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Protests and Police Militarization

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Abstract

What is the role of the militarization of law enforcement agencies in the U.S. in the 2020 wave of protests? This paper shows that aggregate transfers of military equipment up to 2019 increased both the incidence and number of protests in a given county in 2020. However, militarization is not a significant determinant neither of violent protests nor of COVID-related protests. Hence, with our results mostly driven by protests related to the BLM movement, we argue that the 2020 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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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, mostly after the May event (see Figure 1 and Figure A1 in the appendix). Protests occurred in more than half of U.S. counties (see Table 1), with more than twenty thousand protests in 2020 recorded by the recently launched "U.S. Crisis Monitor" (US Crisis Monitor, 2020). Within this context, this paper aims to understand what is the role of militarization of law enforcement agencies (LEAs) in determining the incidence and extent of different types of protests.

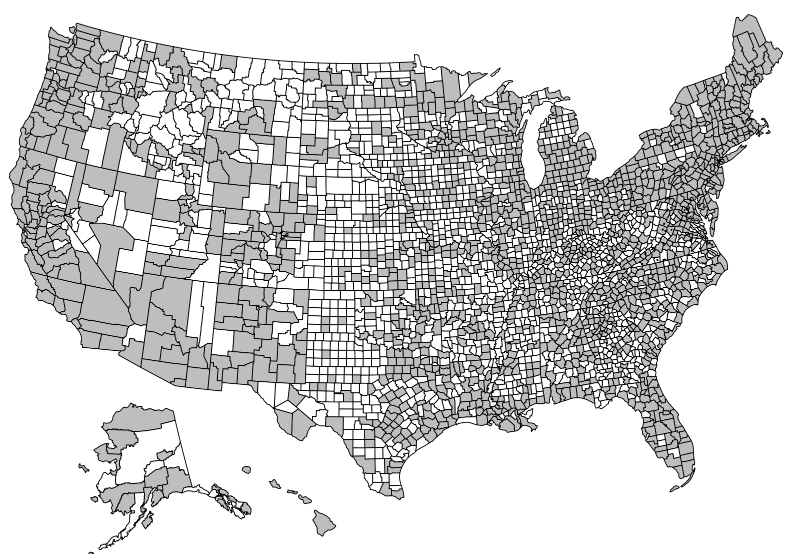

Figure 1: Incidence of protest by county, gray for counties that experienced at least one protest in 2020 (53% of the total number of counties), white for counties with zero protests. Out of all counties 11.5% experienced at least one violent protest.

Indeed, the militarization of LEAs is often linked to protest movements in the political and public debate (Browne, 2020; Lehren et al., 2020). Actually, both the 2014 and 2020 events were followed by scrutiny of certain police practices and a broader debate on police defunding and racial discrimination in the use of police force. In the aftermath of the Ferguson events, Executive Order 13688 of President Obama in 2015 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"*. Executive Order 13688 precisely aimed at better controlling the use of military equipment in the hands of LEAs by imposing restrictions on the "1033 Program". Briefly put, the 1033 Program, run by the Department of Defense, reallocates excess military equipment returned to the U.S. by military units around the world to local LEAs (see Table 1 for descriptive statistics on values of such transfers).This program has been 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) with President Obama's restrictions, later revoked by President Trump's Executive Order 13809 in 2017. Also in the aftermath of the Minneapolis events in 2020, brutal police tactics were hotly debated (H.R.1280 - George Floyd Justice in Policing Act of 2021).

Despite an apparent link between militarization of LEAs and protests, so far there has been no systematic analysis of the effect of militarization 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 recently released Armed Conflict Location and Event Dataset (ACLED) of protests in 2020 to empirically explore the following questions: How does the level of accumulated military transfers through the 1033 program available to LEAs in a given county in 2019 affect the probability that a county had a protest in 2020? And is this effect heterogeneous across different protest types?

We follow the empirical literature on the 1033 Program and use an instrumental variable approach (Bove and Gavrilova, 2017; Harris et al., 2017; Masera, 2021b) while combining ACLED protest data (US Crisis Monitor, 2020) with data on the 1033 program (Gunderson et al., 2021). With this setup, we obtain causal estimates on the effect of militarization on the extensive and intensive margin of protests. In a nutshell, we find that militarization increases the incidence and number of protests in a county.

To better understand the forces behind our benchmark results, we restrict attention to different types of protests. First, we show that militarization is not a significant determinant of the incidence of violent protests. Since transfers through the 1033 program do not increase violence in protests, this equipment does not seem to be used in violent responses by LEAs against protesters or trigger violence by the protesters. However, it does not seem to deter violence in protests either.

Second, we further identify protests linked to the Black Lives Matter (BLM) movement or COVID-related protests, which are two main categories of events in our data (47.3% of the total protests in our sample were BLM related and 18.3% were COVID related). Our results when restricting attention only to BLM protests are qualitatively indistinguishable from our benchmark results. Instead, COVID-related protests appear to be diverse since their incidence is not linked to militarization. These results suggest that militarization increases the incidence of protests, precisely because of protests linked to police attitudes. This conclusion corroborates evidence on citizens disapproving militarized LEAs (Mummolo, 2018) and is at odds with research showing that citizens provide electoral rewards to sheriffs whose counties are more militarized (Mavridis et al., 2021).[1]

Overall, our analysis contributes and extends knowledge in two strands of research that

[1]Differences between the set of voters participating in sheriff elections and protesters could be a natural explanation of these seemingly contradictory results.

have so far only been studied independently: protests and the 1033 program. The literature on the 1033 program is recent and ongoing, 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), and civic engagement (Insler et al., 2019). 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). In this note, we follow the instrumental variable approach often used in this literature to uncover the causal effect of militarization on protests.

