Protests and Police Militarization

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Abstract

What is the role of the militarization of law enforcement agencies in affecting protest activity in the US? This paper shows that transfers of military equipment from the Department of Defense through the 1033 Program increased protest activity in general, as well as the incidence of protests in a given county in 2020. Our results are driven by demonstrations related to the Black Lives Matter movement, with the increase in protests after the killing of George Floyd in May 2020 four times larger in militarized counties when compared to non-militarized ones. Hence, our results highlight how the recent wave of protests is directly linked to the hotly debated 1033 Program, largely responsible for the excessive militarization of local law enforcement agencies in the past decades.

Keywords: 1033 Program, Militarization, Protests

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"Here's what you need to know: The Law Enforcement Support Office, known as the 1033 Program, allows the transfer of surplus military-grade equipment to local law enforcement agencies across the country.

...

Even as we attempt to grapple with the Derek Chauvin trial, anxiously awaiting a verdict — killings by police have continued. Our screens continue to be filled with state-sanctioned violence against Black people. And when we peacefully protest in the name of Black lives, we're met with violence from police. But when white supremacists stormed the Capitol in January, they were invited in and posed for pictures with police.

...

We are calling on the Biden administration to end the 1033 Program altogether. Send a message to the White House and let them know — #End1033 — by the time President Biden reaches his 100th day in office."

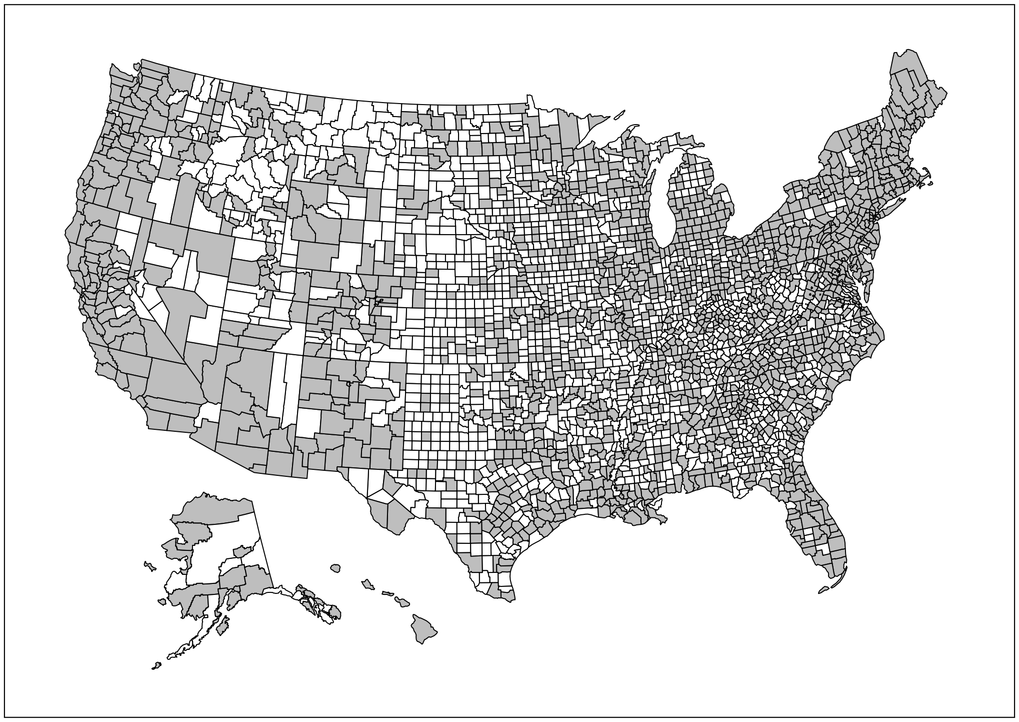
Black Lives Matter (2021)

1 Introduction

Images of protesters on one side of the street with militarized police lined up on the other side are not uncommon in recent years in the United States. The Ferguson unrest in 2014 following the death of Michael Brown, or protests in Minneapolis in May 2020 following the death of George Floyd in police custody are notable examples of social unrest in the last decade (APSR Editors, 2020; Reny and Newman, 2021). Indeed, a large wave of protests spread across the country in 2020, with more than half of all U.S. counties recording at least one, as shown in Figure 1, with more than twenty thousand protests recorded in all of 2020 by the U.S. Crisis Monitor (US Crisis Monitor, 2020). As Figure 2 makes it clear, they are not evenly spread over time with the killing of George Floyd sparking a significant increase in their occurrence. Within this context, this paper aims to understand the role of militarization of law enforcement agencies (LEAs) in determining the incidence of protests.

Indeed, the militarization of LEAs is often linked to protest movements in the political and public debate (Browne, 2020; Lehren et al., 2020) with the 1033 program administered by the Department of Defense to reallocate to local LEAs excess military equipment often criticized for the increase in police militarization and violence in recent years (Delehanty et al., 2017; Lawson Jr, 2019; Tolan and Hernandez, 2020; Masera, 2021a). Figure 3 shows the extent of the 1033 program and the geographical heterogeneity across counties in terms of the value of items allocated.

Figure 1: Incidence of protest by county in 2020

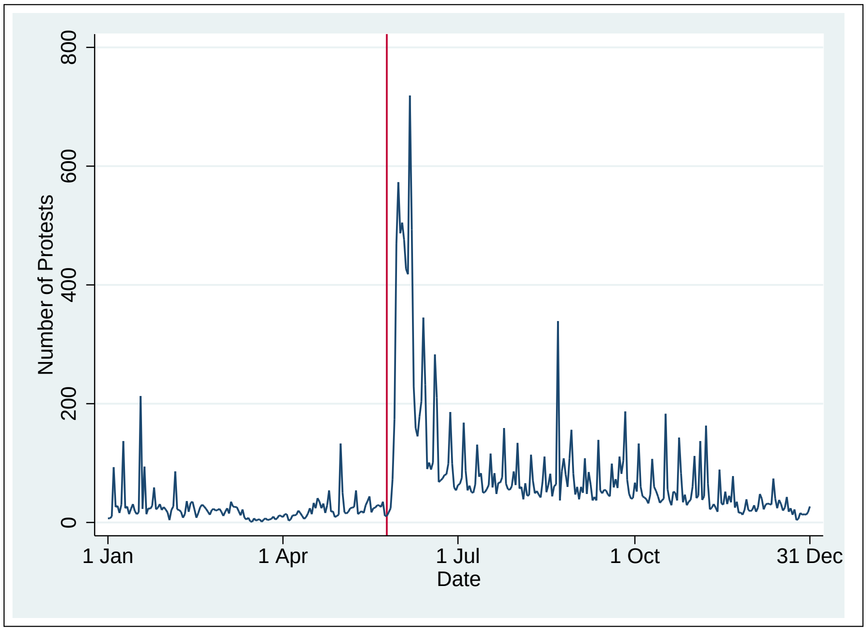


Notes: Gray denotes counties that experienced at least one protest (53% of the total number of counties), white for counties with zero protests.

Protests in 2014 and 2020 were followed by scrutiny of certain aspects of the 1033 program and a broader debate on police defunding. In the aftermath of the Ferguson events, President Obama's Executive Order 13688 recognized that *"[a]t times, the law enforcement response to those protests was characterized as a "military-style" operation, as evidenced by videos and photographs that showed law enforcement officers atop armored vehicles, wearing uniforms often associated with the military, and holding military-type weapons"*.[1] This Executive Order, later revoked by President Trump in 2017, aimed at a better control of the use of military equipment in the hands of LEAs. In the aftermath of the Minneapolis events, brutal police tactics were further debated (H.R.1280 - George Floyd Justice in Policing Act of 2021). President Biden may have not ended the 1033 Program as requested by the Black Lives Matter (BLM) movement (see quote above), but his 2022 Executive Order 14074 *"imposes sensible restrictions on the transfer or purchase with federal funds of military equipment that belongs on a battlefield, not on our streets. The list of prohibited equipment is broader than*

[1]The Executive Order banned LEAs from receiving several types of items: tracked armored vehicles; weaponized aircraft, vessels, and vehicles; grenade launchers; camouflage uniforms used for urban settings; and bayonets. It also required more scrutiny and usage oversight of certain other categories of equipment.

