/wiki/Protest_gegen_Stuttgart_21#30._September_2010:_R%C3%A4umung_des_Schlossgartens_%E2%80%93_%E2%80%9ESchwarzer_Donnerstag%E2%80%9C). A detailed description in English can also be found here: <https://www.spiegel.de/international/germany/the-world-from-berlin-germany-shocked-by-disproportionate-police-action-in-stuttgart-a-720735.html>.
6. All survey data can be downloaded from the ESS webpage: <https://www.europeansocialsurvey.org/data/download.html?r=5> Google trends data can be assessed via the gtrendsR package in R or directly from the google trends webpage. All code to reproduce analysis and figures can be assessed via: <https://osf.io/3nczm/>.
7. The authors are not in any way responsible for fielding the survey. We want to stress the sensitive nature of surveys on policing. According to the ESS webpage, best practices and ethical guidelines were followed when collecting the data.
8. It should be noted that the political impartiality item we use ('the decisions and actions of the police are unduly influenced by pressure from political parties and politicians') did not load on one factor together with an item on police bribery, as suggested in the paper by Jackson et al.. Alongside the fact that we see no theoretical reason why perceptions of police corruption/bribery should be affected by the event, this gives us further empirical confidence that the item should be used 'as is', rather than within a latent construct.
9. We refrain from using a straightforward rd design because there are very few observations in the direct vicinity of the cut-point. Recent research suggests that rd may be an inappropriate method for inference when the post-intervention sample size is small (i.e. limited power) (Stommes et al. 2021).
10. This also controls for a possible east/west effect which should always be considered when using nationally representative data for Germany.
11. Models on the other outcomes (procedural fairness, moral alignment, obligation to obey) were run but are not shown as the effects remain constantly non-significant and non-substantial over time in these models.

## Disclosure statement

No potential conflict of interest was reported by the author(s).

## Funding

This work was supported by Nederlandse organisatie voor wetenschappelijk onderzoek [grant number: VI.Vidi.191.135].