The literature on protests and policing or repression, instead, is vast and a detailed review is beyond the scope of this paper. The reader can refer to earlier references on policing and repression of social movements (Earl et al., 2003; Earl, 2003; Davenport et al., 2019, 2011) or recent work 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). Note that while we focus on a determinant of 2020 protests, we here join Reny and Newman (2021) that also focuses on the 2020 protests but instead provides evidence on the capacity of protests in shaping citizens' attitudes (Lee, 2002; Mazumder, 2018; Wasow, 2020).

2 Data and Methods

2.1 Data

Our analysis is carried out at the county level and has two major data components. Data on the level of militarization of LEAs in 2019 and information on the incidence and number of protests in 2020. We use publicly available data and codes included in Gunderson et al. (2021) replication files to calculate the accumulated values of transfers for each county from

1990 until 2019 as provided by the U.S. Department of Defense.[2] Given valid concerns on the 1033 data (Lowande, 2021; Gunderson et al., 2021), in the appendix we detail why they serve well our purposes and show that our results are robust to other definitions of transfers. Following Bove and Gavrilova (2017); Mavridis et al. (2021), we classify equipment in four categories: gears (e.g., electronic equipment, detectors, etc.), vehicles, weapons, and others (e.g., office material and consumables). We also use a fifth total category that includes all four of them.

We use data on U.S. protests in 2020 from the publicly available Armed Conflict Location and Event Dataset (ACLED)[3] (Raleigh et al., 2010). For our benchmark analysis, we focus on overall protests. Figure 1 shows the counties in which there was at least one protest in 2020 and Table 1 reports summary statistics of our protests data (also about sub-categories of protests that we use in our analysis).

Several other variables are used in the empirical analysis. County demographics, median household income, and land area data come from the U.S. Census. Electoral data results are extracted from the MIT Election Data and Science lab (MIT Election Data and Science Lab, 2018) while identification of a county as High Intensity Drug Trafficking Area is taken from the replication files of Harris et al. (2017).

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

Table 1: Summary statistics

	Mean	St. dev.	Min	Max
Protests:				
Incidence	0.530	0.499	0	1
Number	7.03	23.82	0	601
Violent protests:				
Incidence	0.115	0.320	0	1
Number	0.535	3.73	0	139
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
Equipment transfers:				
Value gears	$1,865	$36,692	$0	$5,026,050
Value vehicles	$9,888	$97,235	$0	$6,885,450
Value weapons	$938	$18,914	$0	$2,084,382
Value others	$611	$14,018	$0	$1,413,249
Value total	$13,303	$116,251	$0	$6,929,373

Notes: Summary statistics at the county level for 3,143 observations. Data on protests refer to 2020 events. Data on equipment transfers refer to aggregate values of transfers available in 2019.

2.2 Econometric Method

Our objective is to find the causal relationship between the level of LEA militarization of a county and the incidence and number of protests. For the incidence of a protest we estimate the following linear probability model:

$$Prot_c = \beta_1 \ln(equip_c) + \beta_2 X_c + \alpha_s + \epsilon_c. \tag{1}$$

$Prot_c$ is our dependent variable that captures the incidence of a protest in county c in state s. It takes value one if there was at least one protest registered in county c in state s in 2020 and zero otherwise. The total cumulative value of equipment that is present in

2019 in county c is given by $equip_c$. It can represent any of the four categories of transfers (gears, vehicles, weapons, others) or the total of the four categories. X_c is a matrix of controls: county median household income, population, share of black population, whether the county is classified as a High Intensity Drug Trafficking Area (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. With the number of protests as our dependent variable, we estimate a Poisson instrumental variables regression where the right-hand side is the same as in (1).[4]

A challenge for the estimation of our specifications is the possible endogeneity of the level of militarization of a county ($equip_c$), in that such transfers may be correlated with other (omitted) attributes that influence the occurrence 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; Mavridis et al., 2021) and implement an instrumental variable approach using the (log) of the geographical size of a county as an instrument (i.e., an instrument also used in this literature). 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 federal agency in charge of the process explicitly encourages large counties to apply (Harris et al., 2017). The results will verify that this instrument is indeed a significant predictor of transfers.[5]

[4]We estimate the number of protests models using a Poisson IV estimation with a control function implemented in Stata using the *ivpoisson* command.

[5]In the appendix we discuss in more detail how and why our instrument varies from existing approaches (Bove and Gavrilova, 2017; Harris et al., 2017; Mavridis et al., 2021); Tables A2 and A3 of the appendix demonstrate that our results are robust to adding some of the other instruments used in the literature.

3 Results

We first present our benchmark results on overall protests as well as the corresponding quantification exercise. We then separately focus on different protest types.