Figure 2: Timeline of number of total protests in the US over 2020



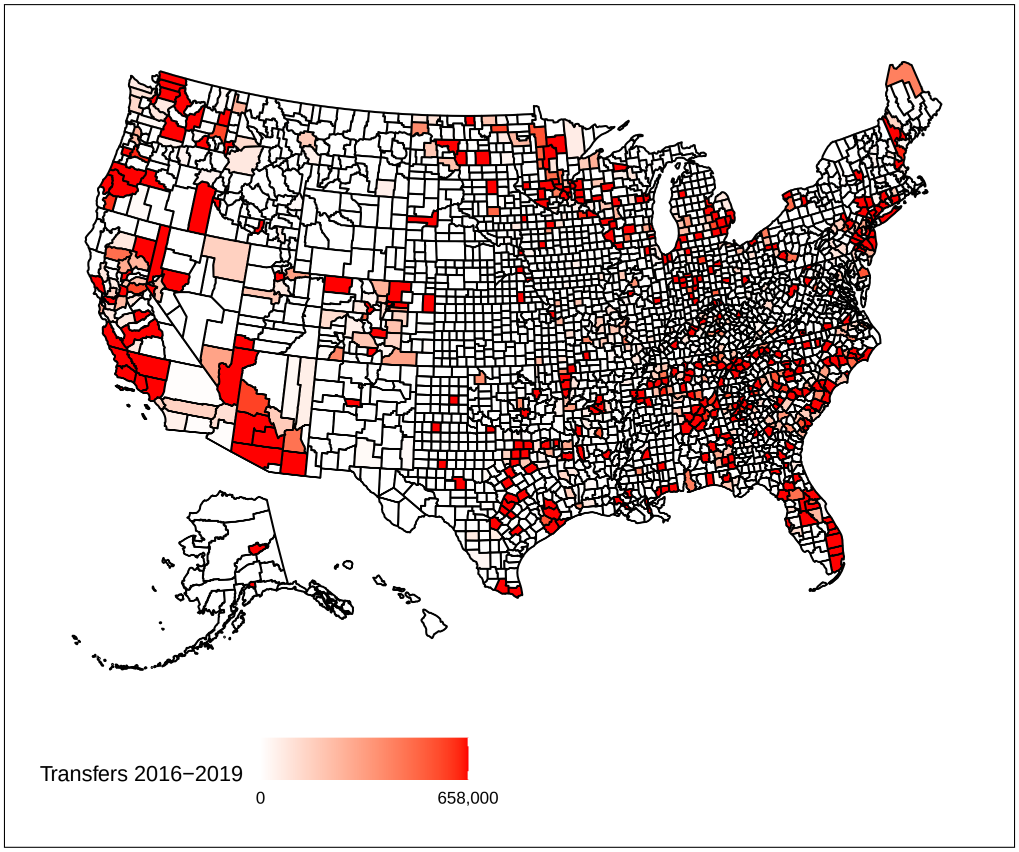
Notes: Red vertical line indicates 25 May 2020.

under the Obama-Biden Administration, and the EO's mandate is broader than the George Floyd Justice in Policing Act (GFJPA)."

Despite the apparent link between militarization of LEAs and protest activity, so far there is no systematic analysis of the effect of the 1033 Program on the incidence of protests. Given that from a theoretical perspective the presence of militarized LEAs and the potential of state repression could *i*) act either way to fuel or deter protests and dissident behavior (Moore, 1998; Davenport, 2007; Davenport et al., 2019), and *ii*) have an heterogeneous effect across different protest types (Davenport et al., 2011), we rely on the 2020 release of the Armed Conflict Location and Event Dataset (ACLED) to empirically explore the following questions: How did the presence of military equipment available to LEAs through the 1033 Program affect the protest wave in 2020 in a given county? Is the effect of militarization heterogeneous across different protest types?

In order to provide a robust answer to these research questions, we follow two alternative econometric strategies. First, we exploit the killing of George Floyd as a natural experiment that affected the salience of police militarization to analyze its causal effect on protest activity. Using a regression discontinuity in time framework, our results lead to two robust findings: The killing lead to a significant increase in protest activity, and the magnitude of this effect is about four times larger in counties heavily equipped via the 1033 Program

Figure 3: Total value of equipment transfers between 2016 and 2019



Notes: For greater clarity transfers are censored above at their 90% quantile which equals $658,000.

than in non-militarized ones. It is also the case that this effect is mostly driven by an increase of BLM protests, which may have substituted those related to Covid. The above results are more pronounced in counties that have a large share of black population, are Democratic leaning, large and urban. Militarization seems to also be an important predictor in the increase in violent protests after the killing. The increase in violent protests, albeit overall smaller in magnitude than the increase in BLM protests, is about six times larger in militarized counties than in non-militarized ones.

Second, we exploit the relationship between militarization and protests by estimating linear probability models and obtain (causal) estimates on the effect of militarization on the incidence of protests. Although this setup is not exclusively focused around the killing of George Floyd, the results we obtain from it are consistent with those from the regression discontinuity framework: militarization increases the incidence of overall protests in a county. Our results when restricting attention only to BLM protests are qualitatively similar to our benchmark results which focuses on all protests. On the other hand, Covid or election related protests appear to be diverse since their incidence is not sensitive to militarization. These results suggest that militarization increases the incidence of overall protests, precisely because of protests linked to police attitudes and corroborate the evidence presented in the first part of our analysis. It also turns out that militarization is not a significant determinant of the incidence of violent protests over the whole of 2020.

Overall, our analysis contributes and extends knowledge in two strands of research that have so far only been studied independently: protests and the 1033 Program. The literature on protests and policing or repression is large and spans across social sciences with a detailed review beyond the scope of this paper. The reader can refer to earlier references on policing and repression of social movements and references therein (Earl et al., 2003; Earl, 2003; Davenport et al., 2019, 2011). Recent empirical work focuses on the effects of internet and social media on protests (Campante et al., 2018; Enikolopov et al., 2020; Manacorda and Tesei, 2020; Zhuravskaya et al., 2020; Ananyev et al., 2019) or why some protests turn violent (Sullivan, 2019; Ives and Lewis, 2020). Of direct relevance to our work is recent research focusing on BLM protests. Chenoweth et al. (2022) analyze individual decisions on attending BLM or Covid related protests during the 2020 wave of protests. Artís (2021) shows how police-related deaths of black citizens are a significant predictor of BLM protests, particularly so in places with a high share of Black population. Artís et al. (2022) show how exposure to the Covid pandemic increased the take-up of social media and subsequently BLM protests in counties with low ex-ante probability of protesting. Reny and Newman (2021) provide evidence on the capacity of BLM protests in shaping citizens' attitudes (Lee, 2002; Mazumder, 2018; Wasow, 2020).

The literature on the 1033 Program is recent and still growing, and it has focused on the causal link between militarization and crime (Bove and Gavrilova, 2017; Harris et al., 2017; Masera, 2021b; Gunderson et al., 2021; Lowande, 2021), police safety and killings (Masera, 2021a), civic engagement (Insler et al., 2019), and local elections (Mavridis et al., 2021). Other related work instead focuses on the association between the 1033 Program and police violence and the use of lethal force (Delehanty et al., 2017; Lawson Jr, 2019). This literature is relevant for us since our instrumental variable approach borrows from closely related work (Bove and Gavrilova, 2017; Harris et al., 2017; Masera, 2021b; Mavridis et al., 2021) while we use data on the 1033 Program from Gunderson et al. (2021).

The rest of the paper is structured as follows. Section 2 describes the data. Section 3 presents empirical results using a regression discontinuity in time to highlight the heterogeneous effect of different protest types as the salience of militarization varies. Section 4 presents results from linear probability models on the link between militarization and protests. Section 5 offers a final discussion.

2 Data

Our empirical analysis is carried out at the county level and has two major data components. Data on the incidence of protests in 2020 and data on the level of militarization of LEAs in 2019. Table 1 reports summary statistics of our protests and militarization data.

