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'If a Fight Starts, Watch the Crowd'

The Effect of Violence on Popular Support for Social Movements.

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Abstract

Diverse social movements often face tactic diversification. In otherwise nonviolent movements, some groups or radical flanks may resort to violent actions such as street rioting. This article analyzes the impact that these violent episodes can have on popular support for the social movement as a whole. To estimate the causal effect of violence, it exploits an unexpected riot outbreak occurred during the fieldwork of a face-to-face survey in Barcelona in May 2016, lead by a squat group linked to the anti-austerity movement known as the 15M or *indignados* that emerged during the financial crisis. By comparing respondents interviewed before and after the riots, it finds that the street violence episode reduced support for the 15M movement in 12 percentage points on average. However, the magnitude of the effect is highly conditional on the respondents' predispositions towards the movement. Core supporters, that are expected to share the frame of the movement in justifying violent actions, are the least affected by the violent outbreak. On the other extreme, weak supporters, opposers and non-aligned citizens reduce their support to a larger extent. Results are robust to different specifications and a wide range of robustness checks. These findings have potentially important implications for movements concerned with broadening their support base.

Keywords: social movements; radical flanks; violence; natural experiment;
15M/Indignados

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Introduction

Following Donald Trump’s inauguration in early 2017, a wave of protests swept the US. Most of them were peaceful, colorful demonstrations and civil disobedience actions. But some protesters also engaged in violent rioting. This is a common pattern in many social movements, from the 1960’s civil rights movement, to the labor movement or even the anti-Nazi resistance in Germany: some of its components -often the majority- tend to engage in peaceful mass protests, or small-scale civil disobedience, while others engage in more violent tactics, often following incentives to diversify tactics within a movement ([Cunningham & Fruge, 2017](#)).

Protest movements in democratic contexts have a wide array of tactical choices at their disposal. While sometimes the repertoire itself has an intrinsic value to the movement participants’, often the choice of a specific set of protest instruments derives from an assessment of the costs and potential benefits of each course of action. Violent protest is under most conditions considered more costly than nonviolent actions. However, assessing the effectiveness various protest tactics might be more difficult, as it involves at least two main challenges. The first one is defining what exactly constitutes a ‘success’ of a protest movement. This has often been operationalized as policy or regime change, but social movements can also have more general, or intermediate goals such as changes in culture or public opinion. The second challenge is the endogeneity of tactic choice: if movements are strategic, they will select one or another course of action depending on the anticipated likelihood of success. These problems, together with the impossibility of observing the counterfactual once a choice is made, make the estimation of the consequences of tactical choices highly problematic in most situations.

In this article we argue that public support for a protest movement is an important outcome of the movement’s tactics. Public support is important for social movements because it can condition their likelihood of success. We offer an arguably credible causal estimation of the effect of violent protest episodes on popular support for protest movements. We do not claim that public support *per se* constitutes movement success, but the maximization of popular support is undoubtedly a relevant goal in itself for most social movements. And, perhaps more interestingly, it can also be a crucial mediator to explain other outcomes of social movements such as fund-raising capacity, organizational strength and, ultimately, policy impact.

We expect violence to have a negative effect on public support for the movement, because violence makes it hard for citizens to identify with the movement. To test this hypothesis we rely on the sudden occurrence of a series of street riots during the fieldwork of a face-to-face survey in Barcelona. The eviction of a squat center

was followed by a set of riots that lasted 5 days by the end of May 2016. The squat center was associated with the so-called 15M movement, a wide protest movement born during the years of the economic crisis in Spain, also known as the *indignados*. This movement had a wide agenda of socio-economic and democratic claims, and had so far been characterized by nonviolent protest. By comparing respondents interviewed before and after the riots, we estimate a negative average effect of the violence outbreak of about 12 percentage points in support for the 15M movement.

We also expect the consequences of violent actions to be different for citizens with different prior attitudes towards the movement, because core supporters will be more receptive to movement frames that justify the violent actions. We find that core supporters of the movement appear as relatively immune to the use of violence, while those that we define as weak supporters are most affected.

Our results are robust to the inclusion of controls and various specifications, as well as a number of robustness tests. They suggest that the cost of violence in terms of social support is very high, and particularly so among the segments of the population in which the movement may have more opportunities to grow. We must however be aware of the fact that the effect we are able to credibly estimate refers to the whole episode, that includes, other than the riots themselves, the police trigger (eviction), the anti-riot police response and the political reactions by members of different parties. However, we regard this loss of precision as a price to pay for external validity of our findings, that are based on a real-world case of violent protest which, as it is often the case, was embedded in a wider set of events, including triggers, reactions and consequences.

Theory

Related Literature

Previous research has addressed the consequences of movement tactics including violent versus nonviolent action for policy, mobilization or cultural outcomes. Some authors have argued that the ability of social movements to bring about political change at various levels depends on their ability to disrupt existing practices ([Fishman & Everson, 2016](#); [Cloward & Piven, 1979](#)) and on using a variety of tactics ([Morris, 1993](#)), including violence. Some studies have provided evidence indicating that the presence of radical flanks that engage in violent actions in otherwise non-violent movements is related to a higher capacity of resource mobilization in social movements through funding ([Haines, 1984](#)) and to campaign progress ([Tompkins,](#)

2007).

However, an increasing amount of evidence seems to suggest that violent tactics are less effective in achieving movement's goals than nonviolent ones ([Chenoweth & Cunningham, 2013](#); [Howes, 2013](#); [Huet-Vaughn, Huet-Vaughn; Stephan et al., 2008](#)). Armed resistance from radical flanks seem to have at least both positive and negative effects, appearing to be counterproductive in the long run even if it may bring some short-term advantages ([Chenoweth & Schock, 2015](#); [Wendt, 2013](#)).

The literature points to a number of mechanisms for the lower efficacy of violent protest. Violence may have several unintended consequences such as enhancing elite's discourses based on public order maintenance ([Wasow, 2017](#)), reinforce the opponent ([Howes, 2013](#)), facilitate repression from the state ([Soule et al., 2004](#); [Stephan et al., 2008](#); [Tompkins, 2007](#)), and reduce the ability to remain resilient in the face of oppression ([Chenoweth & Cunningham, 2013](#)). From the point of view of potential participant activists, violent protest involves a high risk of repression, and is therefore a high cost activity and hence depresses participation in movement campaigns ([Tompkins, 2007](#); [Chenoweth & Schock, 2015](#)). Successful movements need a broad base which can hardly be achieved by violent means ([Ackerman & Rodal, 2008](#)).

One of the mechanisms through which violent actions may reduce the likelihood of success of the movements is because they may have a negative impact on public support for the movement and its goals. Public support is a very important resource for social movements ([Ennis, 1987](#)). Public support for the movement anticipates ability to mobilize other resources, sends signals to elites and majorities, and is more likely to grow further bringing additional activists. This makes public support a very important outcome of social movements actions, consequential for the accomplishment of the movement's objectives, and which can be put at risk with the implementation of violent tactics.

The question of how violent tactics affects public support for a movement remains under-explored. Most of the attention has been devoted to how violent versus nonviolent tactics impact on policy, leaders or regime change ([Enos & Sands, 2017](#); [Huet-Vaughn, Huet-Vaughn; Soule et al., 2004](#); [Tompkins, 2007](#); [Chenoweth & Schock, 2015](#)). While there is a rich literature on cultural outcomes of social movements (see [Earl, 2008](#): for a summary) and some works that connect social movement activity to political attitudes (for instance [Banaszak & Ondercin, 2016](#); [Lee, 2002](#)), very few works address the question of how the use of violent tactics impacts public support for a movement.

[Louis \(2009\)](#) points to the fact that psychology has rarely entered the analysis of how

collective action may produce social change, and so little attention has been paid to the psychological mechanisms that drive support for movements. This requires individual level information that is not easy to obtain in a field where most of the data available come in an aggregate format. Previous works based on individual survey data (for instance [Rohrschneider, 1990](#)) consider mostly demand-side factors, that is, how individual characteristics relate to social movement's support, but not supply-side factors such as movements' actions.

A few recent works have tackled into the broader question of how protests affect individual attitudes. Wallace and her associates [Wallace, Zepeda-Millán & Jones-Correa \(2014\)](#) use observational data to explore how proximity to protests changes individuals' attitudes of political efficacy. [Lee \(2002\)](#) shows how grassroots organizations and local protests in the civil rights movement push demands for social change into the general public. Andrews *et al* have combined survey and contextual data on protest events to analyze how proximity civil right protests affect support for the civil rights movement. They found a positive effect among some white individuals, contingent upon contextual characteristics ([Andrews & Farnum, 2016](#)). Some studies exploit exogenous variation in exposure to protests. [Frye & Borisova \(2016\)](#) for instance compare attitudes of citizens interviewed before and after an important demonstration held in Moscow against election fraud and found that it had a positive effect on trust in government.