## References

- Ares, M. and Hernández, E., 2017. The corrosive effect of corruption on trust in politicians: evidence from a natural experiment. *Research & politics*, 4 (2), 205316801771418. doi:10.1177/2053168017714185.
- Barrie, C., 2020. Searching racism after George Floyd. *Socius: sociological research for a dynamic world*, 6, 237802312097150. doi:10.1177/2378023120971507.
- Bottoms, A. and Tankebe, J., 2012. Beyond procedural justice: a dialogic approach to legitimacy in criminal justice. *The journal of criminal law and criminology*, 102 (1), 119–170.
- Brettschneider, F. and Schuster, W., 2013. *Stuttgart 21: Ein Grossprojekt zwischen Protest und Akzeptanz*. Wiesbaden: Springer VS.
- Ceccato, V., Melo, S.N., and Kahn, T., 2018. Trends and patterns of police-related deaths in Brazil. In: K. Carrington, R. Hogg, J. Scott, and M. Sozzo, eds. *The Palgrave handbook of criminology and the global south*. Springer International Publishing, 521–550. doi:10.1007/978-3-319-65021-0\_26.
- Channing, I., 2021. Policing extreme political protest: a historical evaluation of police prejudice. *Policing: A Journal of Policy and practice*, 14 (4), 900–915. doi:10.1093/polic/pay010.
- Cheng, T., 2021. Social media, socialization, and pursuing legitimation of police violence\*. *Criminology*, 1745-9125.12277. doi:10.1111/1745-9125.12277.
- Curtice, T.B., 2021. How repression affects public perceptions of police: evidence from a natural experiment in Uganda. *Journal of Conflict resolution*, 65 (10).
- Curtice, T.B. and Behlendorf, B., 2021. Street-level repression: protest, policing, and dissent in Uganda. *Journal of Conflict resolution*, 65 (1), 166–194. doi:10.1177/0022002720939304.
- Dawson, A., 2018. Police legitimacy and homicide: a macro-comparative analysis. *Social forces*. Advance Online Publication. doi:10.1093/sf/soy043.
- Della Porta, D., 1999. Protest, protesters, and protest policing. In: S. Tarrow, M. Giugni, D. McAdam, and C. Tilly, eds. *How Social movements matter* (NED-New edition, Vol. 10). University of Minnesota Press; JSTOR, 66–96. <http://www.jstor.org/stable/10.5749/j.ctttt706.9>.
- Earl, J., Soule, S.A., and McCarthy, J.D., 2003. Protest under fire? Explaining the policing of protest. *American Sociological review*, 68 (4), 581. doi:10.2307/1519740.
- Eisner, M. and Nivette, A., 2013. Does low legitimacy cause crime? A review of the evidence. In: J. Tankebe and A. Liebling, eds. *Legitimacy and criminal justice: an international exploration*. OUP Oxford, 308–325. doi:10.1093/acprof:oso/9780198701996.003.0016.
- European Social Survey. 2011. *European Social Survey (ESS), Round 5—2010*. doi:10.21338/NSD-ESS5-2010.
- Feltes, T. and Schnepfer, A., 2011. Stuttgart 21 oder: Wie Bürger, Polizei und Demokratie beschädigt wurden. In: B. Böniger, G. Haug, and U. Sickerl, eds. *Mit Kanonen auf Spatzen. Stuttgart 21 im Blickwinkel des Untersuchungsausschusses zum 30.9.2010*, 11–19.
- Graziano, L.M., 2018. News media and perceptions of police: a state-of-the-art-review. *Policing: an international journal*, 10 (2), 109. doi:10.1108/PIJPSM-11-2017-0134.
- Graziano, L., Schuck, A. and Martin, C., 2010. Police misconduct, media coverage, and public perceptions of racial profiling: an experiment. *Justice Quarterly*, 27 (1), 52–76. doi:10.1080/07418820902763046.
- Greer, C. and McLaughlin, E., 2010. We predict a riot?: Public order policing, new media environments and the rise of the citizen journalist. *British journal of criminology*, 50 (6), 1041–1059. doi:10.1093/bjc/azq039.
- Guzy, N. and Hirtenlehner, H., 2015. Trust in the German police: determinants and consequences for reporting behavior. In: G. Meško and J. Tankebe, eds. *Trust and legitimacy in criminal justice*. Springer International Publishing, 203–229. doi:10.1007/978-3-319-09813-5\_10..
- Hohl, K., Stanko, B., and Newburn, T., 2012. The effect of the 2011 London disorder on public opinion of police and attitudes towards crime, disorder, and sentencing. *Policing: a journal of policy and practice*, 7 (1), 12–20. doi:10.1093/polic/pas055.
- Jackson, J., et al., 2010. Trust in justice: notes on the development of European social indicators. *SSRN electronic journal*. doi:10.2139/ssrn.1717924.
- Jefferis, E.S., et al., 1997. The effect of a videotaped arrest on public perceptions of police use of force. *Journal of criminal justice*, 25, 381–389.
- Jeong, J. and Han, S., 2020. Trust in police as an influencing factor on trust in government: 2SLS analysis using perception of safety. *Policing: an international journal of police strategies & management*, 43 (4), 541–558. doi:10.1108/PIJPSM-11-2019-0172.
- Kääriäinen, J., Isotalus, P., and Thomassen, G., 2016. Does public criticism Erode trust in the police? The case of Jari Aarnio in the Finnish news media and its effects on the public's attitudes towards the police. *Journal of scandinavian studies in criminology and crime prevention*, 17 (1), 70–85. doi:10.1080/14043858.2016.1144315.
- Kaminski, R.J. and Jefferis, E.S., 1998. The effect of a violent televised arrest on public perceptions of the police. *Policing: an international journal of police strategies & management*, 21 (4), 683–706. doi:10.1108/13639519810241692.
- Kochel, T.R., 2019. Explaining racial differences in Ferguson's impact on local residents' trust and perceived legitimacy: policy implications for police. *Criminal justice policy review*, 30 (3), 374–405. doi:10.1177/0887403416684923.