We define our protest variables making use of the full 2020 version of the Armed Conflict and Event Dataset (ACLED) (US Crisis Monitor, 2020).[2] We classify as a protest any event coded as "Protests" or "Riots" in ACLED (Raleigh et al., 2010). Paying attention to the participating actors or the notes in this database we identify protests that linked to the BLM movement, Covid related protests or protests linked to the presidential election. Finally, we also code protests that included violence.[3] When studying the incidence of each protest type, we assign these variables a value of 1 if there has been at least one protest of that type in a given county and zero otherwise.

Given these definitions, protests occurred in 53% out of all 3,144 counties (see Figure 1 for a graphical representation) accounting for 93% of the population in the United States. Focusing on different types of protests, 47.3% of the total protests were BLM related, 18.3% were Covid related, and 4.4% were related to the election. Moreover, 11.5% experienced at least one violent protest. Table 1 also shows summary statistics of the total number of protests, again highlighting substantial differences across types of demonstrations and the heterogeneity across counties (i.e. standard deviation is always significantly larger than the mean value).

The data on transfers of military equipment through the 1033 Program come from publicly available data and codes included in Gunderson et al. (2021) replication files. The data in Gunderson et al. (2021) register transfers from 1990 until 2019 as provided by the U.S.

[2] The data can be downloaded from `https://acleddata.com/`

[3] We define as *BLM protest* any protest that had some organized Black actor participation (i.e., the participating actors in the data included the words "BLM", "Black" or "African American" in their names or descriptions). We define as *Covid protest* any protest as defined above if the notes in ACLED dataset included the words "Covid", "corona", "pandemic", or "lockdown". We code *Election protest* a protest whose ACLED notes included the words "election", "stop the steal" or "count every vote". We define as *violent protest* any event whose sub-event type is "Protest with intervention", "Excessive force against protesters", "Mob violence" or "Violent demonstration".

Table 1: Summary statistics

	Mean	St. dev.	Min	Max
Protests:				
Incidence	0.530	0.499	0	1
Number	7.03	23.82	0	601
BLM Protests:				
Incidence	0.466	0.499	0	1
Number	3.32	10.80	0	258
Covid Protests:				
Incidence	0.231	0.421	0	1
Number	1.29	5.35	0	145
Election Protests:				
Incidence	0.122	0.328	0	1
Number	0.312	1.30	0	31
Violent protests:				
Incidence	0.115	0.320	0	1
Number	0.535	3.73	0	139
Total value of equipment transfers:				
Over 1990-2019	\$399,075	\$1,032,120	\$0	\$19,904,312
Over 2016-2019	\$162,941	\$499,990	\$0	\$8,016,287
Over 2018-2019	\$70,278	\$270,846	\$0	\$3,417,796

Notes: Summary statistics at the county level for 3,144 observations. Data on protests refer to 2020 events.

Department of Defense.[4] In Table 1 we report aggregate transfers at the county level over different time spans with a geographical representation of transfers over 2016-2019 illustrated in Figure 3.

Our control variables are derived from various sources. Median household income, population, Black population percentage, and land size come from the US census. We use the 2019 values for median household income (in thousands) while we employ the 2020 estimates for (Black) population (in millions). Electoral data is extracted from the MIT Election Data and Science lab (MIT Election Data and Science Lab, 2018) and the variable indicating whether a county is designed as a High Intensity Drug Trafficking Areas (HIDTA) is taken from the replication files of Harris et al. (2017).

[4] To be precise, the 1028 Program started in 1990 to be substituted by the 1033 Program in 1997. Hence, our data includes transfers under both programs that for convenience we refer to as the most popular and current name 1033 Program.

3 Regression Discontinuity in Time

The first approach to answer our research question is to take the killing of George Floyd in May 2020 as a natural experiment and see the effect it had, if any, on protest activity. To do so, we employ a regression discountinuity in time (RDiT) setup and use a short window of time around the killing to try to uncover heterogeneous effects across different protest types, as we would not expect all types of demonstrations to be equally affected.

3.1 RDiT: Methodology

The killing of George Floyd happened on 25 May 2020. We consider this date as the threshold date in a RDiT design. The specification to be estimated is the following:

$$DailyProt_{ct} = \beta_0 + \beta_1 25May_t + \beta_2 f(t) + \beta_3 25May_t \times f(t) + \epsilon_{ct} \tag{1}$$

where $DailyProt_{ct}$ records the number of protests in county c on day t. The definition of protest can include any type of protest or refer to a specific type of protest (i.e. BLM, Covid, election, violent). Variable t measures the number of days before and after 26 May. It takes value 0 on 26 May and becomes positive after 26 May and negative before 26 May. The variable $25May_t$ takes value 0 for all days before, and including, 25 May, and 1 for all days after 25 May.[5] Our parameter of interest is β_1, measuring any differential effect due to the killing. As standard in this type of setups, (1) includes a polynomial function $f(t)$ of time, its interaction with $25May_t$ and the error term ϵ_{ct}.

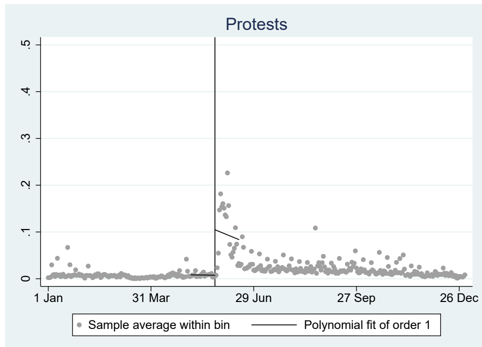
In our main results we use a linear function of time, $f(t) = t$, and a bandwidth of 21 days before and after the event but other functional choices and bandwidths are part of a series of robustness checks in Section 3.2.1. Having a longer bandwidth risks introducing bias in our estimation, as there may be other reasons that drive the protests when moving further away from the date of the killing. On the other hand, too short a bandwidth may be sensitive to possible cyclicalities in protesting (e.g. protests may be more likely on certain days of the week).

To capture the possibly heterogeneous effect of police militarization on protest, we split the sample between "militarized" and "non-militarized" counties. We define as (non-) militarized those counties that have received during 2016-2019 a total value of equipment transfers (below) above the 10% percentile of the distribution of equipment across all counties (which

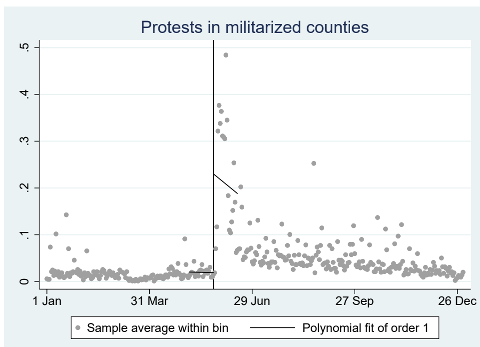
[5]Since the killing of George Floyd happened in the evening of 25 May, we assume that any protest in reaction to this event would have to have taken place starting the next day.

received some transfers) in that time frame. We vary this threshold and the period for its calculations in a series of robustness checks.

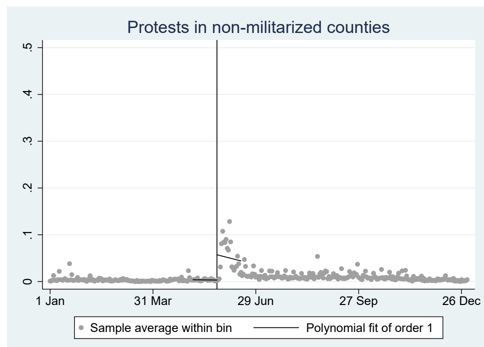
Figure 4: Number of protests



(a) All counties



(b) Militarized counties



(c) Non-militarized counties

Notes: The regression lines come from first-order polynomial regressions of number of protests on time with bandwidth of 21 days.

3.2 RDiT: Results

Figure 4 provides a graphical representation of our design. In each of the three panels the vertical axis represents number of protests in each day and the horizontal axis represents the days of year 2020, with the vertical line centered on May 25. The regression lines on the left and on the right of the 25 May line come from the estimations of specification (1) using a linear function for t and a bandwidth of 21 days.