[Wasow \(2017\)](#) addresses specifically the effects of violent vs. nonviolent protest leveraging the case of 1960s Black Insurgency and its effects on support for the Democratic Party. The analysis shows that proximity to violent protests had a number of effects both on public opinion and Congressional speech that conducted to decline in support for the Democratic party and may have been crucial for the electoral outcome of 1968.

Other works leverage unexpected and arguably exogenous events. Young looks at the unexpected occurrence of state repression to examine its effects on vote choice among the poor ([Young, 2016](#)). [García-Ponce & Pasquale \(2015\)](#) analyze how exposure to pre-election violence influences support for the state through preference falsification voting patterns in Africa. They point that (2015:22) “a next step for this research agenda is to systematically observe how citizens respond to other political shocks – such as opposition protests, rallies, and demonstrations.” Both Young and Ponce and Pasquale studies focus on the context of authoritarian regimes, so we add to this request the need to explore the consequences of violence in democratic political contexts. While the research design of these two works allow to precisely estimate effect of state repression on voting and state support, we still lack a proper analysis of how violent episodes within social movements may affect social movement support

in democratic contexts.

Argument

Our general expectation is that the use of violent protests will harm popular support for social movements. Social support is the main source of political power. Power is not “intrinsic to the rulers” but “comes from the society they govern” (Sharp, 1990). Though Sharp develops his conceptualization of the sources of power mostly thinking on rulers or the State, he also considers that challengers of the status quo can change the distribution of power in society. We contend that the distribution of power will be contingent on the amount of social support that each relevant political actor (the State/the social movement) is capable of gathering in a given conflict. Sharp considers that an adequate strategy of action is a crucial element for the success of a movement to achieve their purposes. He highlights nonviolent action as a means to increase strength and support for the resisters’ cause. *Ad contrarium*, we expect the use of violent actions to reduce this support.

We follow Feinberg and collaborators’ idea that violence makes it difficult for bystanders to identify with movement activists (Simpson & Feinberg, 2018). Since collective identity is the most important predictor of collective action (Klandermans, 1984; Van Zomeren, Postmes & Spears, 2008), we expect that support for the movement will also be conditioned by the extent to which a bystander can identify with those carrying out a protest.

As Snow, Soule & Kriesi (2008) argues, movements success (and movement support) depends on the extent to which grievances (and tactics) are framed in a way that resonates with mainstream beliefs and values. We expect violence to be unlikely to resonate well in democratic political contexts even in the presence of some demands that may be considered legitimate.

Violence may alienate would-be supporters because people have moral issues with violence. While violence may be perceived as justified in certain circumstances, there is a negative connotation associated to the concept. The ethical dimension may be secondary or irrelevant for some movement activists, but it is generally less likely that the general public will justify violent tactics. Political violence is a taboo, according to people’s perceptions (Van Aelst & Walgrave, 2001) and is often incompatible with their values and needs, which are crucial aspects for movement diffusion (Soule et al., 2004). As a consequence, people are expected to be less likely to support/ identify with social movements if the deploy violent tactics.

Violent radical flanks however may have positive effects by affecting the percep-

tions that both the public and decision makers have of moderate sectors within the movement. By deploying violent tactics, radical flanks make moderates appear reasonable, so a more positive perception of the moderates can compensate the loss of support produced by violent radical flanks. However this potentially positive radical flank effects (as argued by [Haines, 1984](#)) would involve a fairly detailed knowledge of the movement dynamics, which may be visible for the ruling elite or the activists themselves, but less so for the average citizen, who will probably have a hard time making the distinction between moderates and radicals. On average, we expect that the the *silent majority* would be put down by this the use of violent tactics.

[Simpson & Feinberg \(2018\)](#) already provide some support for this idea, using survey experiments where they manipulate the extremity of movements protest behavior of different hypothetical groups with different repertoires of action. They find the expected negative effect of extremity on support, with identification with the movement as mediating variable. Our research contributes to this strand of research by providing evidence that is contextually located in a real, and not hypothetical case, and focusing specifically on the consequences of violent actions.

However, the average effect of protest tactics is probably not the most analytically interesting outcome. Often, movements are concerned with the reaction of their strong and likely supporters, but not the whole population. Moreover, we should not necessarily expect everyone to react in the same way when facing political events, as we know that group identities and predispositions greatly condition the way we perceive and process political information.

In other words, the average effect will obscure analytically and politically relevant heterogeneities. Different citizens will react differently to violent protests. Whenever there are protests and violence associated, we often see competing frames of interpretation of the events, with opposing views on who is ultimately responsible for the violent outbreak (police or protesters), the severity of the violence employed by one camp or the other and, explicitly or not, also on the legitimacy of the use of violence. The social movement and its supporters will tend to claim that their actions were a legitimate response to the authorities, while the status quo advocates will de-legitimize the movement by focusing on its violent tactics and frame the response by police forces as necessary, proportional and appropriate.

Therefore, in the aftermath of violent protests, we should expect citizens to be confronted with competing frames. Under most situations, whenever citizens are exposed to political events, they will also be exposed to the actors' interpretation and framing of the events. The attitudinal implications of this situation are not perfectly understood, but the literature has identified a set of cognitive processes

that condition how citizens receive and process information. In the first place, the well-known mechanism of selective exposure predicts that citizens will be over-exposed to the frames that are aligned with their prior views, thus reducing exposure to contradicting information. Second, confirmation bias states that citizens will pay more attention to the messages that support their priors. And finally, motivated reasoning theory expects citizens to be driven by their predispositions in processing information, and may "ignore or devalue contrary information, bias the perception of credibility, or overlook important factors" (Taber & Glathar, 2001).

The adoption of this perspective for the attitudinal consequences of protest violence lead to more nuanced expectations than a general, across the board negative effect. Even if we assume a general dislike towards violence, the effects of violent tactics on support shall be conditional on citizens' prior predispositions towards the movement. If we classify citizens along a continuum of support for the movement, we can distinguish between core supporters, weak supporters, indifferent, and opposers. For each of these groups we might lay out different expectations.

Opposers will be exposed and willing to receive negative information about the movement, so we expect the outbreak of violence to negatively affect their attitudes towards the movement, fostering rejection. However, the effect among opposers might be subject to floor effects: if they already display an extremely low support for the movement, there might not be much room for further decrease. At the other extreme of the spectrum, core supporters will tend to be exposed to and privilege the movements' interpretation of the events. Therefore we expect the impact of the violent tactics on their support for the movement to be generally minimal. Even if they dislike violence in general, they might be shielded from the negative effect by the processes of selective exposure and motivated reasoning.

Weak supporters, on the other hand, have some sympathy for the movement but the lack of deep emotional attachment or group identification means that they should not be affected in the same way as core supporters by the psychological mechanisms described above. Therefore, we expect them to be fully sensitive to the off-putting effect of violence. Likewise, the ambivalent and neutral segments of the public opinion shall also be negatively affected by violence, as long as they pay sufficient attention to the violent events and have an attitude at all towards the movement.

H1 Street violence is expected to have a negative impact on popular support for protest movements.

H2 The effect will be weaker among core supporters, who will tend to share the movement's framing of the violent outbreak.¹

¹Our ability to test this second hypothesis is hindered by the threat of post-treatment bias:

Empirics

The case: 15M movement in Barcelona and the 2016 Gràcia Riots

Our research strategy takes the case of the 15M movement, and a series of related riots that took place in May 2016 in Barcelona. We may consider Barcelona a city in which the 15M and its demands have enjoyed considerable support and political projection, and hence, a difficult case to test the potential negative effect of violent protest on public opinion support for this movement. The course of events is as follows.

In May 2011, a protest wave emerged in crisis-ridden Spain. The movement, that would take the name of its founding date (15M) started with a series of demonstrations that led to long term occupation of the central squares of the country's main cities. It had a diverse agenda, with a focus on both socio-economic and political discontent, staged by the platform *Democracia Real Ya*. The 15M has been a highly popular movement in Spain, with levels of support from public opinion that were over 65% at the time of its birth ([Anduiza, Cristancho & Sabucedo, 2014](#)). Soon after its peak moment, the movement decided to decentralize at the local and community level, and a myriad of local groups and initiatives emerged. Some of its members also chose to engage in electoral politics, which lead to the rise of the new leftist party *Podemos* and several local platforms, such as *Barcelona en Comú*.

In the wake of the 2011 15M mobilizations, a group of people occupied a disaffected former bank office in a lively and commercial street of Gràcia, a central neighborhood in Barcelona. In this premises, about 30 or 40 people started to develop a “free place” project, without state or private property, in which different social activities were carried out (food banks, free shop, library). The place was called *banc expropiat* (expropriated bank). Following the decentralisation of the 15M movement, its Gràcia local chapter met there too.

