‘If a fight starts, watch the crowd’:
The effect of violence on popular support for social movements

Jordi Muñoz
Department of Political Science, University of Barcelona

Eva Anduiza
Department of Political Science and Public Law, Universitat Autònoma de Barcelona

Abstract
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 movement as a whole. To estimate the causal effect of violence, it exploits an unexpected riot outbreak that occurred during the fieldwork of a face-to-face survey in Barcelona in May 2016, led by a squat group linked to the anti-austerity movement known as the 15-M 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 15-M movement by 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
15-M, indignados, natural experiment, protest, radical flanks, social movements, violence

Introduction
Following Donald Trump’s inauguration in early 2017, a wave of protests swept the USA. 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 civil rights movement in the 1960s, to the labor movement or even the anti-Nazi resistance in Germany: some of their 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, Dahl & 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 participants, often the choice of a specific set of protest instruments is derived 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 of 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 fundraising 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 unexpected outbreak 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 five days at the end of May 2016. The squat center was associated with the so-called 15-M 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 15-M 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 checks. 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, which are based on a real-world case of violent protest which, as 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 vs. 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 (Cloward & Piven, 1979; Fishman & Everson, 2016) 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 nonviolent 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 movements’ goals than nonviolent ones (Chenoweth & Cunningham, 2013; Howes, 2013; Huet-Vaughn, 2013; Stephan et al., 2008). Armed resistance from radical flanks seems 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 the discourses of the elite based on public order maintenance (Wasow, 2017), reinforcing the opponent (Howes, 2013), facilitating repression from the state (Soule et al., 2004; Stephan et al., 2008; Tompkins, 2007), and reducing 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 therefore it is a high cost activity and hence depresses participation in...
movement campaigns (Chenoweth & Schock, 2015; Tompkins, 2007). Successful movements need a broad base which can seldom 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 that 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 anticipates the movements’ 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 the actions of social movements, consequent for the accomplishment of their objectives, and which can be put at risk with the implementation of violent tactics.

The question of how violent tactics affect public support for a movement remains underexplored. Most of the attention has been devoted to how violent vs. nonviolent tactics impact on policy, leaders or regime change (Chenoweth & Schock, 2015; Enos, Kaufman & Sands, 2017; Huet-Vaughn, 2013; Soule et al., 2004; Tompkins, 2007). 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 (e.g. 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 (e.g. Rohrschneider, 1990) consider mostly demand-side factors, that is, how individual characteristics relate to support for social movement, but not supply-side factors such as the movements’ actions.

A few recent works have tackled the broader question of how protests affect individual attitudes. 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 to civil rights protests affects support for the civil rights movement. They found a positive effect among some white individuals, contingent upon contextual characteristics (Andrews, Beyerlein & 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 find 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 black insurgency in the 1960s 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 led 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 out that ‘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’ (2015:22). The Young and Ponce and Pasquale studies both 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 allows a precise estimation of the 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). While Sharp mostly focuses on rulers or the state when developing his conceptualization of the sources of power, 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 its 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 the idea of Feinberg and collaborators that violence makes it difficult for bystanders to identify with movement activists (Simpson, Willer & Feinberg, 2018). Since collective identity is one of the most important predictors 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) argue, movement 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 it may be perceived as justified in certain circumstances, there is a negative connotation associated with the concept. The ethical dimension may be secondary or irrelevant for some movement activists, but it is 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 they deploy violent tactics.

Violent radical flanks, however, may have positive effects by affecting the perceptions that both the public and decisionmakers 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 for the loss of support produced by violent radical flanks. However, this potentially positive radical flank effect (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 off by the use of violent tactics.

Simpson, Willer & Feinberg (2018) already provide some support for this idea, using survey experiments where they manipulate the extremity of protest behavior of different hypothetical groups with different repertoires of action. They find the expected negative effect of extremeness on support, with identification with the movement as the 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.

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. When there are protests and associated violence, 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 delegitimize 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 overexposed 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 confirm their prior views, thus reducing exposure to contradicting information. Third, 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, Lodge & Glahtar, 2001: 208–209).