Panel (a) of Figure 4 is based on the total daily protests in the US, while the the other two panels split our sample between militarized and non-militarized counties. The daily number of protests before 25 May has been relatively constant across all counties, which is the case even when taking into account a longer bandwidth. It is evident that after George Floyd's killing there was a jump in the number of daily protests in the US, as widely known. What is more interesting is that the jump is much more pronounced in militarized counties with the the jump across all counties masking a lot of underlying variation in terms of the role of the 1033 Program.

Table 2: Response of protests

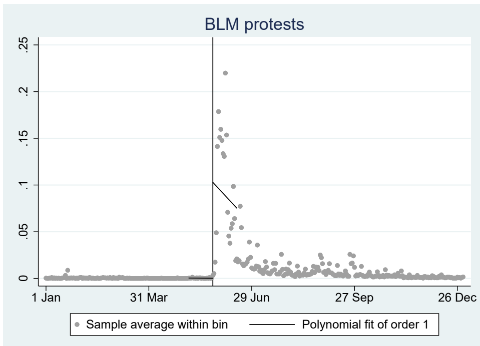
	(1)	(2)	(3)	(4)
Panel A: All protests				
	All counties	Militarized	Non-militarized	
25 May_t	0.097***	0.211***	0.054***	
	(0.005)	(0.016)	(0.003)	
Observations	133,601	36,765	96,836	
Panel B: Protest types				
	BLM	Covid	Election	Violent
25 May_t	0.103***	-0.003***	-0.0002*	0.019***
	(0.005)	(0.001)	(0.000)	(0.002)
Observations	133,601	133,601	133,601	133,601

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

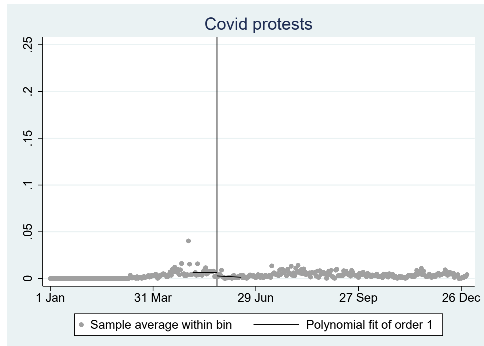
In panel A of Table 2 we report the results used in Figure 4, and the estimates of our coefficient of interest, β_1 in (1), that measures the jump seen in the previous figure. The results confirm that the jump in protests is significant at the 1% level in all three cases, however the jump in militarized counties is about four times greater then the one in non-militarized ones. As a result, the average effect across the US actually doubles when looking at militarized counties, representing a sizable effect.

In Figure 5 and panel B of Table 2 we turn our attention to different types of protests. The figure clearly shows a jump for BLM protests and a smaller one for violent ones. Considering the estimates, the effect on BLM related protests is significant at the 1% level with a coefficient (0.103) very close to the one of all protests (0.097). The increase in violent protests is also highly significant, although the coefficient is much smaller (i.e. more than five times smaller than the effect on BLM protests). Interestingly, the point estimate for Covid protest is also highly significant albeit negative and very small (i.e. not necessarily visible in Figure 5). The coefficient for election protests is essentially zero. If anything, the evidence on the last two types of protests suggests that there may have been some crowding out of Covid (and election) related protests passing the protest scene to the BLM movement.

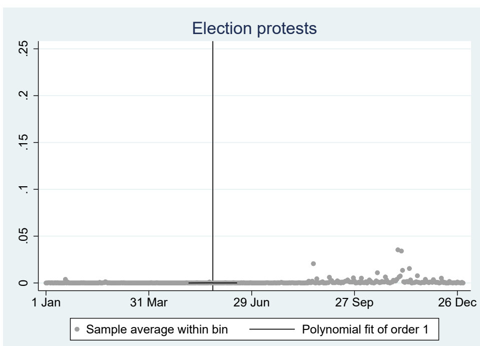
Figure 5: Number of protests by type



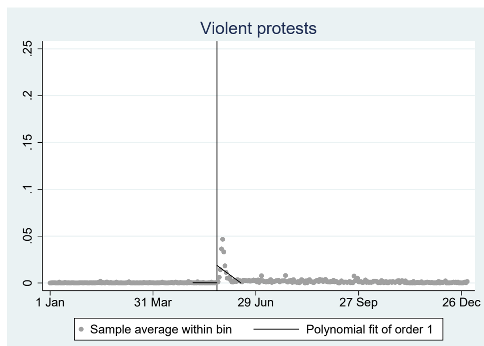
(a) BLM protests



(b) Covid protests



(c) Election protests



(d) Violent protests

Notes: The regression lines come from first-order polynomial regressions of number of protests on time with bandwidth of 21 days.

Given our interest on the role of militarization on protests, it is natural to investigate the role of the transfers through the 1033 Program on different types of protests. Table 3 presents such heterogeneous results across militarized and non-militarized counties. The conclusions are in line with those of Table 2 highlighting the importance of militarization in protesters' response: the effect on militarized counties is always much larger than in non-militarized counties. As the salience of militarization increases due to the killing, we see the effect on BLM and violent protests being about four and six times larger in militarized counties compared to non-militarized ones. The effect on election or Covid related protests is again either of very low magnitude or not significant.

Table 3: Heterogeneous response across levels of militarization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BLM		Covid		Election		Violent	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May$_t$	0.224***	0.057***	-0.009***	-0.001**	-0.0004	-0.0001	0.047***	0.008***
	(0.015)	(0.003)	(0.003)	(0.001)	(0.0003)	(0.0001)	(0.005)	(0.001)
Observations	36,765	96,836	36,765	96,836	36,765	96,836	36,765	96,836

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

Having established the heterogeneous effect of the killing on protests across militarization levels we turn our attention to other characteristics that one may expect to affect protesters' response. Panel A of Table 4 pays attention to socioeconomic characteristics while Panel B reports results related to geographical aspects.

Table 4: Socioeconomic characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Socioeconomic characteristics								
	Democratic	Republican	High Black Share	Low Black Share	Low Income	High Income	HIDTA	No HIDTA
25 May$_t$	0.330***	0.054***	0.151***	0.044***	0.013***	0.107***	0.316***	0.055***
	(0.026)	(0.003)	(0.009)	(0.004)	(0.003)	(0.006)	(0.025)	(0.003)
Observations	20,812	112,789	66,822	66,779	13,330	120,271	21,801	111,800
Panel B: Geographical characteristics								
	Small	Large	Dense	Sparse	Rural	Urban		
25 May$_t$	0.031***	0.377***	0.126***	0.011***	0.031***	0.209***		
	(0.002)	(0.021)	(0.007)	(0.002)	(0.002)	(0.012)		
Observations	107,973	25,628	100,190	33,411	83,721	49,837		

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial. Democratic (Republican) subsample includes counties in which in the 2016 election Hillary Clinton received a higher (lower) share than Donald Trump. High Black Share (Low Black Share) counties have a share of black population higher (lower) than the median share of black population by county. The High Income (Low Income) subsample includes counties with median household income greater (lower) than the 10^{th} percentile of the median household income by county. HIDTA subsample includes counties that have been designated as High Intensity Drug Trafficking Areas. Counties are small if they have fewer than 100,000 people, dense if their population density is above the 25^{th} percentile, and rural if as such defined by the US Department of Agriculture.

Table 4 indicates that the jump in protest activities has been larger in counties that are Democratic leaning, with a large black population, with higher median incomes, and designated as high intensity drug trafficking area (HIDTA). The increase in protests was also more pronounced in larger, densely populated, and urban counties. While our results on socioeconomic and geographical characteristics are not surprising and compatible with the findings in Artís (2021) and Chenoweth et al. (2022), they help us better understand the importance of militarization by comparing these magnitudes also with those reported earlier. For instance, the effect of the killing in Democratic leaning and HIDTA counties is essentially the same even if the correlation between these two dimensions is very low (i.e. 0.27). At the same time, the increase between militarized and non-militarized counties (about 3.9 times) is on a par with the difference based on the share of black population.