The owner of the place, the savings bank *Catalunya Caixa*, tried to recover the property suing the occupants in 2013, but abandoned the civil procedure in 2014 to sell the place. The new owner did not continue with the judicial process because, as it was later to be known, the government of Barcelona, headed at that time by mayor Trias (CiU) had been paying him an annual rent of 65,000 euros to avoid the political costs of an new eviction. That same year mayor Trias had failed to achieve

if support status is affected by the violence outbreak, we cannot estimate its moderating effect. In order to circumvent this problem we rely on a proxy for support based on past behavior (vote recall). See the discussion in the Data and Measurement section.

the eviction of other premises in a similar situation under the pressure of an intense and escalating wave of protests.

Barcelona en Comú won the 2015 local elections in Barcelona under the leadership of Ada Colau, former speaker of an anti-eviction movement organization. In January 2016 the government of Barcelona stopped paying the rent to the legal owner of the *Banc Expropiat*. This produced a judicial eviction order, that was carried out by the Catalan police on the 23rd of May 2016. A campaign against this eviction started with a demonstration that very evening, and continued in the days to follow. Initially the campaign included social media mobilization actions, stickers and posters, and peaceful demonstrations. But soon the protests evolved into full-scale rioting, including clashes with the police, erection of fire barricades and property destruction in the neighborhood of Gràcia. Dozens of protesters and policemen were wounded during the riots, which lasted at peak intensity for four nights. Protest actions continued for two weeks.

Both the police intervention and subsequent riots were unexpected. The 15M has been a nonviolent movement. Although there have been some mild episodes of violence in some of the protest events it has staged, these go back to 2011 and none has been as significant or intense as the one related to the *Banc Expropiat*.² As such, our case reflects a situation of an occasional violent outburst, rather than a case of systematic violent tactics or armed struggle.

Several facts make evident the connection between the 15M and the *Banc Expropiat*, which mutually acknowledge each other. The *Banc Expropiat* is included as one of the October follow-up mobilizations events in the 15Mpedia (15Mpedia, 2016). The *Banc Expropiat* blog refers to itself as a venue for the Gràcia chapter of the 15M meetings *in the cooler and rainy weather of Autumn* (Banc, 2015). These details may be known only to a narrow audience of very well informed activists, while most citizens may remain unaware of these connections. But even if this were the case, a weak connection of the two objects would work against our expectations leading to a weak or null effect of the treatment. A significant effect of the *Banc Expropiat* treatment over the level of support for the 15M, on the other hand, would suggest that, at some level, people are aware of the connection between the two.

²A summary of different protest events related to the 15M up to April 2015 can be found at https://en.wikipedia.org/wiki/Anti-austerity_movement_in_Spain (retrieved 29 June 2018). In Barcelona the most important events in which there was some violence were the demonstrations in front of the Catalan Parliament in June 2011.

Identification strategy: Assumptions and threats

We exploit the unexpected occurrence of these riots connected to a local group of the 15M movement during the fieldwork of a face-to-face survey in Barcelona in order to estimate the effect of these violent protests on citizens' support for the movement. Using unexpected events during survey fieldworks is an increasingly used identification strategy to address a number of questions. These events include terrorist attacks (Legewie, 2013), corruption scandals (Ares & Hernández, 2017), protests (Frye & Borisova, 2016) or state repression (García-Ponce & Pasquale, 2015). Scholars have explored their effects on social and political trust, support for the incumbent or attitudes towards immigrants, among others. This research design allows for an estimation of the causal effect of the event on a given outcome under a common set of potentially problematic identifying assumptions. In this section we discuss the assumptions and present some evidence and strategies to make them credible.

Ignorability

The first one is ignorability of treatment assignment. Since assignment to treatment and control groups is not random neither controlled by the researchers, and correlates perfectly with time in which the survey was administered, it might correlate with observable characteristics of the respondents related to the fieldwork organization.

In our case, the research team led by the authors selected over 150 starting points across the city (in a stratified random selection of addresses), from which random routes started. The fieldwork company set the order in which the routes would be followed, but they were given instructions to distribute the interviewers across districts during the whole duration of the fieldwork instead of concentrating first in some districts and then moving to the others. Figure 1 shows the location of the squat center and the extension of the riots, together with the geo-location of respondents interviewed before and after the riots.

The map suggests no systematic geographical pattern, although this can also be formally tested, together with other possible, non-geographic imbalances between respondents interviewed before and after the riots. Table I presents the balance tests on a number of observable variables.

The pre and post-riots samples are balanced on most covariates. Some slight imbalances are present with respect to ideology but they are minor and do not seem to suggest any systematic bias in a specific direction. In any case we will take into account these imbalances in the estimation.

Table I. Balance table: Comparing respondents pre and post-riots

| | Pre | Post | Difference | p-value |
|-------------------------------|--------|--------|------------|---------|
| Core supporters | 0.284 | 0.297 | -0.013 | 0.718 |
| Weak supporters | 0.362 | 0.339 | 0.023 | 0.623 |
| Opposers | 0.304 | 0.288 | 0.016 | 0.745 |
| Non-aligned | 0.050 | 0.076 | -0.026 | 0.136 |
| Age | 49.071 | 49.732 | -0.661 | 0.342 |
| Ideology (0-10) | 3.761 | 3.686 | 0.075 | 0.016 |
| Woman | 1.512 | 1.558 | -0.046 | 0.233 |
| Political knowledge (0-3) | 1.185 | 1.242 | -0.057 | 0.497 |
| Latitude | 41.405 | 41.405 | 0.000 | 0.704 |
| Longitude | 2.166 | 2.166 | -0.000 | 0.652 |
| Spanish nationality | 0.881 | 0.897 | -0.016 | 0.520 |
| Association member (0-1) | 1.714 | 1.735 | -0.021 | 0.392 |
| Internet | 0.878 | 0.914 | -0.036 | 0.079 |
| Evaluation economic situation | 4.017 | 4.068 | -0.051 | 0.354 |
| National identification | 3.625 | 3.689 | -0.064 | 0.479 |
| Social trust | 5.455 | 5.461 | -0.006 | 0.272 |
| Political interest | 2.436 | 2.436 | -0.000 | 1.000 |
| N | 771 | 729 | 1,500 | |

Excludability

The other main assumption on which we rely is the exclusion restriction, or excludability. In this case, this means that being surveyed before or after the riot outbreak only affects the outcome through the actual *treatment* of interest (exposure to violent tactics). This can be violated under two circumstances. One is any unrelated simultaneous or quasi-simultaneous political event that may also have affected the outcome and confounds the treatment effect. A close reading of those days' newspapers does not seem to suggest any potential threat to the exclusion restriction. We have analyzed the main three Barcelona newspapers during the fieldwork and there were no other news directly concerning the 15M movement that were on the media front-pages. Online appendix A contains a detailed description of the front-pages of the main three Barcelona newspapers during the fieldwork.

The other potential threat to the exclusion restriction is the concatenation of events spurred by the riot outbreak. This, in our design, is not possible to rule out. First, the riots were spurred by the police decision to evict the squat center. Second, the Catalan police used a variety of tactics to counter the protesters, from the use of batons to foam projectiles. This also attracted media attention and was criticized by the protesters and some other actors (most notably the radical left party CUP). And third, all relevant local political actors expressed their position with regards

to the conflict. Most of them sided with the police, some -most notably, the leftist mayor Ada Colau, offered themselves as mediators.

With our empirical strategy, it is not possible to isolate the effect of violence *per se* from the combined effect of these reactions, so any estimate must be interpreted as the compound effect of this set of combined events. Although the riots, the police action and the political and media reactions are analytically distinct events, we are not able to separately estimate their effects. We regard this as a trade-off between external validity and precision of the treatment for which one can estimate the effects: in the real world social movements violent tactics do not appear in isolation but nested in this tangle of things.

It is our claim that the unexpected nature of the riot outbreak offers a good opportunity to estimate the effect of violent tactics on movement support. The aforementioned threats and limits to identification, however, need to be taken into account in the estimation and interpretation of results. Together with the main results, we also present a set of robustness checks aimed at strengthening the estimates' accuracy and credibility. More specifically, in the Online appendix we show two tests that lend additional credibility to the excludability assumption: one in which we show how the effect is far larger for the residents in the district in which the riots took place, and a set of placebo tests in which we show how our treatment variable does not affect support for unrelated social movements or external political efficacy.

Data: Sampling and measurement

The data was collected in the context of the research project *Pathways to Political Inclusion*. Our data come from a survey that was conducted between May 9 and June 9 2016 on a sample of 1500 respondents, older than 18, living in Barcelona. The stratified sample is based on 60 zones, corresponding to the 73 neighborhoods in the city that result from grouping those with less than 8,000 inhabitants with their neighboring areas. The interviews were proportionally allocated to a random selection of non-contiguous Primary Sampling Units (PSUs), that in this case were census tracts within each neighborhood. Within this PSUs, households were contacted following random routes and within the household, the respondent was selected according to a quota-system based on age (18-29, 30-44, 45-59, >60) and gender (men-women), calculated within each district. The survey was devoted to political participation, with a set of questions on various forms of engagement, attitudes towards social movements and a set of socio-demographic controls.