3.1 Benchmark Results

Our benchmark results on the likelihood and number of protests are presented in Tables 2 and 3. In terms of Table 2, we see that the probability that a county had a protest in 2020 is increasing in the value of military transfers the county had received. The effect is positive and significant at least at the 5% level across all categories of transfers and for their total value. The qualitative results are very similar for the number of protests, as shown in Table 3, although the effects are stronger for vehicles and other items (significant at 1%) than for the other categories (significant at 10%). These tables show that the instrument is always highly significant and the Kleibergen-Paap F-stat is generally quite high, confirming the relevance of the instrument.[6]

In terms of our control variables, two regressors display the same type of effect on the incidence and number of protests. Counties identified as HIDTA are more prone to protests while higher percentages of votes for Donald Trump (in the 2016 presidential election) decrease the likelihood and number of protests. With Donald Trump still in office at the time of the protests, the latter effect illustrates that Republican voters are protesting less than Democrats and is consistent with Republican voters being tolerant to the overall use of guns. In light of the Black Lives Matter movement, it may be surprising to see that the share of black population does not affect the incidence of protests and it is only marginally significant and negative for the number of protests. Given the relevance of BLM, we revisit these

[6]Table A1 in the appendix is the equivalent of Table 2 but estimated with ordinary least squares. Our key regressor of interest is always positive and significant but the magnitude is much smaller, indicating a negative bias in such estimates.

Table 2: Incidence of protests

	Gears	Vehicles	Weapons	Others	Total
Second stage					
ln(equip)	0.079**	0.058***	0.070**	0.101***	0.051**
	(0.032)	(0.020)	(0.032)	(0.028)	(0.024)
Median Household Income	0.003*	0.003**	0.004**	0.004**	0.004***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Population	-0.121*	-0.101*	-0.073	-0.205**	-0.045
	(0.070)	(0.059)	(0.056)	(0.089)	(0.045)
Black population percentage	-0.004	-0.004	-0.004	-0.005	-0.003
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
HIDTA	0.097**	0.081**	0.091**	0.141***	0.090*
	(0.041)	(0.038)	(0.044)	(0.043)	(0.046)
Trump vote percentage	-0.008***	-0.008***	-0.008***	-0.009***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
First Stage					
ln(land)	0.973***	1.342***	1.103***	0.763***	1.512***
	(0.166)	(0.270)	(0.170)	(0.209)	(0.252)
Median Household Income	0.050***	0.065***	0.048***	0.030**	0.065***
	(0.016)	(0.021)	(0.013)	(0.012)	(0.018)
Population	1.794***	2.137***	1.350***	2.238***	1.301**
	(0.693)	(0.764)	(0.502)	(0.640)	(0.662)
Black population percentage	-0.000	-0.011	-0.010	0.004	-0.019
	(0.021)	(0.023)	(0.018)	(0.015)	(0.025)
HIDTA	1.065***	1.749***	1.302***	0.404	1.790***
	(0.361)	(0.455)	(0.338)	(0.289)	(0.438)
Trump vote percentage	-0.028**	-0.035**	-0.030***	-0.005	-0.044**
	(0.012)	(0.016)	(0.011)	(0.010)	(0.017)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	34.6	24.8	42.0	13.3	35.9

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

considerations later by focusing on BLM-related protests. Higher levels of median household income lead to a higher probability of protests but have no effect on their numbers. Instead, there is some evidence that larger populations may lead to a decrease in the likelihood of protests (although the associated point estimates are not systematically significant across categories of equipment) but, conditional on protests occurring, to exert a positive effect.[7]

The results in Tables 2 and 3 demonstrate that the much touted link between militarization and protest is more than just anecdotal evidence or journalistic headlines. People do react to militarization and let their views known in the face of it.

3.2 Quantification

Having established a causal effect of the transfer of equipment to a county on the incidence and number of protests, it is important to quantify the impact of such results. To this end, we focus on counties that have not received any equipment throughout the sample period, and we calculate by how much would their probability of protest and the number of protests increase, had they obtained the 10^{th} percentile, the median, or the 90^{th} percentile of the distribution of total values of transfers (for the counties that have received some equipment).

The results of this exercise appear in Table 4. Interestingly, even a small transfer of equipment, at the bottom 10% of the distribution, would display quite a sizable effect. It would increase the probability of having a protest from statistically insignificant to about 50% (and significant). Higher level of transfers would increase this probability further, but it is clear that a major effect arises from having some transfers in the first place. In terms of number of protests, a transfer worth 10% of the distribution almost doubles the expected

[7] Table A5 in the appendix reports results when adding, one at the time, two more regressors: per capita crime rate (arrests for murder, manslaughter, rape, robbery, aggravated assault, burglary, and vehicle theft) in 2015 and a dummy if there had been a mass shooting in a county or in an adjacent one in the years 2016-2020. These extra regressors are almost never significant while the point estimates of our regressor of interest are virtually unchanged.