Taking stock of the RDiT results, it is clear that the killing of George Floyd did have a statistically significant and a meaningful positive effect on the number of protests that followed. Central to this paper, the effect was an order of magnitude larger for those counties who have received transfers of military equipment in the previous 4 years, with much more pronounced effects for BLM and violent protests. These conclusions are robust to a series of robustness checks that we discuss next.

3.2.1 Robustness checks

In the following, we discuss various robustness checks applied to our main tables (i.e., Tables 2 and 3) with all results reported in the Appendix. Starting with methodological considerations, we experimented with a shorter bandwidth of 14 days, and a longer one of 28 days. Moreover we experimented with the inclusion of a polynomial of second order, and the use of a triangular kernel. Our conclusions are qualitatively unchanged even though the point estimates do sometimes change – but when they do, the relative size across types of protests and militarization status are unaffected (see Tables A1-A4).

From a conceptual perspective, the definition of militarized county is crucial in the analysis and it rests on two considerations: percentile of the distribution of total value of transfers and sample period for the calculation of such distribution. In the main analysis we consider as militarized a county with a total value of transfers above the 10^{th} percentile of (positive) transfers over the years 2016-2019. To consider the relevance of these choices, we have engaged in a series of sensitivity checks defining as militarizes those counties: i) above the 25^{th} percentile of transfers during 2016-2019; ii) above the 10^{th} percentile of transfers during 2018-2019; iii) above the 10^{th} percentile of transfers during 1990-2019; and iv) above the 25^{th} percentile of transfers during 1990-2019. Again, the results in Tables A5-A8 indicate that these choices are inconsequential for the conclusions reached earlier.

Finally, we have also exploited the granularity of the data on military transfers to split the total value of transfers among 4 categories: gears, vehicles, weapons, and a residual group of non-lethal items (e.g., high-tech cameras, office supplies). No matter which category we use to define whether a county is militarized (with a 10^{th} percentile for transfers in 2016-2019 as in the benchmark analysis), the incidence of overall protests is always (statistically) higher in militarized counties (see Table A8).[6]

4 Linear probability model

Having exploited the killing of George Floyd and considered the discontinuity in the incidence of protests just before and after this event, we now explore more generally the relationship between the militarization level and the incidence of protests in a given county. We see these two econometric frameworks as complementing each other.

4.1 Methodology

We estimate the following linear probability model:

$$Prot_c = \beta_1 \ln(equipment_c) + \beta_2 X_c + \alpha_s + \epsilon_c, \tag{2}$$

where $Prot_c$ is our dependent variable that captures the incidence of protests in county c. It takes value one if there was at least one protest recorded in county c in 2020 and zero otherwise. As in the case of the regression discontinuity design, the definition of protest can include any type of protest or refer to a specific type of protest (BLM, Covid, election, violent). Our main explanatory variable of interest is $equipment_c$ denoting the total value of equipment present in 2019 in county c resulting from transfers during the years 2016-2019. X_c is a matrix of county-level controls: median household income, population, share of black population, whether the county is classified as HIDTA, as well as the 2016 vote share of Donald Trump in the presidential election. We also include state fixed effects (α_s). Standard errors are clustered at the state level.

A challenge for the identification of the causal effect of military equipment is the possible endogeneity of the level of militarization of a county ($equipment_c$), in that such transfers may be correlated with other (omitted) attributes that influence the occurrence of protests. In this case, estimating Equation (2) through ordinary least squares (OLS) may only provide evidence of correlation between the level of militarization and the incidence of protests. To overcome this possibility, we follow the literature on the causal effects of the 1033 Program on different outcomes of interest (Bove and Gavrilova, 2017; Harris et al., 2017; Masera, 2021b;

[6] The same holds true if we use these categories and focus on BLM and violent protests.

Mavridis et al., 2021) and implement an instrumental variable (IV) approach. Among the instruments proposed in the literature, we employ an indicator of the distance to equipment disposition centers and geographical size of a county. Both instruments are constructed using data from Harris et al. (2017). An indicator of the distance between the county centroid and the location of nearby disposition centers is probably among the most convincing instruments proposed. LEAs requesting transfers are responsible for the cost of shipping of any item requested from the storage location and hence a good predictor of transfers. In our first stage, we use a dummy variable indicating whether a disposition center can be found within a certain radius of the centroid of a given county. This type of instrument was first employed by Masera (2021a,b) who chose the cut-off in order to maximize the performance of the instrument. In the same way, we use a cut-off of 214 miles (versus an average and median distance of 215 and 191 miles, respectively) in our benchmark results but engage in a series of robustness checks to show that this choice does not really affect the point estimates that we recover. Regarding county size, we use the log of the land area of a county. Counties vary dramatically in this dimension from a couple of square miles (Falls Church in Virginia) to over 20,000 square miles (San Bernardino in California). It is reasonable to hypothesize that size affects the decision to request items (e.g. for patrolling) and it is also the case that the federal agency in charge of the process explicitly encourages large counties to apply (Harris et al., 2017). As Masera (2021a,b) points out, the instrument indicating closeseness to a disposition center may picking up effects of proximity to a military base, regardless of whether this base is also a disposition center, and this proximity may have an effect on violent behavior. Moreover, as he points out, it is common practice for police to hire veterans. In any case, this proximity may indeed affect the likelihood that a county experiences a protest. We therefore, we control for closeness to a military base by constructing a dummy variable that indicates whether any military base is within a radius of around 25 miles from the centroid of the county. This radius is equal to the 10th percentile of the distribution of distances between the centroid of a county and its closest military base.[7]

The first stage results for our benchmark specifications are reported in the first column of Table 5. The two instruments are both statistically significant and present the expected positive sign: transfers are higher if a county is close to a disposition center and the larger that county is. They also satisfy the test for overidentifying restrictions, with the Hansen J Stat not rejecting the null hypothesis that the instruments are valid and are correctly

[7]These distances were constructed using a dataset of US military base locations, downloaded from opendatasoft.com (`https://public.opendatasoft.com/explore/dataset/military-bases/table/`).

excluded from the second stage. As for the Kleibergen-Paap F-stat, it is not very high but different cut-off choices for our distance dummy would result in higher values (i.e. above 10) without any meaningful change on the estimated coefficients (but a lower Hansen J Stat). We demonstrate the robustness of the results to variations in the use of the instruments and their definition in Section 4.2.1 below. For completeness, we also always report OLS results.

Table 5: Effect of of militarization on incidence of protests

	(1) First stage	(2) IV	(3) OLS
$\ln(land)_c$	0.839***		
	(0.242)		
Disposition Center Dummy$_c$	0.755**		
	(0.332)		
$ln(equipment)_c$		0.093***	0.014***
		(0.024)	(0.002)
Median Household Income$_c$	0.072***	0.000	0.005***
	(0.018)	(0.002)	(0.001)
Population$_c$	2.174***	-0.186**	0.000
	(0.783)	(0.072)	(0.028)
Black population$_c$ percentage	0.003	-0.005*	-0.004
	(0.019)	(0.003)	(0.003)
HIDTA$_c$	1.641***	0.029	0.164***
	(0.430)	(0.043)	(0.037)
Trump vote percentage$_c$	-0.018	-0.008***	-0.009***
	(0.012)	(0.002)	(0.002)
Military Base Dummy$_c$	0.799**	-0.007	0.054*
	(0.342)	(0.040)	(0.028)
State FE	Yes	Yes	Yes
Observations	3106	3106	3106
R^2			0.177
Kleibergen-Paap F-stat		10.302	
Hansen J Stat p-value		0.632	

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests. Equipment transfers over 2016-2019.