The outcome variable (support for the 15M movement) was elicited through a direct

question on whether the respondent supports the movement, located within a set of questions on various social movements. For each movement, respondents were asked whether they knew it, and whether they sympathize with it or not. Although we privilege the dichotomous operationalization of support, in the Online Appendix we present analyses that take into account also the "don't know" responses, and deal carefully with non-response.

The treatment of interest is whether the respondent was interviewed before ($T = 0$) or after ($T = 1$) the riots. We prefer to not use the respondents interviewed during the days the riots were taking place, since we could miss-classify them as treated when they had not yet received the information. However, in the Online appendix we show how results hold if we consider them as treated.

A key moderator to test our theoretical argument are predispositions towards the movement. In order to test this, we need to specify an interaction between support and exposure to the riots. However, we do not have a pure pre-treatment measure of support for those in the treatment group, and conditioning on post-treatment variables can induce bias on the estimates ([Montgomery & Torres, 2016](#)).

Given our data, the best option to estimate the heterogeneous effects is relying on a proxy for movement support that is likely unaffected by the treatment. Therefore we rely on vote recall in the 2015 general elections as a proxy for movement support. This is a rough proxy of our variable of interest, and might limit our ability to test the theory. However, it allows to plausibly estimate the effects with the data we have. Additionally, in the Online appendix we show how the main findings are robust to the use of alternative moderators, such as party identification and left-right self-placement. As our proxy refers to a past behavior, it should arguably be free from post-treatment bias.³ We code voters of the pro-15M party *En Comú-Podem* (linked to both *Barcelona en Comú* and *Podemos*) as core supporters, voters of the center-left ERC and PSOE as weak supporters, and voters of the center-right and right parties (PP, PDeCAT and Ciudadanos) as opposers. Those that did not vote or casted a blank ballot are coded separately as non-aligned. Because we are referring to parties' relation towards the 15M and not to specific individual attitudes towards the 15M, and because this movement was quite popular (see section 3.1) we may still find some support towards the 15M among voters of the right and center-right parties.

³This will hold except in the case that the riots had an effect on the propensity to declare vote for some options, which we cannot rule out, although empirically does not seem to be the case given balance between treatment and control.

Results

Table II shows the raw proportions of support for the 15M movement before and after the riots. As it can be observed there is a 10 percentage point drop in support among respondents interviewed after the violent outbreak: the proportion of respondents that sympathized with the 15M went down from 0.65 to 0.55. This is a substantively important and statistically significant difference that points to a loss of support following the violent tactics deployed by groups of activists affiliated with the 15M.

Table II. Support for the 15M movement, pre and post-riots

| | (Pre) | (Post) | (Difference) |
|-----------------------|--------|--------|--------------|
| Proportion supporting | 0.65 | 0.55 | -0.10 |
| (SE) | (0.02) | (0.02) | (0.03)*** |
| Observations | 649 | 524 | 1,173 |

However, in order to interpret this difference as an effect of the riots, as discussed earlier, two assumptions need to be satisfied: ignorability and excludability. In table III we present the results of a set of models aimed at fostering the credibility of the estimate. All models have precinct-clustered standard errors, in order to account for the sampling strategy described earlier. The first four models are linear probability models estimated through multivariate OLS regressions, and the final two models are based on an entropy balancing, a data preprocessing method conceived to achieve covariate balance (Hainmueller, 2012; Hainmueller & Xu, 2013) that reduces model-dependence of the estimates.

Model 1 just reproduces the bivariate findings presented above. In model 2 to 4 we include a vector of relevant individual-level controls (age, gender, past vote, latitude and longitude), as well as district fixed effects. They are meant to ensure conditional ignorability of treatment assignment. Models 3 and 4 address the problem of potential unobservable confounders, by incorporating the number of refusals collected before each interview as a control for *reachability*.

One strategy to lend additional credibility of the exclusion restriction, understood as the absence of simultaneous political events that might confound our effect, is to restrict the time frame around the riot outbreak. The probability that some other event is driving our results shall be smaller as we narrow down the amount of days we take into consideration. Given that our data comes from a face-to-face survey, the fieldwork was relatively long: it lasted from the 25th of April to the 9 of June, 2016. Therefore, a useful robustness check might come from narrowing it down.

Since the narrowing down of the time span can be arbitrary, in table IV we present,

Table III. Support for the 15M movement, pre and post-riots

| | OLS | | | Entropy balancing |
|-----------------------|-------------------|-------------------|-------------------|-------------------|
| | (1) | (2) | (3) | (4) |
| Post-riots | -0.10** (0.03) | -0.11** (0.03) | -0.11** (0.03) | -0.12** (0.03) |
| Controls | | YES | YES | YES |
| District FE | | | YES | YES |
| Reachability control | | | YES | YES |
| Cluster SE (Precinct) | YES | YES | YES | YES |
| R-squared | 0.010 | 0.257 | 0.258 | 0.015 |
| N | 1,173 | 816 | 814 | 848 |

† p<.1, * p<.05, ** p<.01

*Controls include age, gender, past vote, latitude, longitude and district

for the sake of transparency, the estimate with a range of time windows, from 1 day before and after the riots started, to 11 days. As it can be observed, the effect is negative, of a similar magnitude, and statistically significant down to the 5+5 days window. If we take 4 or 3 days before and after the riots, the effect is still negative but very imprecisely estimated. In the two narrowest time periods, with less than 100 valid observations, we find a non-significant positive effect.

The data do not show, therefore, a sudden change in public opinion, but rather a realignment that took a few days. Certainly, this might point to the fact that the observed result is the composite effect of the whole episode of eviction, riots and political reactions, rather than an immediate reaction to the riots *per se*. However, in the Online appendix we present a number of placebo tests which indicate that the event did not affect respondents' perception of the political system, or other social movements (except for the most closely related to the 15M), which gives us some additional confidence that we are actually capturing the riot effect. Overall, however, the whole set of events generated a substantial decrease in sympathy for the 15M of 12 percentage points, providing evidence in line with our first hypothesis.

Additionally, in section C2 of the Online appendix we also show how our results are robust to the inclusion of a linear time trend interacted with our treatment, and that other arbitrary or meaningful partitions of the sample do not yield the same results as the riot outbreak. There is, therefore, no apparent time dynamic other than the one caused by the riot outbreak. On the other hand, after the riots we do not see any time trend at all, so we have no evidence of recovery of support for the 15M. This speaks to the duration of the effects, but of course this might be related to the fact that the post-riot fieldwork period is too short to observe such a recovery.

Table IV. Estimate under different time windows

| Days | Estimate | SE | Valid N | N Pre | N Post |
|-------|----------|--------|---------|-------|--------|
| 11+11 | -0.10* | (0.04) | 590 | 224 | 366 |
| 10+10 | -0.10* | (0.04) | 561 | 211 | 350 |
| 9+9 | -0.11* | (0.04) | 521 | 198 | 323 |
| 8+8 | -0.13** | (0.04) | 479 | 189 | 290 |
| 7+7 | -0.13** | (0.04) | 474 | 189 | 285 |
| 6+6 | -0.13** | (0.04) | 417 | 167 | 250 |
| 5+5 | -0.11* | (0.05) | 331 | 123 | 208 |
| 4+4 | -0.08 | (0.06) | 238 | 84 | 154 |
| 3+3 | -0.05 | (0.06) | 153 | 56 | 97 |
| 2+2 | 0.01 | (0.09) | 88 | 41 | 47 |
| 1+1 | 0.06 | (0.10) | 74 | 41 | 33 |

† p<.1, * p<.05, ** p<.01

Heterogeneous effects

Regarding our second hypothesis, we explore the heterogeneous effects across groups defined by their (proxied) predispositions towards the movement. As discussed, we use past vote as our preferred proxy, in order to minimize post-treatment bias. As robustness checks we replicate the analysis using partisanship and ideology as proxies for previous attitudes towards the movement and the results remain essentially stable (see Online appendix).

In table V we show the raw data of support for the 15M movement before and after the riots, split by past-vote groups. As it can be seen, voters of parties that strongly supported the movement (Podemos and CUP) that we label as core supporters show only a slight and non-significant decrease in support. Voters of center-left parties (ERC and PSC) -that we treat as 'weak supporters'-, opposers (voters of center-right and right) and non-aligned (abstainers) show a much stronger decrease in support after the riots.