- Kochel, T.R. and Skogan, W.G., 2021. Accountability and transparency as levers to promote public trust and police legitimacy: findings from a natural experiment. *Policing: an international journal*, (ahead-of-print). doi:10.1108/PIJPSM-04-2021-0062.
- Lai Hang Hui, D. and Chi Yan Au, R., 2014. Police legitimacy and protest policing: a case study of Hong Kong. *Asian Education and Development Studies*, 3 (3), 223–234. doi:10.1108/AEDS-07-2014-0030.
- Lasley, J.R., 1994. The impact of the Rodney king incident on citizen attitudes toward police. *Policing and society*, 3 (4), 245–255. doi:10.1080/10439463.1994.9964673.
- Lawson, E., 2019. TRENDS: police militarization and the use of lethal force. *Political research quarterly*, 72 (1), 177–189. doi:10.1177/1065912918784209.
- Maguire, E.R. and Oakley, M., 2020. Policing protests. lessons from the occupy movement, Ferguson & beyond: a guide for police. *Harry Frank Guggenheim Foundation*, 1–84.
- Mazerolle, L., et al., 2013. Procedural justice and police legitimacy: a systematic review of the research evidence. *Journal of experimental criminology*, 9 (3), 245–274. doi:10.1007/s11292-013-9175-2.
- McLean, K., 2021. Justice-restoring responses: a theoretical framework for understanding citizen complaints against the police. *Policing and society*, 31 (2), 209–228. doi:10.1080/10439463.2019.1704755.
- Mellon, J., 2014. Internet search data and issue salience: the properties of Google trends as a measure of issue salience. *Journal of elections, public opinion and parties*, 24 (1), 45–72. doi:10.1080/17457289.2013.846346.
- Metcalfe, C. and Pickett, J.T., 2021. Public fear of protesters and support for protest policing: an experimental test of two theoretical models. *Criminology*, 30. doi:10.1111/1745-9125.12291.
- Minkus, L., Deutschmann, E., and Delhey, J., 2019. A Trump effect on the EU's popularity? The U.S. presidential election as a natural experiment. *Perspectives on politics*, 17 (02), 399–416. doi:10.1017/S1537592718003262.
- Muñoz, J., Falcó-Gimeno, A., and Hernández, E., 2019. Unexpected event during survey design: promise and pitfalls for causal inference. *Political analysis*, OnlineFirst, 1–21. doi:10.1017/pan.2019.27.
- Nägel, C. and Lutter, M., 2021. The 2017 French riots and trust in the police: a quasi-experimental approach. *European journal of criminology*, 147737082199897. doi:10.1177/1477370821998974.
- Newburn, T., et al., 2018. 'The biggest gang'? Police and people in the 2011 England riots. *Policing and society*, 28 (2), 205–222. doi:10.1080/10439463.2016.1165220.
- O'Brien, T.C., Meares, T.L., and Tyler, T.R., 2019. Reconciling police and communities with apologies, acknowledgements, or both: a controlled experiment. *Annals of the American academy of political and social science*, 687, 202–215.
- Perry, G., Jonathan-Zamir, T., and Weisburd, D., 2017. The effect of paramilitary protest policing on protestors' trust in the police: the case of the "Occupy Israel" movement: effects of paramilitary policing. *Law & society review*, 51 (3), 602–634. doi:10.1111/lasr.12279.
- Reiner, R. (2010). *The politics of the police*. Vol. 4. Oxford: Oxford Univ. Press.
- Reny, T.T. and Newman, B.J., 2021. The opinion mobilizing effect of social protest against police violence: evidence from the 2020 George Floyd protests. *American political science review*. [https://www.dropbox.com/s/189v1jq3iltlyqe/reny\\_newman\\_apsr\\_sharing.pdf?dl=0](https://www.dropbox.com/s/189v1jq3iltlyqe/reny_newman_apsr_sharing.pdf?dl=0).
- Roché, S. and Oberwittler, D., 2017. Towards a broader view of police-citizen relations: how societal cleavages and political contexts shape trust and distrust, legitimacy and illegitimacy. In: D. Oberwittler and S. Roché, eds. *Routledge frontiers of criminal justice. Police-Citizen relations across the world: comparing sources and contexts of trust and legitimacy*. 1st ed.. New York: Taylor and Francis, 3–26.
- Rosie, M. and Gorringer, H., 2009. What a difference a death makes: protest, policing and the press at the G20. *Sociological Research online*, 14 (5), 68–76. doi:10.5153/sro.2047.
- Sahin, N., et al., 2017. The impact of procedurally-just policing on citizen perceptions of police during traffic stops: the adana randomized controlled trial. *Journal of quantitative criminology*, 33 (4), 701–726. doi:10.1007/s10940-016-9308-7.
- Sato, M., 2017. Police legitimacy and public cooperation: Is Japan an outlier in the procedural justice model. In: D. Oberwittler and S. Roché, eds. *Routledge frontiers of criminal justice. Police-citizen relations across the world: comparing sources and contexts of trust and legitimacy*. 1st ed. New York: Taylor and Francis, 108–126.
- Stommes, D., Aronow, P.M., and Sävje, F., 2021. On the reliability of published findings using the regression discontinuity design in political science. *ArXiv:2109.14526 [Stat]*. <http://arxiv.org/abs/2109.14526>.
- Tankebe, J., 2013. Viewing things differently: the dimensions of public perceptions of police legitimacy: public perceptions of police legitimacy. *Criminology*, 51 (1), 103–135. doi:10.1111/j.1745-9125.2012.00291.x.
- Thomassen, G., Strype, J., and Egge, M., 2014. Trust no matter what? Citizens' perception of the police 1 year after the terror attacks in Norway. *Policing*, 8 (1), 79–87. doi:10.1093/policing/pat032.
- Thompson, J., 2021. *Support for same-sex marriage after the pulse nightclub shooting: evidence from a quasi-experiment* [Preprint]. Open Science Framework. doi:10.31219/osf.io/64qm9.
- Trinkner, R. and Tyler, T.R., 2016. Legal socialization: coercion versus consent in an era of mistrust. *Annual review of law and social science*, 12 (1), 417–439. doi:10.1146/annurev-lawsocsci-110615-085141.
- Tuch, S.A. and Weitzer, R., 1997. The polls – trends: racial differences in attitudes toward the police". *Public opinion quarterly*, 61, 642–663.