4.2 Results

Our benchmark results on the likelihood of protests are presented in Table 5. The probability that a county had a protest in 2020 is increasing in the value of military transfers the county had received during 2016-2019. The effect is significant at the 1% level in both the IV and OLS specifications. The point estimates for our key regressor is smaller in the OLS regressions, indicating a negative bias in such specification and supporting the use of an IV approach. In comparing the OLS and IV estimates, it is useful to consider the role of the control variables. Counties in favor of Donald Trump in 2016 are less likely to see protests while this political dimension is not a determinant of transfers (in the first-stage regression). With Donald Trump still in office at the time of the protests, this result is consistent with Republican voters protesting less than Democrats and is aligned with Republican voters being tolerant (or in favor) of the overall use of military equipment. On the other hand, the positive correlation of HIDTA and median household income with protests in the OLS specification disappears in the IV estimates but they are important determinants of transfers (and the IV methodology allows to properly capture their role). Population plays opposite effects in that more populous counties receive more transfers but they are less likely to experience protests, and given these conflicting effects, it is no surprise that population is not significant in the last column of Table 5. In light of the BLM movement, it may be surprising to see that the share of black population is only marginally significant and negative for the incidence of protests and only in the IV specification. Closeness to a military base does not seem to have any effect in the IV specification while it has a positive effect on the OLS one.

As in the case of our RDiT design, it is important to see how results may differ across different types of protests. Table 6 reports the outcome of such exercise. If the OLS specifications indicate a positive correlation of military equipment with all types of protests, the IV results only find a significant effect for BLM demonstrations. This asymmetry in the results land further support to the IV strategy in that the same first-stage regression across the various columns (i.e. the endogenous value of transfers does not change across types of protest) leads to different effects depending on the chosen dependent variable. We can also observe some differences in the significance of the control variables in explaining protests of various nature. Median household income, population, HIDTA and closeness to a military base are sometimes positively associated with some specific types of protests (e.g. population for election and violent demonstrations) on top of their role in explaining the level of military transfers received by a county (i.e. the first stage which is the same as in Table 5).

Table 6: Effect of of militarization on incidence of different types of protests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BLM		Covid		Election		Violent	
	IV	OLS	IV	OLS	IV	OLS	IV	OLS
$\ln(equipment)_c$	0.082***	0.015***	0.011	0.007***	0.014	0.005***	-0.012	0.004***
	(0.026)	(0.002)	(0.018)	(0.002)	(0.013)	(0.001)	(0.015)	(0.001)
Median Household Income$_c$	0.000	0.005***	0.004**	0.004***	0.002	0.002***	0.002	0.001
	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population$_c$	-0.124*	0.035	0.145	0.154**	0.209***	0.230***	0.286***	0.248***
	(0.067)	(0.036)	(0.094)	(0.073)	(0.071)	(0.071)	(0.102)	(0.078)
Black population percentage$_c$	-0.006*	-0.005*	-0.002	-0.002	-0.002	-0.002	0.000	0.000
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
HIDTA$_c$	0.043	0.158***	0.201***	0.207***	0.108***	0.123***	0.201***	0.174***
	(0.045)	(0.037)	(0.035)	(0.036)	(0.028)	(0.030)	(0.037)	(0.029)
Trump vote percentage$_c$	-0.009***	-0.010***	-0.007***	-0.007***	-0.004***	-0.004***	-0.005***	-0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Military Base Dummy$_c$	-0.006	0.046	0.040	0.043	0.046**	0.053***	0.048*	0.035
	(0.034)	(0.031)	(0.031)	(0.029)	(0.021)	(0.020)	(0.028)	(0.022)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3106	3106	3106	3106	3106	3106	3106	3106
R^2		0.194		0.259		0.236		0.277
Hansen J Stat p-value	0.368		0.196		0.937		0.104	

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. First stage results and Kleibergen-Paap F-stat as in Table 5.

In trying to align the different time focus of the linear probability models and the RDiT design, Table 7 presents the results when applying the probability models to the period before or after the killing of George Floyd, using a symmetric window of 146 days (i.e. days from January 1 till May 25) As on the full sample, our coefficient of interest is much more significant in the OLS estimations than when applying the IV methodology. The IV results make evident that before the killing, militarization if anything had a negative albeit small effect in BLM, election, and violent protests. Instead, after the killing there is a positive effect on overall protest activity, which is mainly driven by the BLM movement. These conclusions corroborate the results found in the first part of our analysis: the level of militarization through the 1033 Program does affect the incidence of protests that a county experiences, with the strongest effect for BLM protests.

4.2.1 Robustness checks

As for the RDiT design, we have verified that this second set of results is robust to alternative modeling choices. Starting from the choice of our instrument, the cut-off value chosen to define the dummy variable is largely irrelevant. Using any of the 9 deciles leads to estimates of the main coefficient of interest varying between 0.075 and 0.100 (significant at the 1%

Table 7: Effect of militarization 146 days before and after 25 May

	(1) Protests	(2) BLM	(3) Covid	(4) Election	(5) Violent
Panel A: Before 25 May					
IV					
$\ln(equipment)_c$	0.016	-0.018**	-0.000	-0.009*	-0.018*
	(0.018)	(0.007)	(0.012)	(0.005)	(0.010)
Hansen J Stat p-value	0.007	0.275	0.249	0.846	0.237
OLS					
$\ln(equipment)_c$	0.008***	0.000	0.004***	-0.000	-0.001*
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
R^2	0.247	0.198	0.215	0.069	0.181
Panel B: After 25 May					
IV					
$\ln(equipment)_c$	0.090***	0.083***	0.006	0.019**	-0.012
	(0.024)	(0.028)	(0.015)	(0.010)	(0.015)
Hansen J Stat p-value	0.517	0.439	0.048	0.303	0.095
OLS					
$\ln(equipment)_c$	0.015***	0.015***	0.006***	0.004**	0.003***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
R^2	0.183	0.193	0.294	0.120	0.273

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Both IV and OLS regressions include 3,106 observations. Regressions include Median Household Income, Population, Black population percentage, HIDTA, Trump vote percentage and Military Base Dummy as controls. 146 days before and after 25 May. First stage results and Kleibergen-Paap F-stat as in Table 5.

level), which are not statistically different from what si found in Table 5 (see Table A10 in the Appendix for full set of results). The cut-off does affect the values of the Kleibergen-Paap F-stat and Hansen J Stat tests, with our choice for the main tables trying to strike a balance between the two. Given that we have more instruments than endogenous variables, we have also experimented using one instrument at the time. As reported in Table A11, the conclusions reached earlier are unchanged.

We have also re-run our main regressions using the same type of robustness checks as for the RDit analysis. In Table A12 we show that changing the period for the calculation of the total value of transfers received by a county does not make a difference, be it a two-year window (i.e. 2018-2019) or cumulative transfers over 1990-2019. Similarly, in Table A13 we report results using the different categories of military equipment to show that there is not much of a difference among them. In Table A14 we repeat the same exercise of Table 7 but this time we focus only on 21 days before and after May 25, a length of time equal to the main bandwidth we use on our RDiT design. The results are qualitatively similar to the results in Table 7. Finally, Bove and Gavrilova (2017) use a different instrument, the share of years a county has received equipment in their sample.[8] We construct our own version of the instrument, the share of years between 1990-2019 a county has received transfers and use it as an additional instrument. The results of such specification, shown in Table A15, demonstrate that our qualitative results are unaffected by the addition of this instrument (albeit with smaller point estimates). However, the higher values for the Kleibergen-Paap F-stats are accompanied by very low p-values for the Hansen J Stat for overidentifying restrictions. Thus, we only show this version as a robustness check.

5 Discussion

In this paper we aimed to verify whether the militarization of US counties through the 1033 Program leads to higher protest activity. To identify a causal effect we employ two complementary econometric strategies. Exploiting the killing of George Floyd on May 25, 2020 we exploit the subsequent increase in protests to see if it differs depending on the level of militarization. Our results from the regression discontinuity design indicate that the value of military transfers is a significant determinant of protests, with most of the effect coming from BLM and violent protests. These conclusions are corroborated by the results of estimating linear probability models on the whole of 2020 or around the killing of George Floyd.