Table V. Support for the 15M movement

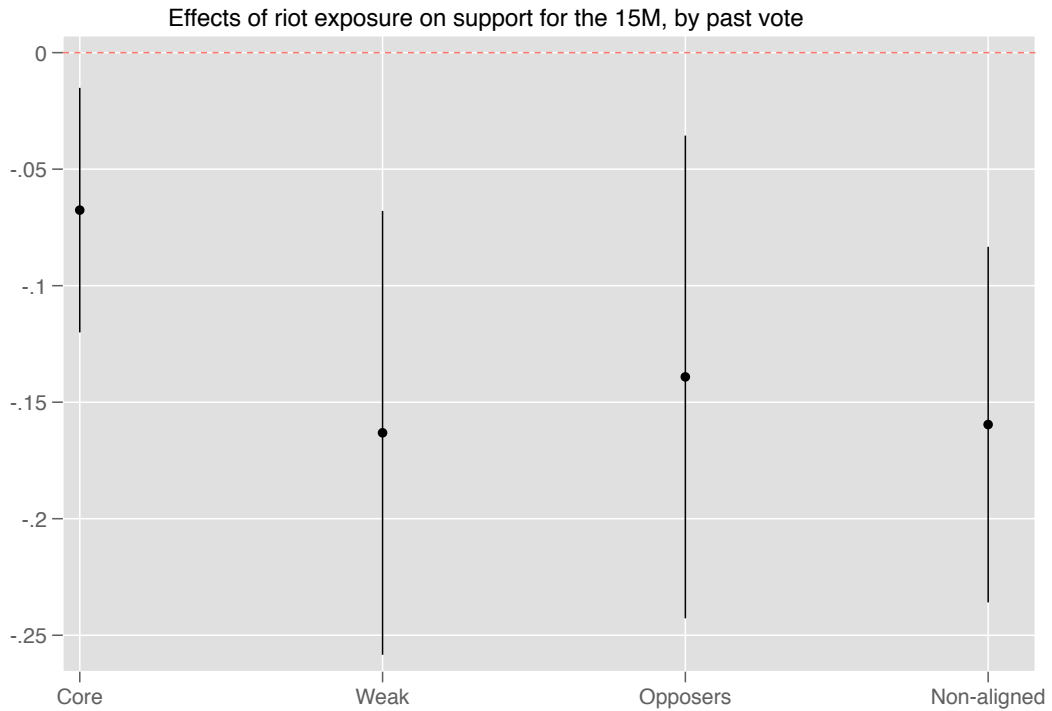
| | Core | Weak | <i>Opposers</i> | Non-Partisans | Total | N |
|------------|------|-------|-----------------|---------------|---------|-------|
| Pre-riots | 91.9 | 67.1 | 40.6 | 60.0 | 66.16 | 649 |
| Post-riots | 90.2 | 51.1 | 27.4 | 26.09 | 55.47 | 594 |
| Difference | -0.1 | -16** | -13.2* | -33.9* | -10.7** | 1,243 |

† p<.1, * p<.05, ** p<.01

This result goes in line with our expectations. However in this sub-group analysis the results could be partly driven by compositional effects. In figure 2 we show the result of an interaction analysis based on a full model using entropy balancing with the same specification as model 4 in Table III (full table in the Online appendix).

While there is a negative effect across all groups, it is significantly weaker for the core supporters (-6.8 percentage points) than for the weak supporters (-15.5), the opposers (-13) and the non-aligned (-15.5). Moreover, these heterogeneous effects are robust to the use of alternative moderators, such as ideology or partisanship, as shown in the Online appendix.

Figure 2. Treatment effects by past vote



Conclusions

Research on protest and social movements increasingly converges towards the finding that non-violent protest movements tend to be, on the long run, more successful in reaching their goals and promoting policy change than violent ones. But, as social movements are often constellations of organizations, we also know that there are strong incentives for intra-movement tactical diversification ([Cunningham & Fruge, 2017](#)).

In this article we have explored a likely mechanism through which the use of violent tactics might harm a movements' prospects of success: the erosion of popular support. Assuming a generalized dislike of violence, we can expect, *ceteris paribus*, that movements that resort to violence will lose some degree of public support.

Taking advantage of an unexpected set of riots occurred in Barcelona during the

fieldwork of a face-to-face survey, we have estimated a negative average effect of these violent actions on support for the 15M movement of about 12 percentage points. Our identification strategy arguably allows us to provide a credible estimate of the effect, with a high degree of internal and external validity. However, we must acknowledge that it has some limitations, especially referred to the exclusion restriction: by relying in a real-world setting, we can only estimate the compound effect of all the events that happened at the moment together with the violent outbreak: the police intervention, the riots themselves and the reactions by political actors and the media.

Further research could try to pursue at least three different questions that our research leaves unanswered. First, it could disentangle the consequences of the different actions and reactions of the actors involved in a riot episode, perhaps using a more stylized experimental setting that would of course come at the expense of external validity. Further research could also try to estimate the duration of these affects over time, considering larger time spans than what our research design allows us to do. Finally, further research could also explore more in depth the mechanisms through which violent tactics operate when reducing public support. Here special attention should be granted to the role of different sources of information regarding the events. We could expect different consequences for individuals that witness violence directly (for instance hearing the riots from home, or seeing the damage the day after), through personal conversations with friends or relatives, from social media, or from mainstream media.

We have also argued that, if we take into account what we know about psychological reactions to political events, we should not expect all citizens to react in the same way, even if they share this general dislike of violence. The consequences of violent outbreaks on citizens' support is be conditioned by their predispositions towards the movement. These predispositions spur processes of motivated reasoning, and make some framing efforts by movements resonate more among those that are better predisposed, to some extent armoring them against the potentially damaging consequences of the use of violence on support.

Our data indeed show that the effect of violence is reduced to 6 points for core supporters. While core supporters are still negatively affected by exposure to the violent episodes, the effect is far smaller than for other citizens that are not as close or oppose the movement. This points to the idea that movements are in some way able to shield their core groups of support from the negative effects of the use of violent tactics -for example, through framing the event in a way that underscores police repression-, but find a harder time with those that are not as close.

Given our design, we had to rely on a rough proxy of pre-treatment support status. While this proxy does not allow us to test our hypothesis on heterogeneous effects in the best possible way, we argue that it allow us to test it in a sufficiently adequate way, while at the same time keeping a high level of external validity. Further research should overcome this limitation by relying on alternative strategies, such as the use of longitudinal data or survey experiments with pre-treatment baseline measures of support.

One could also think that core supporters are capable of distinguishing between moderate and radical flanks within the movement. As it often happens often, in this case the rioters represented a specific fraction of the movement, not the movement in its entirety. This subtle distinction can be understood by those that have more familiarity with the movement, but might be more difficult to grasp for the rest of the population. We regard this argument as a complementary mechanism that might contribute to explaining the heterogeneous effects in relation to the framing effects: often the movement itself may portray trouble-makers as a small, non-representative fringe of the movement so as to divert blame, while other actors might have an interest in identifying the violent faction with the whole social movement as such.

More generally, our results point to the fact that, through the use of violent tactics, social movements might keep their core bases of support but risk losing the sympathy of less committed citizens, alienate those which display lower support levels, and increase antagonism of those that already are distant from the movement. This points to a clear dilemma, common with other political actors, including parties: the decisions that movements may take if they care primarily about their core supporters are different than those that they would take if they are concerned about the rest of the population's opinion. The radicals might not dislike some violent tactics as much as other groups do. This fact, together with the need for tactical diversification (Cunningham & Fruge, 2017) might explain why the radical flanks of mass protest movements often resort to violent tactics, despite them being very costly for the participants and, potentially for the movement as a whole, and even organizations that are close to the movement but not directly part of it.

Our results are expected to hold in other similar situations where a radical flank carries out some violent actions in a nonviolent movement. Our case is not an example of armed conflict, which is the kind of violence analyzed by much of previous work on the consequences of violence for social movements success. The *banc expropriat* riots can even hardly be considered as systematic violent tactics. Rather, they were an isolated violent outburst within an nonviolent movement. In spite of this, the consequences of such limited violence for public support for the movement seem to be large and robust. This may be partly due to the fact that the 15M was a very

popular movement in Spain, that gathered high levels of social support, hence with a wide margin to decrease. Moreover, the fact that the episode of violent protest we are analyzing was rather unexpected and isolated might also contribute to the effect. Finally, one might also argue that the memory of the ETA terrorist campaigns might have increased the social rejection of political violence in Spain.

In spite of these particular characteristics of the case, we expect our results to basically hold for other types of conflicts, for instance those based on ethnic divisions in society. In our case the *15M/indignados* movement has had major implications for party system change, and so support for this movement is today largely a partisan issue. At least in Western democracies parties are strong group definers that often produce stronger attachments than other social groups based on language, religion, ethnicity or region ([Westwood & Strijbis, 2018](#); [Martini & Torcal, forthcoming](#)). Further research should subject this expectation to empirical scrutiny, exploring ways in which contextual characteristics may condition the effect of violence over public support for social movements.

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Replication data

The dataset, codebook, and do-files for the empirical analysis in this article, along with the Online appendix, can be found at <http://www.prio.org/jpr/datasets>.

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Appendices

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A Balance

A.1 Entropy balancing

As a complement to the multivariate regression approach, we also applied the data pre-processing method known as entropy balancing, as described by [Hainmueller \(2012\)](#) and [Hainmueller & Xu \(2013\)](#). Entropy balancing produces a set of weights that balance the treatment and control distributions on a vector of covariates. Crucially, it balances them in the first, second and third moments of the distribution. It reduces model dependence, as compared to the multivariate regression framework in which several assumptions of the functional forms of the relationships are made.