Table 3: Number of protests

	Gears	Vehicles	Weapons	Others	Total
Second stage					
ln(equip)	0.264*	0.211***	0.253*	0.348***	0.177*
	(0.138)	(0.081)	(0.136)	(0.125)	(0.099)
Median Household Income	0.006	0.006	0.008	0.009	0.008
	(0.008)	(0.006)	(0.007)	(0.007)	(0.006)
Population	1.791*	1.529*	1.792*	1.551*	1.840**
	(0.955)	(0.859)	(0.915)	(0.939)	(0.871)
Black population percentage	-0.029*	-0.026*	-0.027*	-0.031*	-0.025*
	(0.016)	(0.014)	(0.014)	(0.016)	(0.015)
HIDTA	0.640***	0.617***	0.609***	0.799***	0.611***
	(0.169)	(0.154)	(0.187)	(0.177)	(0.187)
Trump vote percentage	-0.067***	-0.068***	-0.066***	-0.073***	-0.066***
	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
First stage (control function)					
ln(land)	0.973***	1.342***	1.103***	0.763***	1.512***
	(0.164)	(0.267)	(0.168)	(0.207)	(0.250)
Median Household Income	0.050***	0.065***	0.048***	0.030***	0.065***
	(0.016)	(0.021)	(0.013)	(0.011)	(0.018)
Population	1.794***	2.137***	1.350***	2.238***	1.301**
	(0.685)	(0.756)	(0.496)	(0.633)	(0.655)
Black population percentage	-0.000	-0.011	-0.010	0.004	-0.019
	(0.021)	(0.023)	(0.018)	(0.015)	(0.025)
HIDTA	1.065***	1.749***	1.302***	0.404	1.790***
	(0.357)	(0.450)	(0.334)	(0.285)	(0.433)
Trump vote percentage	-0.028**	-0.035**	-0.030***	-0.005	-0.044***
	(0.012)	(0.016)	(0.011)	(0.009)	(0.017)
ρ	-0.215	-0.152*	-0.169	-0.313**	-0.100
	(0.142)	(0.083)	(0.136)	(0.127)	(0.099)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110

Notes: ρ is the coefficient of the residual variable included to control for the endogeneity of ln(equip). Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 4: Quantification exercise

	Incidence of protest	Number of protests
0	0.141	0.565***
	(0.140)	(0.034)
10%	0.507***	0.979***
	(0.032)	(0.043)
50%	0.736***	1.383***
	(0.139)	(0.114)
90%	0.869***	1.691***
	(0.202)	(0.183)
Observations	1,014	1,014

Notes: Standard errors in parentheses clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Effects shown for no transfers, and total transfers at the 10th percentile, the median, and the 90th percentile of the distribution of total values of transfers among the counties that had received some equipment.

number of protest, with the effect becoming much more pronounced for higher levels of transfers (e.g., almost 2 protests at the 90^{th} percentile).

In conclusion, not only the results are statistically significant but they are quantitatively meaningful.

3.3 Protest types

So far our results focused on all protest events present in the ACLED dataset without further distinction among different types of protests. However, the data allows us to check if a county's militarization level has a heterogeneous effect across different protest outcome variables. We pay attention to three different categories: violent protests, BLM protests, and COVID protests. Table 1 provides summary statistics across these three types of protests.[8]

[8] We focus only on the incidence of these types of protests and not their overall numbers because they are much fewer when the data is split in this way.

3.3.1 Violent Protests

Our dependent variable is considering only the incidence of violent protests in a given county. Here, we follow the classification of ACLED where any protest that involved violence (either by protesters, police, or both) is classified as such. Approximately 20% of all protests involved violence (see Table 1). The results of this exercise are presented in Table 5.[9] It is interesting to see that no category of equipment (or the total) is significant in affecting the overall incidence of violent protests. The only variables that significantly and positively affect the incidence of violent protests are a county's population and whether it has been designated a HIDTA. The share of votes for Donald Trump has a small but significantly negative effect, while median household income is only sometimes significant (and positive) at the 10% level.

Table 5: Incidence of violent protests

	Gears	Vehicles	Weapons	Others	Total
ln(equip)	-0.004	-0.003	-0.003	-0.004	-0.002
	(0.012)	(0.009)	(0.011)	(0.016)	(0.008)
Median Household Income	0.002	0.002*	0.002*	0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population	0.266***	0.265***	0.264***	0.270***	0.263***
	(0.091)	(0.089)	(0.087)	(0.099)	(0.085)
Black population percentage	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
HIDTA	0.186***	0.187***	0.186***	0.184***	0.186***
	(0.032)	(0.032)	(0.032)	(0.030)	(0.032)
Trump vote percentage	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	34.6	24.8	42.0	13.3	35.9

Notes: Second stage estimates. First stage as in Table 2. Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

[9] Note that Tables 5, 6 and 7 include only second-stage results since the first stages are identical to the ones of Table 2.

3.3.2 BLM and COVID protests

We replicate our analysis on the determinants of protests restricting attention to the incidence of protests that are either linked to the BLM movement or are COVID related.[10] Tables 6 and 7 correspond to regression results of the incidence of BLM protests and COVID protests, respectively. We see that, across the board, equipment has a positive effect on the incidence of BLM protests, while it has no significant effect on the incidence of COVID protests.