In conclusion, we provide robust evidence on the causal effect of militarization on protests. One important result is that (BLM) protesters disapprove of the accumulation of military equipment in LEAs through the 1033 Program, with the latter often in the public debate on excessive police violence, particularly towards racial minorities. This result is in line with evidence supporting that citizens disapprove militarized LEAs (Mummolo, 2018) and shows that protests as a fundamental right of the First Amendment serve well the purpose of signaling citizens' disapproval towards militarization practices. This result is of further interest when interpreted jointly with contradicting evidence showing that voters provide electoral rewards to sheriffs involved in militarization (Mavridis et al., 2021). Since these

[8] In their set-up this is in fact interacted with US military spending in each year to generate a time-varying instrument.

two results do not move in the same direction, we provide evidence how two fundamental aspects of democracy can represent diverging views and how representation in protests can be diverse to representation in the electorate (especially when turnout in elections is low).

On a final note, militarization per se does not seem to have a deterrent effect on the incidence of protests unrelated to the 1033 Program, as represented in our sample by Covid and election-related protests. This result highlights that despite citizens disapproving militarization, citizens' right to protest is not harmed by militarization.

Appendix

A1 RDiT Methodology

Table A1: Response of protests, 14 days bandwidth

	(1)	(2)	(3)	(4)
Panel A: All protests				
	All counties	Militarized	Non-militarized	
25 May$_t$	0.056***	0.120***	0.031***	
	(0.003)	(0.011)	(0.002)	
Observations	90,103	24,795	65,308	
Panel B: Protest types				
	BLM	Covid	Election	Violent
25 May$_t$	0.059***	-0.002**	-0.0002*	0.019***
	(0.003)	(0.001)	(0.000)	(0.002)
Observations	90,103	90,103	90,103	90,103

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 14 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

Table A2: Response of protests, 28 days bandwidth

	(1)	(2)	(3)	(4)
Panel A: All protests				
	All counties	Militarized	Non-militarized	
25 May_t	0.110^{***}	0.239^{***}	0.061^{***}	
	(0.005)	(0.017)	(0.003)	
Observations	177,099	48,735	128,364	
Panel B: Protest types				
	BLM	Covid	Election	Violent
25 May_t	0.114^{***}	-0.002^{***}	-0.0001^{*}	0.017^{***}
	(0.005)	(0.001)	(0.000)	(0.001)
Observations	177,099	177,099	177,099	177,099

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 28 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

Table A3: Response of protests, second-order polynomial

	(1)	(2)	(3)	(4)
Panel A: All protests				
	All counties	Militarized	Non-militarized	
25 May_t	0.014^{***}	0.029^{***}	0.008^{***}	
	(0.003)	(0.008)	(0.002)	
Observations	133,601	36,765	96,836	
Panel B: Protest types				
	BLM	Covid	Election	Violent
25 May_t	0.014^{***}	0.0002	-0.0002	0.014^{***}
	(0.005)	(0.001)	(0.000)	(0.001)
Observations	133,601	133,601	133,601	133,601

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; second-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

Table A4: Response of protests, triangular kernel

	(1)	(2)	(3)	(4)
Panel A: All protests				
	All counties	Militarized	Non-militarized	
25 May$_t$	0.059***	0.128***	0.033***	
	(0.003)	(0.011)	(0.002)	
Observations	127,387	35,055	92,332	
Panel B: Protest types				
	BLM	Covid	Election	Violent
25 May$_t$	0.062***	-0.0002**	-0.0002*	0.016***
	(0.003)	(0.001)	(0.000)	(0.001)
Observations	127,387	127,387	127,387	127,387

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; triangular kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019.

Table A5: Response of protests, transfers based on 25^{th} percentile over 2016-2019.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All protests								
	Militarized	Non-Militarized						
25 May$_t$	0.224***	0.060***						
	(0.018)	(0.003)						
Observations	30,659	102,942						
Panel B: Heterogeneous response across levels of militarization								
	BLM		Covid		Election		Violent	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May$_t$	0.238***	0.062***	-0.010***	-0.002**	-0.0002	-0.0001	0.051***	0.009***
	(0.017)	(0.003)	(0.003)	(0.001)	(0.000)	(0.000)	(0.006)	(0.001)
Observations	30,659	102,942	30,659	102,942	30,659	102,942	30,659	102,942

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 25^{th} percentile over 2016-2019.

Table A6: Response of protests, transfers based on 10^{th} percentile over 2018-2019.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All protests								
	Militarized	Non-Militarized						
25 May_t	0.229***	0.066***						
	(0.020)	(0.004)						
Observations	25,413	108,188						
Panel B: Heterogeneous response across levels of militarization								
	BLM		Covid		Election		Violent	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May_t	0.243***	0.070***	-0.011***	-0.002**	-0.0000	-0.0002*	0.053***	0.010***
	(0.020)	(0.003)	(0.003)	(0.001)	(0.000)	(0.0001)	(0.007)	(0.001)
Observations	25,413	108,188	25,413	108,188	25,413	108,188	25,413	108,188

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 2018-2019.

Table A7: Response of protests, transfers based on 10^{th} percentile over 1990-2019.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All protests								
	Militarized	Non-Militarized						
25 May_t	0.137***	0.036***						
	(0.008)	(0.003)						
Observations	81,055	52,546						
Panel B: Heterogeneous response across levels of militarization								
	BLM		Covid		Election		Violent	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May_t	0.145***	0.038***	-0.005***	-0.001*	-0.0003*		0.027***	0.006***
	(0.008)	(0.003)	(0.001)	(0.0007)	(0.0001)		(0.003)	(0.001)
Observations	81,055	52,546	81,055	52,546	81,055	52,546	81,055	52,546

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 1990-2019. There are no Election non-militarized results since the dependend variable in this case is constant for this bandwidth.

Table A8: Response of protests, transfers based on 25^{th} percentile over 1990-2019.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All protests								
	Militarized	Non-Militarized						
25 May$_t$	0.157***	0.036***						
	(0.009)	(0.003)						
Observations	67,596	66,005						
Panel B: Heterogeneous response across levels of militarization								
	BLM		Covid		Election		Violent	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May$_t$	0.165***	0.039***	-0.005***	-0.002**	-0.0002	-0.0001	0.032***	0.005***
	(0.009)	(0.003)	(0.002)	(0.0007)	(0.0001)	(0.0001)	(0.003)	(0.001)
Observations	67,596	66,005	67,596	66,005	67,596	66,005	67,596	66,005

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 25^{th} percentile over 1990-2019.

Table A9: Response of protests given categories of equipment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weapons		Vehicles		Gears		Others	
	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.	Milit.	Non-mil.
25 May$_t$	0.255***	0.071***	0.220***	0.073***	0.228***	0.067***	0.258***	0.076***
	(0.026)	(0.004)	(0.021)	(0.004)	(0.020)	(0.004)	(0.030)	(0.004)
Observations	19,350	114,251	21,801	111,800	25,155	108,446	15,824	117,777

Notes: Standard errors in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively, based on standard errors clustered at county level; uniform kernel; bandwidth of 21 days; first-order polynomial; transfers based on 10^{th} percentile over 2016-2019 for each category of equipment.

A2 IV Methodology

Table A10: Distance dummy robustness check by distance distribution deciles

	1st	2nd	3rd	4th	5th
$\ln(equipment)_c$	0.099***	0.084***	0.075***	0.094***	0.098***
	(0.030)	(0.027)	(0.026)	(0.028)	(0.028)
Kleibergen-Paap F-stat	5.745	5.978	6.767	5.522	6.099
Hansen J Stat p-value	0.951	0.026	0.010	0.581	0.971
	6th	7th	8th	9th	
$\ln(equipment)_c$	0.100***	0.096***	0.093***	0.098***	
	(0.026)	(0.027)	(0.027)	(0.030)	
Kleibergen-Paap F-stat	9.941	8.915	7.452	5.484	
Hansen J Stat p-value	0.871	0.681	0.489	0.967	

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. For each column the cut-off of the disposition center dummy is fixed at the corresponding decile of the distribution of distances to the closest distribution center. The number of observations is 3,106 in all cases. Regressions on incidence of protests. Median Household Income, Population, Black population percentage, HIDTA, Trump vote percentage, and Military Base Dummy included as controls. Equipment transfers over 2016-2019.