Table A.1 shows the treatment and control moments of the distribution across a range of variables. The main, statistically significant unbalance found is related to the share of weak supporters in the treatment group, that is higher than in the control group. There are also marginally significant imbalances in gender composition and latitude. Table A.2 compares treatment and control after the balancing, showing full balance.

Table A.1. Pre-weighting Balance

| | Pre-riots | | | Post-riots | | |
|-----------------|-----------|----------|----------|------------|----------|----------|
| | mean | variance | skewness | mean | variance | skewness |
| Core Supporters | 0.30 | 0.21 | 0.89 | 0.28 | 0.20 | 0.96 |
| Weak Supporters | 0.34 | 0.22 | 0.68 | 0.36 | 0.23 | 0.57 |
| Opposers | 0.29 | 0.21 | 0.93 | 0.30 | 0.21 | 0.85 |
| Age | 51.83 | 300.50 | -0.03 | 50.89 | 352.90 | 0.02 |
| Refusals | 5.73 | 68.65 | 3.12 | 6.32 | 94.90 | 5.24 |
| Gender | 0.55 | 0.25 | -0.21 | 0.53 | 0.25 | -0.10 |
| Gracia | 0.10 | 0.09 | 2.70 | 0.07 | 0.07 | 3.31 |
| lat | 41.41 | 0.00 | 0.19 | 41.41 | 0.00 | 0.19 |
| lon | 2.17 | 0.00 | 0.07 | 2.17 | 0.00 | 0.04 |

B Full tables with interaction

Here we present the full table of the interaction on which we base our estimation of the heterogeneous effects. It shows how, relative to the core supporters, the effect is significantly larger among the weak supporters and the non-aligned. Moreover, use two alternative moderators that are allegedly more stable than vote choice: party identification and left-right self-placement. While there are reasons to think that these are stable attitudes unlikely to be affected by such an event, we privilege in

Table A.2. Post-weighting balance

| | Pre-riots | | | Post-riots | | |
|-----------------|-----------|----------|----------|------------|----------|----------|
| | mean | variance | skewness | mean | variance | skewness |
| Core Supporters | 0.30 | 0.21 | 0.89 | 0.30 | 0.21 | 0.89 |
| Weak Supporters | 0.34 | 0.22 | 0.68 | 0.34 | 0.22 | 0.68 |
| Opposers | 0.29 | 0.21 | 0.93 | 0.29 | 0.21 | 0.93 |
| Age | 51.83 | 300.50 | -0.03 | 51.82 | 300.50 | -0.02 |
| Refusals | 5.73 | 68.65 | 3.12 | 5.73 | 68.65 | 4.14 |
| Gender | 0.55 | 0.25 | -0.21 | 0.55 | 0.25 | -0.21 |
| Gracia | 0.10 | 0.09 | 2.70 | 0.10 | 0.09 | 2.70 |
| lat | 41.41 | 0.00 | 0.19 | 41.40 | 0.00 | 0.34 |
| lon | 2.17 | 0.00 | 0.07 | 2.17 | 0.00 | 0.04 |

the main text vote recall in order to avoid potential post-treatment bias. With party identification we use a similar classification scheme as with vote recall, but including CUP supporters to the core group (in the previous case we cannot use them because the CUP does not run in Spanish elections). Finally, with left-right, we distinguish between those in the leftmost positions of the scale (0-2), those in the center-left (3-4), center(5), center-right (6-7) and far-right (9-10).

Figure B.1 presents the average marginal effects using partisanship as moderator, and figure B.2 shows the results with self-reported ideology as a measure of predisposition towards the movement. Results are generally consistent across specifications, with the effect being always small -and in these cases, non significant) for the core supporters, and stronger for the weak supporters.

C Robustness Checks

C.1 Reachability

This is a serious threat, especially in the nominal samples in which a specific individual is selected and contacted repeated times until it can be effectively interviewed. In this case, however, we sampled addresses and not individuals, and the fieldwork protocol did not include multiple attempts at interviewing specific individuals. Refusals were substituted by close neighbors using random routes departing from the selected address.

In spite of this it might still be the case that some confounding effect of reachability biases the estimates. While this is, by definition, difficult to assess, we can proxy for it using the number of refusals collected before every completed survey.

Table B.1. Heterogeneous Treatment Effects

| | (1) | (2) | (3) |
|----------------------------|--------------------|--------------------|--------------------|
| | Past vote | Partisanship | Ideology |
| Post | -0.04 (0.03) | -0.02 (0.04) | -0.08* (0.05) |
| Weak Supporters | -0.27*** (0.04) | -0.24*** (0.05) | |
| Opposers | -0.54*** (0.05) | -0.50*** (0.05) | |
| Non-aligned | -0.34*** (0.13) | -0.34*** (0.05) | |
| Post \times Weak | -0.12* (0.07) | -0.16** (0.08) | |
| Post \times Opposers | -0.09 (0.07) | -0.15** (0.07) | |
| Post \times Non-aligned | -0.30* (0.16) | -0.06 (0.07) | |
| Center-left | | | -0.07 (0.05) |
| Center | | | -0.38*** (0.06) |
| Center-right | | | -0.46*** (0.07) |
| Right | | | -0.47*** (0.10) |
| Post \times Center-left | | | -0.06 (0.07) |
| Post \times Center | | | 0.09 (0.08) |
| Post \times Center-right | | | -0.05 (0.10) |
| Post \times Right | | | -0.14 (0.13) |
| Constant | 0.94*** (0.02) | 0.92*** (0.02) | 0.84*** (0.03) |
| R-squared | 0.247 | 0.185 | 0.152 |
| N | 848 | 1173 | 1102 |

* p<.1. ** p<.05. *** p<.01

Controls include age, gender, ideology

Reference category: Core

As it can be seen in figure C.1, the average number of refusals is relatively constant around 5 with three exceptions in specific days: one before the riot outbreak, one after, and the final day of the fieldwork in which it took 32 attempts to complete the final two questionnaires. The other two peaks correspond to Saturdays, while the final one might be related to the quota sampling procedure. We will take this into