Table 6: Incidence of BLM protests

	Gears	Vehicles	Weapons	Others	Total
ln(equip)	0.074**	0.054**	0.066*	0.095***	0.048*
	(0.036)	(0.022)	(0.035)	(0.031)	(0.026)
Median Household Income	0.003	0.003*	0.003*	0.004**	0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Population	-0.075	-0.057	-0.030	-0.154*	-0.004
	(0.074)	(0.061)	(0.061)	(0.086)	(0.049)
Black population percentage	-0.005*	-0.005*	-0.005*	-0.006*	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
HIDTA	0.098**	0.083*	0.092*	0.139***	0.091*
	(0.046)	(0.042)	(0.048)	(0.042)	(0.051)
Trump vote percentage	-0.009***	-0.009***	-0.009***	-0.010***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	34.6	24.8	42.0	13.3	35.9

Notes: Second stage estimates. First stage as in Table 2. Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

These results indicate that the documented increase in protests in our benchmark specifications is mostly driven by BLM protests. This is not surprising given that out of all counties that had at least one protest, 87.9% of them had at least one BLM protest. Instead, out of all counties that had at least one protest, only 43.5% of them had a COVID-related protest. Putting together these pieces of evidence, our results precisely highlight the rationale behind many of the 2020 protests triggered by the death of George Floyd against racial bias and ex-

[10] In the appendix we detail how we classify a protest as BLM or COVID related.

Table 7: Incidence of COVID protests

	Gears	Vehicles	Weapons	Others	Total
ln(equip)	0.017	0.012	0.015	0.021	0.011
	(0.020)	(0.014)	(0.018)	(0.023)	(0.014)
Median Household Income	0.004***	0.004***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Population	0.140	0.144*	0.150*	0.122	0.156*
	(0.088)	(0.086)	(0.084)	(0.096)	(0.080)
Black population percentage	-0.002	-0.001	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
HIDTA	0.202***	0.199***	0.201***	0.211***	0.201***
	(0.034)	(0.033)	(0.034)	(0.033)	(0.035)
Trump vote percentage	-0.007***	-0.007***	-0.007***	-0.008***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	34.6	24.8	42.0	13.3	35.9

Notes: Second stage estimates. First stage as in Table 2. Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

cessive use of police force. Military transfers through the 1033 programme up to 2019 prove a robust determinant of protests in 2020. This result corroborates that the causal effect of transfers through the 1033 programme on protests is mainly driven by the BLM movement. Instead, the presence of militarized police per se does not seem to be a determinant of protests linked to COVID.

4 Discussion

We provide robust evidence on the causal effect of militarization on protests. Transfers of military equipment increase the incidence and number of protests in a given county, particularly for those linked to BLM activism. These results suggest 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 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.

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 related protests. This result, particularly when read jointly with the fact that militarization is not increasing violence in protests, highlights that despite citizens disapproving militarization, citizens' right to protest is not harmed by militarization.

Appendix

A1 Data and variable construction

We define our protest variables making use of the full 2020 version of the Armed Conflict and Event Dataset (ACLED) (US Crisis Monitor, 2020). We define as *protest* any event coded as "Protests" or "Riots" (Raleigh et al., 2010) in ACLED. With such definition, Figure A1 depicts the timeline of total protests in the US in 2020. We define as *violent protest* any event whose sub-event type is "Protest with intervention", "Excessive force against protesters", "Mob violence" or "Violent demonstration".

To look further into different types of protests, we create two more versions to characterize Black American participation and events related to the ongoing pandemic. We define as *BLM protest* any protest as defined above and that had some organized Black actor participation: the participating actors in the protest had the words "BLM", "Black" or "African American" in their names or descriptions. Moreover, we define as *COVID protest* any protest as defined above if the notes in ACLED dataset included the words "Covid", "corona", "pandemic", or "lockdown".

When studying the incidence of each type of protests, 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. The count of such events is used when investigating the intensive margin.

Data on transfers of military equipment come from the replication files of Gunderson et al. (2021). These data are a 2019 release of military transfers to each county starting in 1990. Summing up all transfers from 1990 to 2019 at the county level, we have a measure of registered transfers available in a given county in 2019. This variable serves well our purposes, as we aim to estimate how the transfers available in a given county in 2019 affect the incidence and count of protests in 2020. Note that concerns about data released by the Ministry of Defense have been raised in the literature (Lowande, 2021; Gunderson et al., 2021), which are particularly pronounced when one focuses on the time series dimension of such data (Lowande, 2021). Given that our focus is on the cross section, these concerns are alleviated in the current setting. However, for further scrutiny, in our robustness section below, we also show that our results are robust when using a 2015 data release (Mavridis et al., 2021), hence estimating how transfers available in a given county in 2015 affect the incidence and count of protests in 2020. In these robustness checks, we also consider only transfers in the past 1, 5 or 10 years, or focus only on "controlled" items (as defined below).

Our control variables are derived from various sources. Median household income, population and 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). Mother Jones (Mother Jones, 2020) provides us with county data on mass shootings while data on crime comes from Kaplan (Kaplan, 2019). Electoral data is extracted from the MIT Election Data and Science lab (MIT Election Data and Science Lab, 2018) and the variable HIDTA is taken from the replication files of Harris et al. (2017).