Table A11: One instrument at a time robustness check

	(1)	(2)	(3)	(4)
	Only ln(*land*)		Only Disp. Cent. Dummy	
	First stage	IV	First stage	IV
$\ln(land)_c$	0.792***			
	(0.238)			
Disposition Center Dummy$_c$			0.610*	
			(0.346)	
$\ln(equipment)_c$		0.098***		0.074*
		(0.030)		(0.040)
Median Household Income$_c$	0.072***	-0.000	0.066***	0.002
	(0.018)	(0.003)	(0.016)	(0.002)
Population$_c$	2.198***	-0.198**	2.339***	-0.139
	(0.787)	(0.081)	(0.788)	(0.105)
Black population percentage$_c$	0.004	-0.005	0.004	-0.005*
	(0.018)	(0.003)	(0.018)	(0.003)
HIDTA$_c$	1.624***	0.020	1.716***	0.063
	(0.433)	(0.049)	(0.441)	(0.082)
Trump vote percentage$_c$	-0.019	-0.008***	-0.011	-0.008***
	(0.012)	(0.002)	(0.012)	(0.002)
Military Base Dummy$_c$	0.801**	-0.011	0.768**	0.008
	(0.349)	(0.046)	(0.349)	(0.035)
State FE	Yes	Yes	Yes	Yes
Observations	3106	3106	3106	3106
Kleibergen-Paap F-stat		11.030		3.106

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests. Equipment transfers over 2016-2019.

Table A12: Effect of of militarization on incidence of protests, different militarization horizons

	(1)	(2)	(3)	(4)
	2018-2019		1990-2019	
	First Stage	IV	First Stage	IV
$\ln(land)_c$	0.811***		1.572***	
	(0.220)		(0.231)	
Disposition Center Dummy$_c$	0.352		0.789**	
	(0.224)		(0.380)	
$\ln(equipment)_c$		0.103***		0.053**
		(0.026)		(0.023)
Median Household Income$_c$	0.037***	0.003**	0.062***	0.004***
	(0.012)	(0.001)	(0.018)	(0.001)
Population$_c$	2.011***	-0.191**	1.240*	-0.049
	(0.716)	(0.079)	(0.637)	(0.041)
Black population percentage$_c$	0.006	-0.005*	-0.020	-0.003
	(0.016)	(0.003)	(0.025)	(0.002)
HIDTA$_c$	0.856***	0.092**	1.786***	0.086*
	(0.320)	(0.037)	(0.432)	(0.045)
Trump vote percentage$_c$	-0.015	-0.008***	-0.041**	-0.007***
	(0.011)	(0.002)	(0.018)	(0.002)
Military Base Dummy$_c$	0.634*	0.002	0.870***	0.022
	(0.325)	(0.040)	(0.313)	(0.033)
State FE	Yes	Yes	Yes	Yes
Observations	3106	3106	3106	3106
Kleibergen-Paap F-stat		9.247		50.640
Hansen J Stat p-value		0.405		0.520

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests. First two columns refer to transfers that took place only in 2018-2019, second two columns to all transfers since 1990. For each of these two cases instrumental variables estimation and its first stage are depicted.

Table A13: Effect of different categories of equipment on incidence of protests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weapons		Vehicles		Gears		Others	
	First Stage	IV	First Stage	IV	First Stage	IV	First Stage	IV
$\ln(land)_c$	0.442***		0.669***		0.560***		0.624***	
	(0.151)		(0.221)		(0.150)		(0.181)	
Disposition Center Dummy$_c$	0.407**		0.456*		0.474*		0.081	
	(0.163)		(0.245)		(0.272)		(0.201)	
$\ln(equipment)_c$		0.176***		0.123***		0.142***		0.130***
		(0.045)		(0.035)		(0.041)		(0.035)
Median Household Income$_c$	0.030***	0.002	0.045***	0.001	0.044***	0.001	0.022***	0.004***
	(0.010)	(0.002)	(0.014)	(0.002)	(0.012)	(0.003)	(0.007)	(0.002)
Population$_c$	1.860***	-0.311**	1.874***	-0.214**	1.706**	-0.225**	1.929***	-0.232**
	(0.490)	(0.116)	(0.672)	(0.095)	(0.677)	(0.095)	(0.491)	(0.090)
Black population percentage$_c$	-0.001	-0.004	-0.001	-0.004	0.005	-0.005	0.002	-0.005
	(0.012)	(0.003)	(0.015)	(0.003)	(0.014)	(0.003)	(0.010)	(0.003)
HIDTA$_c$	0.908***	0.022	1.120***	0.044	0.755**	0.075*	0.176	0.157***
	(0.311)	(0.050)	(0.365)	(0.055)	(0.307)	(0.044)	(0.251)	(0.044)
Trump vote percentage$_c$	0.001	-0.010***	-0.011	-0.008***	-0.011	-0.008***	-0.003	-0.009***
	(0.006)	(0.003)	(0.010)	(0.002)	(0.008)	(0.002)	(0.006)	(0.002)
Military Base Dummy$_c$	0.663**	-0.049	0.729***	-0.022	0.428	0.007	0.526**	-0.000
	(0.275)	(0.054)	(0.244)	(0.047)	(0.294)	(0.045)	(0.267)	(0.048)
State FE		Yes		Yes		Yes		Yes
Observations	3106	3106	3106	3106	3106	3106	3106	3106
Kleibergen-Paap F-stat		6.218		7.389		8.571		6.052
Hansen J Stat p-value		0.659		0.920		0.769		0.101

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests. Transferred equipment is broken down in four categories, weapons, vehicles, gears and others. For each category the instrumental variable estimation and its first stage are depicted. Equipment transfers over 2016-2019.

Table A14: Effect of militarization 21 days before and after 25 May

	(1) Protests	(2) BLM	(3) Covid	(4) Election	(5) Violent
Panel A: Before 25 May					
IV					
$\ln(equipment)_c$	0.003	-0.014**	-0.002	-0.001	-0.002
	(0.008)	(0.005)	(0.010)	(0.001)	(0.002)
Hansen J Stat p-value	0.053	0.215	0.055	0.552	0.323
OLS					
$\ln(equipment)_c$	0.002**	-0.000	0.002*	-0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
R^2	0.211	0.063	0.211	0.005	0.058
Panel B: After 25 May					
IV					
$\ln(equipment)_c$	0.080***	0.075***	-0.003	-0.001	-0.013
	(0.026)	(0.028)	(0.008)	(0.001)	(0.010)
Hansen J Stat p-value	0.078	0.108	0.744	0.909	0.248
OLS					
$\ln(equipment)_c$	0.015***	0.015***	0.000	-0.000*	0.003***
	(0.002)	(0.002)	(0.001)	(0.000)	(0.001)
R^2	0.195	0.195	0.154	0.189	0.288

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests. Both IV and OLS regressions include 3,106 observations. Regressions include Median Household Income, Population, Black population percentage, HIDTA, Trump vote percentage and Military Base Dummy as controls. 21 days before and after 25 May. Equipment transfers over 2016-2019. First stage results and Kleibergen-Paap F-stat as in Table 5.

Table A15: Effect of of militarization on incidence of protests, adding Bove and Gavrilova (2017) instrument

	(1) First Stage	(2) IV
$\ln(land)_c$	-0.075	
	(0.131)	
Disposition Center Dummy$_c$	0.362	
	(0.256)	
Share of years receiving equipment in 1990-2019	27.232***	
	(1.351)	
$\ln(equipment)_c$		0.025***
		(0.003)
Median Household Income$_c$	0.035***	0.005***
	(0.009)	(0.001)
Population$_c$	-0.311	-0.026
	(0.227)	(0.023)
Black population percentage$_c$	0.003	-0.005
	(0.010)	(0.003)
HIDTA$_c$	0.570**	0.145***
	(0.244)	(0.034)
Trump vote percentage$_c$	0.002	-0.009***
	(0.008)	(0.002)
Military Base Dummy$_c$	0.278	0.045
	(0.243)	(0.027)
State FE		Yes
Observations	3106	3106
Kleibergen-Paap F-stat		139.909
Hansen J Stat p-value		0.003

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Regressions on incidence of protests.

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