The adoption of this perspective for the attitudinal consequences of protest violence leads 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 will 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 can 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 movement’s interpretation of the events. Therefore we expect the impact of 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 public opinion will also be negatively affected by violence, as long as they pay sufficient attention to the violent events and have some attitude 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

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

Empirics

**The case: The 15-M movement in Barcelona and the 2016 Gràcia riots**

Our research strategy takes the case of the 15-M movement, and a series of related riots that took place in May 2016 in Barcelona. We may consider Barcelona a city in which the 15-M 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 demonstration date (15-M: 15 May), started with a series of protests 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 15-M 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 led to the rise of the new leftist party Podemos and several local platforms, such as Barcelona en Comú.

In the wake of the 2011 15-M 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 these 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 decentralization of the 15-M movement, its Gràcia local chapter met there too. The owner of the premises, the savings bank Cata-lunya 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 a new eviction. That same year Mayor Trias had failed to achieve 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 23 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 police officers 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 15-M had 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. 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 15-M 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 15-Mpedia (15-Mpedia, 2016). The Banc Expropiat blog refers to itself as a venue for the Gràcia chapter of the 15-M meetings in the cooler and rainy weather of autumn (Banc Expropiat, 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 between the two organizations would work 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 geolocation of respondents interviewed before and after the riots.

Identification strategy: Assumptions and threats

We exploit the unexpected occurrence of these riots connected to a local group of the 15-M 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 assumption is ignorability of treatment assignment. Since assignment to treatment and control groups is not random or 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 geolocation 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 1 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

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2 A summary of different protest events related to the 15-M 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.
any case we will take into account these imbalances in the estimation.

A potentially more troublesome violation of the ignorability assumption is the imbalance on unobservables. In this context, one might think that the main unobservable that can correlate with timing of the interview is respondents’ reachability: those (types of) respondents that are easier to reach and more cooperative are interviewed first, and those that are less reachable are effectively interviewed at the end of the fieldwork period. If these types are systematically different in their attitudes towards the movement, or in their general patterns of acquiescence in surveys, this might confound the effect. We address this concern by analyzing the patterns of disposition to respond to the survey over time (see the Online appendix), as well as through the inclusion of a control for the number of contact attempts before each interview in our preferred specifications.

**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 assumption can be violated under two circumstances. The first 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 three main Barcelona newspapers during the fieldwork and there were no other news items directly concerning the 15-M movement that were on the media front pages. The Online appendix contains a detailed description of the front pages of the three main Barcelona newspapers during the fieldwork.

Figure 1. Pre- (red) and post-riot (green) interviews
*Source: Author using OpenStreetMap (©OpenStreetMap contributors, licensed under CC BY-SA terms).*
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 real world social movements, violent tactics do not appear in isolation but are 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 accuracy and credibility of the estimates. More specifically, in the Online appendix we show, among others, 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.

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      |

Data: Sampling and measurement

The data were collected in the context of the research project Pathways to Political Inclusion. The survey that was fielded between 9 May and 9 June 2016 on a sample of 1,500 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 survey were census tracts within each neighborhood. Within these 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 sociodemographic controls.

The outcome variable (support for the 15-M 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 of it, and whether they sympathized 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 not to use the respondents that were interviewed during the days the riots were taking place, since we could misclassify them as treated when they had
not yet received the information. However, in the Online appendix we show how our results hold if we consider them as treated.

A key moderator to test our theoretical argument is predisposition towards the movement. In order to test this, we need to specify an interaction between movement 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, Nyhan & Torres, 2016).

Given our data, the best option to estimate the heterogeneous effects is to use a proxy for movement support that is likely unaffected by the treatment. Therefore, we rely on vote recall in the 2015 general election 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 us 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. We code voters of the pro-15-M 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 cast a blank ballot are coded separately as non-aligned.

Because we are referring to parties’ relation towards the 15-M and not to specific individual attitudes towards the 15-M, and because this movement was quite popular (see above), we may still find some support towards the 15-M among voters of the right and center-right parties.

Results

Table II shows the raw proportions of support for the 15-M movement before and after the riots. As it can be observed there is a ten percentage-point drop in support among respondents interviewed after the violent outbreak. The proportion of respondents that sympathized with the 15-M 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 15-M.