A2 Methodology, specifications and robustness checks

Our benchmark results on the incidence of protests prove robust to a number of alternative specifications. Below we provide several alternative specifications when varying the methodology and the definition of our variables. We also provide several robustness checks adding further controls.

IV approach

We follow Harris et al. (2017); Bove and Gavrilova (2017) in using an IV approach to tackle obvious endogeneity concerns. For comparison purposes, we include the results of a least squares estimation of the incidence of protests in Table A1. The point estimates for our key regressors are always positive and significant but their magnitudes are systematically smaller, indicating a negative bias in such specifications and supporting the use of an IV approach (given that the instrument is significant and the Kleibergen-Paap F-statistic is high).

Note that our chosen instrument varies slightly from the existing literature but we think serves well our purpose. The instrument is inspired by Harris et al. (2017) where they use as instruments not only the size of a county, but also (the inverse of) its distance from the closest and sixth-closest Field Activity Center (FAC),[11] and whether the county has ever been designated High Intensity Drug Trafficking Area (HIDTA).[12] In our setting HIDTA is always significant in the second stage and as such is not suitable as an instrument. Focusing on the incidence of protests model, in Table A2 we include measures of distance to the closest (D^1) and sixth-closest (D^6) FAC as instruments. Compared to Table 2 we see that the estimates are qualitatively similar in the second stage. However, D^1 is never significant in the first stage and D^6 is only significant at the 10% and only for one category of transfers. Instead, ln(land) is always highly significant in the first stage. As a result of the insignificance of the added instruments, the Kleibergen-Paap F-stat is much lower across the board in Table 2. Having more instruments than endogenous variables we are able to test for overidentifying restrictions. The Hansen J-stat at the bottom of the table shows that we never reject the null hypothesis that the instruments are valid and correctly excluded from the second stage in all cases except for weapons, for which we reject the null hypothesis but only at 10% level. In consideration of the lower Kleibergen-Paap F-stats and one case of rejection of the overidentifying restrictions, our benchmark regressions only use ln(land) as an instrument.

Notice that Bove and Gavrilova (2017) use yet a different instrument: the share of years

[11] Centers from which the equipment is distributed.

[12] Since in their setup there is the time dimension as well, all of their instruments were interacted by the total value of transfers in the US in a given year.

a county has received equipment during their sample.[13] We constructed the same instrument for our sample period and used it as an additional instrument along with the size of a county in the incidence of protests specification. The results of such specification, shown in Table A3, 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 (except for the total value of transfers, in which case the p-value is 0.12). Thus, we only show this version as a robustness check.

In conclusion, our choice to only keep ln(land) as an instrument is justified since on the one hand D^1 and D^6 do not seem to affect the qualitative results while worsening the performance of the instruments. On the other hand, the instrument suggested by Bove and Gavrilova (2017) seems to suffer from endogeneity in our setup (possibly because of the purely cross-sectional nature of our analysis compared to their time-series setting).

Alternative transfer specifications

Our results prove robust to alternative definitions of our main regressor. Recall that in our main regressions, ln(equip), represents the value of all transfers of a particular category of equipment a county has received until and including 2019. There has been a discussion about the quality of equipment transfers data, and concerns around this data (Gunderson et al., 2021; Lowande, 2021). Therefore, we engage in several robustness checks to verify whether our results are robust to different calculations of the amount of transfers.

Panel A of Table A4 shows estimates of the incidence of protests if we focus only on transfers that occurred in 2019. The equipment estimates are all positive and significant at the 1% level. However, the Kleibergen-Paap F-statistic is much lower in all cases. In

[13] Similar to the previous case, this is interacted with US military spending in each year to generate a time-varying instrument.

Panel B, we focus our attention on transfers to a county over the 5 years previous to 2020 (i.e., 2015-2019). Again, the equipment estimates are all significant and positive, and the Kleibergen-Paap F-statistics improve compared to Panel A. In Panel C we consider transfers over the past 10 years and recover similar findings on the relevance of transfers on the likelihood of protest but now the Kleibergen-Paap F-statistics are all much improved.

In Panel D the equipment regressor includes all transfers until 2015 but using transfer data released in 2015 and aggregated at the county level using the process described in Mavridis et al. (2021). The positive and significant effect of equipment on the probability of a county having a protest is maintained in these specifications and the Kleibergen-Paap F-statistics perform well.

Finally, in Panel E we turn our attention to transfers of only "controlled" equipment a county has received until and including 2019. After the 2014 Ferguson uprising and the widespread criticism of police militarization that followed, President Obama in 2015 prohibited the transfer of certain items and introduced additional control measures for the transfer of "controlled" equipment (Executive Order 13688). "Controlled" items included manned aircrafts, unmanned aerial vehicles, armored and tactical wheeled vehicles, command and control vehicles, under .50-caliber firearms and ammunition, explosives and pyrotechnics, breaching apparatus, riot batons, helmets, and shields. Again, in all cases our qualitative findings do not change and the Kleibergen-Paap F-statistics are similar to the ones in Table 2.