Figure B.1. Treatment effects, by partisanship

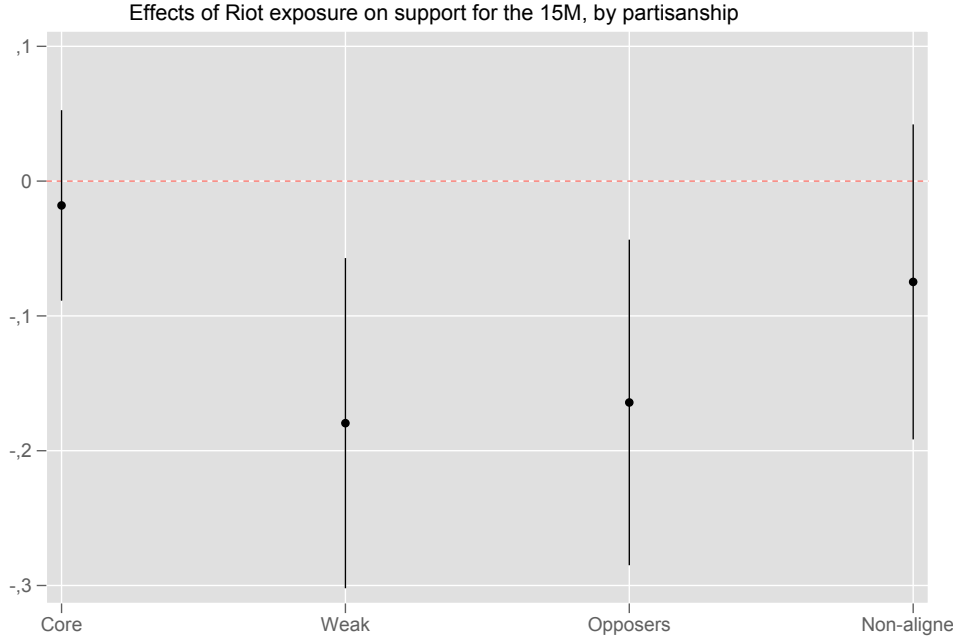
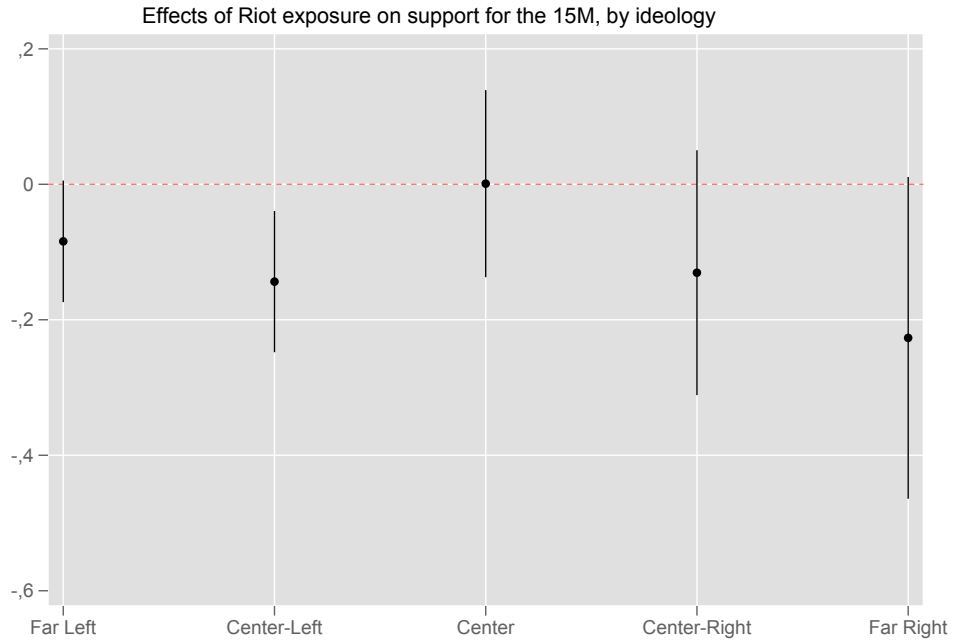
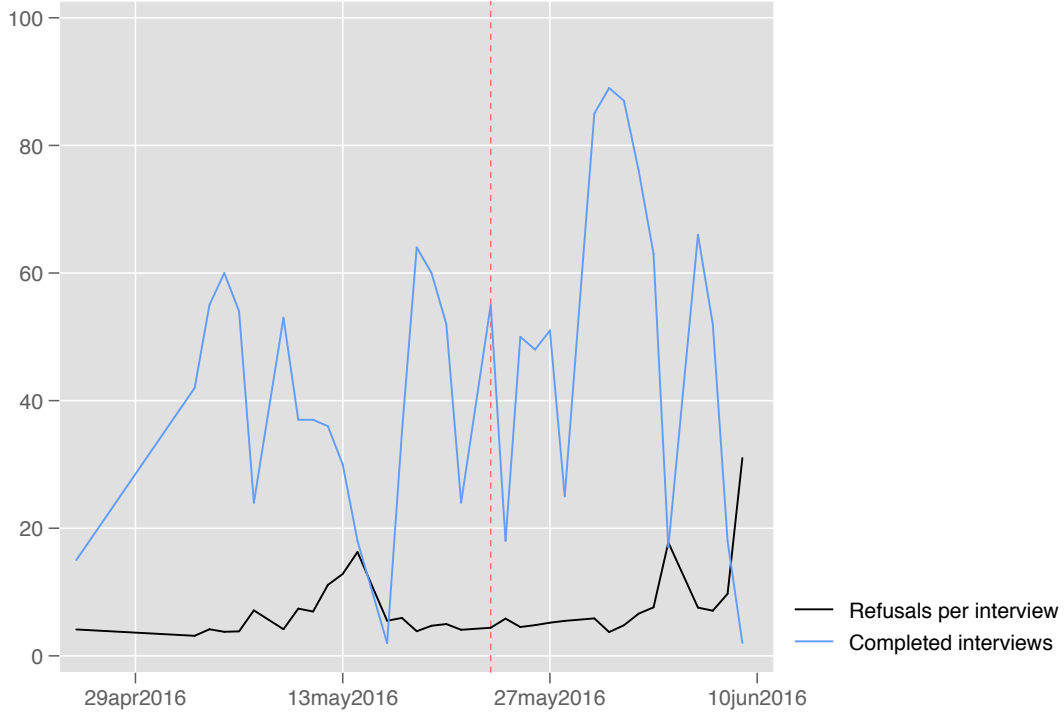


Figure B.2. Treatment Effects, by Ideology



account in the estimation. In any case, the difference between the average number of refusals per survey pre-riots (5.6) and post-riots (6.16) is not statistically significant ($t = -1.25$; $p = 0.211$) so as far as we can test, there isn't a serious problem of imbalance on reachability.

Figure C.1. Interviews and refusals by day



C.2 Time trends

A possible concern with our research design is the existence of unrelated time trends that covary with our treatment of interest and that might be driving our results. In table C.1 we present some robustness checks that allow us to rule out this possibility. Column 1 re-estimates the main model with a linear time trend (centered around the first day of riots) and its interaction with the treatment. Both the linear trend and the interaction have non-significant and negligible effects on our outcome, while the treatment maintains its effect. In columns 2 and 3 of this table we test the effect of the trend in the pre and post-riot samples, to show that there is no apparent time trend within each of these groups. Therefore, the net effect of time on our outcome of interest can be attributed to the treatment.

In columns 4 to 6, we further deal with the issue of time trends using alternative treatments. The model in column 4 uses an arbitrary placebo date located at the empirical median of the control group (as suggested by [Imbens & Lemieux \(2008\)](#) for the case of regression discontinuity designs) and estimates its effect for the control group. This partition of the sample does not show any significant difference between the pre and post-placebo groups.

In columns 5 and 6 we use a less arbitrary date: the 5th anniversary of the 15M movement, that occurred during the survey, one week before the riot outbreak. This

event got some (scant) attention in the press, as shown by our front page analysis in section D. As this could be a potential confounder, we test the effect of this alternative date. In column 5 we show how the anniversary does not have any effect, once we condition for the riot outbreak. In column 6 we investigate the effect of the anniversary only within the control group, and find no effect whatsoever.

Table C.1. Time trends and placebo dates

| | Linear trend | | | Placebo dates | | |
|--------------------|--------------------|-------------------|-------------------|-----------------------|------------------------|-------------------|
| | (1) Interaction | (2) Pre | (3) Post | (4) Median control | (5) 15M Anniversary | (6) |
| Post-riot | -0.14* (0.08) | | | | -0.11** (0.05) | |
| Days | -0.00 (0.00) | -0.00 (0.00) | 0.01 (0.01) | | | |
| Post \times Days | 0.01 (0.01) | | | | | |
| Placebo date | | | | -0.04 (0.05) | | |
| 15M anniversary | | | | | -0.03 (0.05) | -0.03 (0.05) |
| Constant | 0.63*** (0.05) | 0.63*** (0.05) | 0.49*** (0.07) | 0.70*** (0.03) | 0.69*** (0.03) | 0.69*** (0.03) |
| R-squared | 0.020 | 0.005 | 0.003 | 0.002 | 0.017 | 0.001 |
| N | 848 | 464 | 384 | 464 | 848 | 464 |

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

All models with entropy balancing weights as defined previously, and precinct-clustered SE

C.3 Gràcia effect

One obvious way of providing additional credibility to our estimated effect is to check whether it varies with intensity of exposure. If our treatment is capturing exposure to the riots, and therefore we are indeed estimating the effect of the use of violence on support for the 15-M movement, we should expect the treatment to have a stronger effect on those that were more exposed to it, namely the residents of the district of Gràcia. We have 94 Gràcia residents in our sample, 52 interviewed before the riots and 42 after the beginning of the violent outbreak in the neighborhood. In table C.2 we present the results of a model in which treatment is interacted with residence in Gràcia. We use the weights produced by the entropy balancing to keep balance in observables. The interaction term is statistically significant, and indeed the marginal effects for Gràcia predict a drop of support of 39 percentage points, far stronger than the 9 points for the rest of the city.

Table C.2. Gràcia Conditional Average Treatment Effect

| | (1) |
|----------------------|--------------------|
| Post-riot | -0.10*** (0.04) |
| Gràcia | 0.11 (0.07) |
| Post \times Gràcia | -0.30*** (0.10) |
| Constant | 0.67*** (0.03) |
| N | 848 |

* p<.1, ** p<.05, *** p<.01
Estimates after entropy balancing

Table C.3. Alternative operationalizations of treatment

| | (1) | (2) |
|------------|--------------------|--------------------|
| Post-start | -0.10*** (0.03) | |
| During | | 0.06 (0.06) |
| After | | -0.13*** (0.03) |
| Constant | 0.68*** (0.02) | 0.68*** (0.02) |
| N | 906 | 906 |

* p<.1. ** p<.05. *** p<.01

C.4 Alternative operationalization of treatment

In the main text we have used a dichotomous operationalization of the treatment variable, that divides the sample between those that were interviewed before and after the riots. However, the conflict extended over four nights, so the *treatment* is not as sharp and the effects might not be immediate. To account for that, in table C.3 we use two alternative operationalizations: one based on a dummy variable that takes value one if the respondent was interviewed after the start of the riots and zero otherwise, and another one based on three categories: one for those interviewed before the start of the riots, one for those interviewed during the riots (N=98) and one for those interviewed after the riots (N=631). Results show that the effect is concentrated after the end of the rioting week, which might point to the fact that it took some time to materialize.