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 three models are linear probability models estimated through OLS regressions, and the final model is based on entropy balancing weights, 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 Models 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 to the exclusion restriction, understood as the absence of simultaneous political events that might confound our effect, is restricting the time frame around the riot outbreak. The probability that some other event is driving our results will decrease as we narrow down the number of days we take into consideration. Given that our data come from a face-to-face survey, the fieldwork was relatively long: it lasted from the 25 April to 9 June 2016. Therefore, a useful robustness check might come from narrowing it down.

Since the choice of the time window can be arbitrary, in Table IV we present, for the sake of transparency, the estimate with a range of time windows, from one day before and after the riots started, to 11 days. As can be observed, the effect is negative, of a similar magnitude, and statistically significant down to the 5+5 days window. If we take four or three 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 drop in public support, but rather a progressive change 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 15-M), which gives us some additional confidence that we are actually capturing the riot effect. Overall, the whole set of events generated a substantial decrease in sympathy for the 15-M of 12 percentage points, providing evidence in line with our first hypothesis.

Additionally, in 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. In addition, after the riots we do not see any time trend at all, so we have no evidence of recovery of support for the 15-M. This speaks to the duration of the effects, but of course the post-riot fieldwork period may be too short to observe such a recovery.

**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 recall 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 15-M movement before and after the riots, split by past-vote groups. As 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.

This result goes in line with our expectations. However, in this subgroup 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 weights 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 core supporters (−6.8 percentage points) than for weak supporters (−15.5), the opposers (−13), and the nonaligned (−15.5). These heterogeneous effects are robust to the use of alternative moderators, such as ideology or partisanship, as shown in the Online appendix.

Conclusions

Research on social movements increasingly converges towards the finding that nonviolent protest movements tend to be, in the long run, more successful in reaching their goals and promoting policy change than violent movements. But, as social movements are often constellations of organizations, we also know that there are strong incentives for intramovement tactical diversification (Cunningham, Dahl & Fruege, 2017).

In this article we have explored a likely mechanism through which the use of violent tactics might harm a movement’s 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 that 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 15-M 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 on a real-world setting, we can only estimate the compound effect of all the events that happened at the same time as with the violent outbreak: the police intervention, the riots themselves, and the reactions of 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 effects over time, considering larger time spans than what our research design allows us to do. Finally, further research could also explore in more 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 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 armorng 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 six 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 this is harder to achieve 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, it allows us to test it in an 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 argue that core supporters are capable of distinguishing between moderate and radical flanks within the movement. As is often the 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 troublemakers 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.

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 who display lower support levels, and increase antagonism of those that are already 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 from those that they would take if they are concerned about the opinions of the rest of the population. The radicals might not dislike some violent tactics as much as other groups do. This fact, together with the need for tactical diversification (Cunningham, Dahl & Fruge, 2017), might explain why the radical flanks of mass protest movements often resort to violent tactics, despite their being very costly for the participants and, potentially, for the movement as a whole.

Our results are expected to hold in other similar situations where a radical flank carries out some violent actions in an otherwise nonviolent movement. Our case is not an example of armed conflict, which is the kind of violence analyzed by much of the previous work on the consequences of violence for the success of social movements. The Banc Expropiat riots can hardly be considered as systematic violent tactics. Rather, they were an isolated violent outburst within a 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 15-M was a very popular movement in Spain, that gathered high levels of social support, and hence had a wide margin to drop. 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 hold for other types of conflicts, for instance those based on ethnic divisions in society. In our case the 15-M/indignados movement has had major implications for party system change, 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 (Martini & Torcal, 2016; Westwood et al., 2018). Further research should subject this expectation to empirical scrutiny, exploring ways in which contextual characteristics may condition the effect of violence on public support for social movements.

**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. All analyses were conducted using Stata 14.

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**ORCID iD**
Jordi Muñoz https://orcid.org/0000-0002-2078-4047

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JORDI MUÑOZ, b. 1979, PhD in Political and Social Sciences (Pompeu Fabra University, 2009); Ramón y Cajal Senior Research Fellow, Political Science Department, University of Barcelona; member of the Institutions and Political Economy Research Group; research interests: political behavior, corruption and accountability, historical political economy, and experimental methods.

EVA ANDUIZA, b. 1970, PhD in Political and Social Sciences (European University Institute, 1997); Professor of Political Science, Universitat Autònoma de Barcelona; Fellow, Center for Advanced Study in the Behavioral Sciences, Stanford University (2018–19); research interests: political participation, protest, political attitudes.