Additional controls

As a further robustness check we add, one at a time, two more variables in our incidence of protests specification. The results appear in Table A5. The first one is per capita crime rate (arrests for murder, manslaughter, rape, robbery, aggravated assault, burglary, and vehicle theft) in 2015, as a recent literature has focused on the effect of militarization on crime (Bove and Gavrilova, 2017; Harris et al., 2017; Gunderson et al., 2021; Masera, 2021b). In Panel A of Table A5 we see that the inclusion of the crime variable, which is never significant, does not affect the coefficients of our equipment variables which are virtually identical to the ones in Table 2. The second additional control is a dummy variable that is equal to 1 if there has been a mass shooting in that county or in an adjacent one in the years 2016-2020. As shown by the results in Panel B of Table A5, the inclusion of the mass shootings variable also leaves the transfers coefficients basically unchanged, and is only significant (at the 5% level) in the case of total transfers.

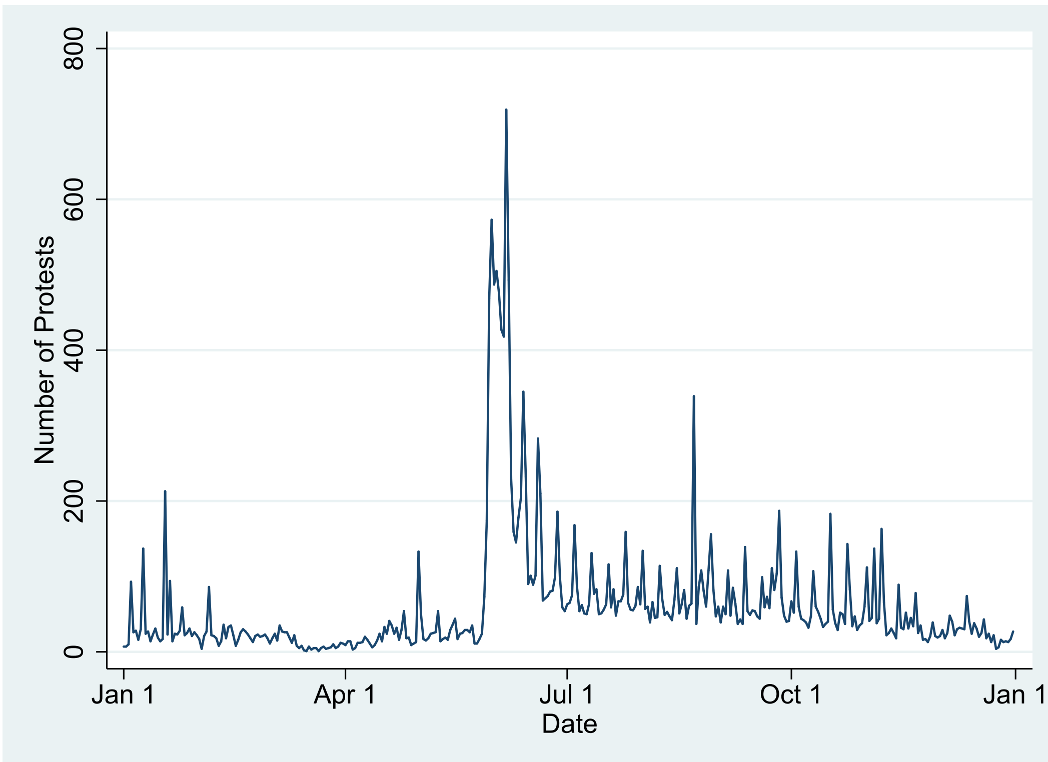

Figure A1: Timeline of number of total protests in the US over 2020.

Table A1: Incidence of protests - least squares estimation

	Gears	Vehicles	Weapons	Others	Total
ln(equip)	0.013***	0.015***	0.019***	0.009***	0.019***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Median Household Income	0.006***	0.006***	0.006***	0.006***	0.006***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Population	0.010	0.000	0.007	0.015	0.007
	(0.031)	(0.030)	(0.032)	(0.036)	(0.029)
Black population percentage	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
HIDTA	0.174***	0.161***	0.163***	0.185***	0.153***
	(0.038)	(0.036)	(0.036)	(0.039)	(0.035)
Trump vote percentage	-0.009***	-0.009***	-0.009***	-0.009***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.719***	0.703***	0.670***	0.746***	0.633***
	(0.214)	(0.208)	(0.216)	(0.215)	(0.212)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
R^2	0.30	0.31	0.31	0.29	0.32

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A2: Incidence of protests - Robustness check adding D^1 and D^6 as instruments