C.5 Alternative outcomes and Placebo Tests

To lend additional credibility to our causal identification strategy, in this section we present a set of placebo tests. Results are shown in table C.4. Following the same model specification as in the main set of results, we show how the treatment (exposure to the riots) does not affect support for other social movements, not linked to the riots: the AAVV neighborhood associations (quite relevant in Barcelona), the anti-eviction movement PAH or the large-scale pro-independence movement National Assembly of Catalonia (ANC).

We also show how the riots did not have any significant effect on external political efficacy, as measured by the question *"To what extent would you say that the Spanish political system allows people like you to have an political impact?"*. Although we are limited by the questions included in the survey, this is suggestive evidence that the riots and the police response did not affect the citizens' mood towards the political system in general. We interpret this, with caution, as an indication that our treatment is primarily capturing the effect of the riots.

Table C.4. Alternative outcomes

| | (1) AAVV | (2) ANC | (3) PAH | (4) Ext. Efficacy |
|------------|-------------------|-------------------|-------------------|----------------------|
| Post-Riots | -0.04 (0.04) | -0.04 (0.06) | 0.01 (0.05) | -0.04 (0.06) |
| Constant | 0.55*** (0.03) | 0.87*** (0.04) | 0.99*** (0.03) | 0.85*** (0.04) |
| N | 732 | 721 | 801 | 924 |

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

Entropy balancing weights and precinct-clustered SE

C.6 Non-response

A potential threat to our identification strategy might come from non-response. Some respondents may refrain from answering the outcome variable precisely because of the treatment. This creates a situation of attrition that might bias our estimates, because the missigness is related to the potential outcomes (Gerber & Green, 2012).

Overall, we have a 17.1% of respondents who declared not to know the 15M movement, and therefore, they did not answer the question on sympathy. In the pre-riots group we have a 15.8% of *Don't knows* and in the post-riots group the figure grows up to 18.5%. A two-sample test of proportions reveals that the difference is only

marginally significant in a one-tailed test ($Pr(Z > z) = 0.07$). However, the effect disappears when we introduce controls, especially the neighborhood fixed-effects. This suggests that the differences in levels of *don't knows* are related to compositional sampling effects rather than a by-product of the riots. The very limited nature of the riot outbreak makes it highly implausible that the street agitation created an environment in which some respondents refrained from expressing their opinion towards the movement.

Table C.5. Missing values: Don't know the movement

| | (1) | (2) | (3) | (4) |
|--------------------------|-------------------|--------------------|-------------------|-------------------|
| Post-riots | -0.08 (0.15) | -1.03*** (0.31) | -0.81** (0.40) | -0.88 (0.64) |
| Political knowledge | | 0.94*** (0.19) | 1.20*** (0.37) | 0.94*** (0.19) |
| Lef-right self placement | | -0.18** (0.08) | -0.18** (0.08) | -0.16 (0.11) |
| Post × Knowledge | | | -0.36 (0.42) | |
| Post × Ideology | | | | -0.04 (0.13) |
| Age | | -0.02** (0.01) | -0.02** (0.01) | -0.02** (0.01) |
| Gender | | 0.32 (0.30) | 0.32 (0.30) | 0.32 (0.30) |
| Weak Supporters | | 0.11 (0.43) | 0.12 (0.43) | 0.11 (0.43) |
| Opposers | | 0.09 (0.49) | 0.11 (0.49) | 0.10 (0.49) |
| Non-aligned | | -0.65 (0.60) | -0.62 (0.60) | -0.63 (0.60) |
| Constant | 1.83*** (0.10) | 3.56*** (1.01) | 3.44*** (1.02) | 3.46*** (1.07) |
| Neighborhood FE | | | YES | YES |
| Pseudo-R2 | 0.000 | 0.160 | 0.162 | 0.160 |
| N | 1402 | 887 | 887 | 887 |

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

If we analyze the patterns of missigness, we observe that the share of respondents that declare not to know the movement is higher among the less politically knowledgeable and also higher among the citizens located at the right. However, as shown in table C.5, these predictors of missigness do not interact with our treatment: rightist, or less politically aware respondents are not any more likely than the rest of the sample, to declare that they do not know the movement after the start of the riots.

Given this coavariates of missingness, we can expect that, if anything, the higher

rate of *don't knows* in the post-riots group provokes a downward bias in our estimates given that those that declare not to know the movement are more similar to those that oppose it.

We can use multiple imputation to check the robustness of our results. Using the relevant predictors of support for the 15M movement (ideology, partisanship, age, gender and neighborhood) we have imputed the missing values in our dependent variable. Results are presented in table C.6 and show how the effect is robust to the imputation of missing values.

Table C.6. Multiple imputation models

| | (1) | (2) | (3) |
|-------------|--------------------|--------------------|--------------------|
| | mi_1 | mi_2 | mi_3 |
| Post-Riots | -0.10*** (0.03) | -0.10*** (0.03) | -0.11*** (0.03) |
| Controls | | YES | YES |
| District FE | | | YES |
| Constant | 0.65*** (0.02) | 0.70*** (0.09) | 0.58*** (0.12) |
| R-squared | . | . | . |
| N | 1244 | 887 | 887 |

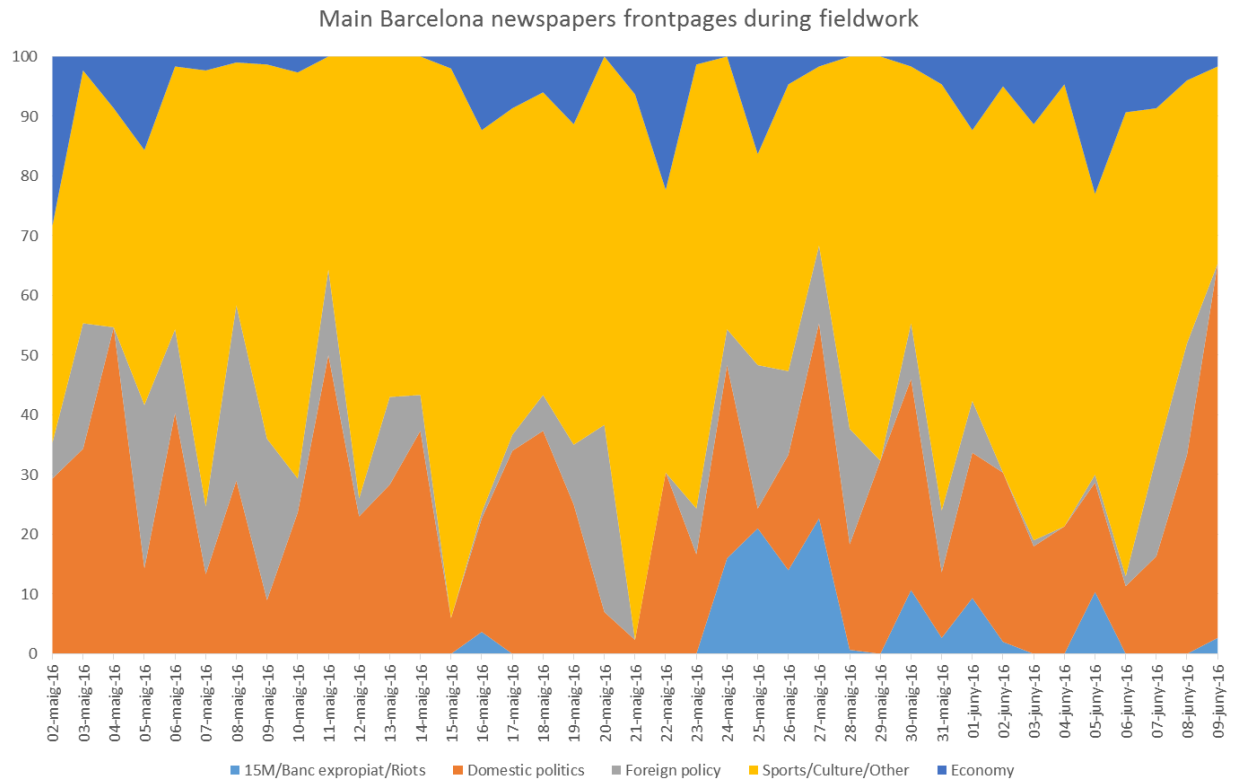
* p<.1, ** p<.05, *** p<.01

D Newspapers

We reviewed and coded the frontpages of the three main Barcelona-based newspapers (La Vanguardia, El Periódico and Ara) during the whole period of the fieldwork, in order to make sure that the event was indeed salient in the news, and that during the period there were no simultaneous events potentially affecting our outcome. In figure D.1 we present a summary of the contents day-by-day.

The riots (clear blue) made an appearance during a few days in the frontpages, but other than that there was no other reference to the movement except for a small piece on its anniversary on the 15M in one newspaper (el Periódico). The reference was a small announcement of a special edition of the lifestyle section of the newspaper, that was devoted to the legacy of the 15M movement (in a generally positive tone). No other news during the period covered topics that would plausibly affect citizens' attitudes towards the movement. The full sample of frontpages, and the detailed coding is available upon request.

Figure D.1. Frontpage topics



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