- Tyler, T.R. and Blader, S.L., 2003. The group engagement model: procedural justice, social identity, and cooperative behavior. *Personality and social psychology review*, 7 (4), 349–361. doi:10.1207/S15327957PSPR0704\_07.
- Tyler, T.R., Fagan, J., and Geller, A., 2014. Street stops and police legitimacy: teachable moments in young urban men's legal socialization: street stops and police legitimacy. *Journal of empirical legal studies*, 11 (4), 751–785. doi:10.1111/jels.12055.
- Tyler, T.R. and Huo, Y.J. 2002. *Trust in the law: Encouraging public cooperation with the police and courts*. <http://search.ebscohost.com/login.aspx?direct=true&scope=site&db=nlebk&AN=1069888>.
- Tyler, T.R. and Jackson, J., 2014. Popular legitimacy and the exercise of legal authority: motivating compliance, cooperation, and engagement. *Psychology, public policy, and law*, 20 (1), 78–95.
- Walters, G.D. and Bolger, P.C., 2019. Procedural justice perceptions, legitimacy beliefs, and compliance with the law: a meta-analysis. *Journal of experimental criminology*, 15 (3), 341–372. doi:10.1007/s11292-018-9338-2.
- Wang, P., Joosse, P., and Cho, L.L., 2020. The evolution of protest policing in a hybrid regime. *The British journal of criminology*, azaa040. doi:10.1093/bjc/azaa040.
- Weitzer, R., 1999. Citizen's perceptions of police misconduct: race and neighborhood context. *Justice quarterly*, 16, 819–846.
- Weitzer, R., 2002. Incidents of police misconduct and public opinion. *Journal of criminal justice*, 30 (5), 397–408. doi:10.1016/S0047-2352(02)00150-2.
- Weitzer, R., 2017. *Recent trends in police-citizen relations*. In: D. Oberwittler and S. Roché, eds., 1st ed.. New York: Taylor and Francis.
- Weitzer, R. and Tuch, S., 2004. Race and perceptions of police misconduct. *Social Problems*, 51 (3), 305–325. doi:10.1525/sp.2004.51.3.305.
- White, C., Weisburd, D., and Wire, S., 2018. Examining the impact of the Freddie Gray unrest on perceptions of the police. *Criminology & public policy*, 17 (4), 829–858. doi:10.1111/1745-9133.12404.
- Wood, L., 2020. Policing counter-protest. *Sociology compass*, 14 (11), 1–10. doi:10.1111/soc4.12833.
- Worden, R.E. and McLean, S.J., 2017. Research on police legitimacy: the state of the art. *Policing: an international journal of police strategies & management*, 40 (3), 480–513. doi:10.1108/PIJPSM-05-2017-0062.
- Zaller, J., 1992. *The nature and origins of mass opinion*. Cambridge: Cambridge University Press.