	Gears	Vehicles	Weapons	Others	Total
Second stage					
ln(equip)	0.087***	0.065***	0.075**	0.107***	0.057**
	(0.027)	(0.019)	(0.032)	(0.027)	(0.024)
Median Household Income	0.003*	0.003**	0.004**	0.004**	0.004**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Population	-0.137**	-0.119**	-0.081	-0.218**	-0.054
	(0.062)	(0.057)	(0.055)	(0.089)	(0.042)
Black population percentage	-0.004	-0.004	-0.004	-0.005	-0.003
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
HIDTA	0.088**	0.068*	0.083*	0.138***	0.080*
	(0.039)	(0.037)	(0.044)	(0.045)	(0.046)
Trump vote percentage	-0.007***	-0.008***	-0.008***	-0.009***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
First stage					
ln(land)	1.007***	1.370***	1.117***	0.774***	1.537***
	(0.161)	(0.269)	(0.162)	(0.210)	(0.234)
D^1	5.067	4.848	3.807	5.120	4.064
	(7.673)	(7.189)	(8.108)	(6.024)	(9.103)
D^6	1682.059*	1337.097	417.334	186.992	1114.971
	(942.859)	(1123.963)	(980.887)	(744.876)	(1184.270)
Median Household Income	0.053***	0.068***	0.049***	0.030***	0.067***
	(0.014)	(0.020)	(0.012)	(0.011)	(0.016)
Population	1.735***	2.089***	1.331***	2.225***	1.259**
	(0.657)	(0.737)	(0.483)	(0.634)	(0.630)
Black population percentage	-0.002	-0.012	-0.010	0.003	-0.020
	(0.020)	(0.022)	(0.018)	(0.015)	(0.024)
HIDTA	0.987***	1.688***	1.284***	0.397	1.739***
	(0.360)	(0.458)	(0.340)	(0.282)	(0.444)
Trump vote percentage	-0.032***	-0.039***	-0.031***	-0.005	-0.047***
	(0.011)	(0.015)	(0.010)	(0.009)	(0.016)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3106	3106	3106	3106	3106
Kleibergen-Paap F-stat	17.8	13.7	20.3	6.95	27.2
Hansen J Stat p-value	0.78	0.27	0.06	0.12	0.20

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A3: Incidence of protests - Robustness check adding probability of transfers as instrument

	Gears	Vehicles	Weapons	Others	Total
Second stage					
ln(equip)	0.026***	0.024***	0.030***	0.038***	0.025***
	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)
Median Household Income	0.005***	0.005***	0.005***	0.006***	0.005***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Population	-0.015	-0.022	-0.010	-0.056**	-0.003
	(0.025)	(0.025)	(0.027)	(0.024)	(0.026)
Black population percentage	-0.004	-0.004	-0.004	-0.005	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
HIDTA	0.159***	0.144***	0.147***	0.171***	0.142***
	(0.036)	(0.034)	(0.035)	(0.039)	(0.034)
Trump vote percentage	-0.009***	-0.009***	-0.009***	-0.009***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
First stage					
ln(land)	0.088	0.399***	0.333**	0.171	0.580***
	(0.115)	(0.149)	(0.139)	(0.130)	(0.224)
Prob. of receiving equip. in 1990-2019	27.262***	29.029***	23.709***	18.218***	28.675***
	(1.030)	(1.315)	(1.430)	(0.984)	(1.980)
Median Household Income	0.011*	0.024**	0.014***	0.004	0.024***
	(0.006)	(0.010)	(0.006)	(0.006)	(0.007)
Population	-0.724***	-0.544**	-0.840***	0.555*	-1.348***
	(0.221)	(0.225)	(0.245)	(0.283)	(0.282)
Black population percentage	-0.001	-0.012	-0.011	0.003	-0.020
	(0.010)	(0.014)	(0.009)	(0.008)	(0.015)
HIDTA	-0.012	0.603*	0.366*	-0.315	0.658**
	(0.199)	(0.319)	(0.207)	(0.219)	(0.320)
Trump vote percentage	-0.006	-0.012	-0.011*	0.009	-0.022*
	(0.007)	(0.009)	(0.007)	(0.006)	(0.011)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	351.2	263.1	150.5	180.8	116.1
Hansen J Stat p-value	0.02	0.05	0.08	0.03	0.12

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A4: Incidence of protests - Robustness check with alternative transfer specifications

	Gears	Vehicles	Weapons	Others	Total
Panel A: Only 2019 transfers					
ln(equip)	0.186***	0.143***	0.352***	0.140***	0.098***
	(0.059)	(0.046)	(0.130)	(0.038)	(0.027)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	9.75	8.90	6.43	9.82	10.7
Panel B: Transfers over the past 5 years					
ln(equip)	0.150***	0.106***	0.171***	0.127***	0.092***
	(0.052)	(0.032)	(0.054)	(0.036)	(0.028)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	11.4	9.39	8.51	12.8	12.2
Panel C: Transfers over the past 10 years					
ln(equip)	0.091**	0.058***	0.100**	0.102***	0.057**
	(0.037)	(0.020)	(0.044)	(0.028)	(0.026)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	28.1	24.6	25.1	13.3	30.8
Panel D: Transfers until 2015 (2015 released data)					
ln(equip)	0.088**	0.077**	0.112**	0.112***	0.074**
	(0.039)	(0.030)	(0.045)	(0.036)	(0.030)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	38.7	22.1	12.6	11.1	26.6
Panel E: Transfers of controlled equipment					
ln(equip)	0.082**	0.060***	0.071**	0.145***	0.052**
	(0.034)	(0.020)	(0.033)	(0.046)	(0.025)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3106	3110
Kleibergen-Paap F-stat	36.4	22.8	40.9	13.4	37.9

Notes: Second stage estimates. Controls as in Table 2, estimates not reported to save on space. Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A5: Incidence of protests - Robustness with additional regressors

	Gears	Vehicles	Weapons	Others	Total
Panel A: controlling for crime					
ln(equip)	0.079**	0.057***	0.069**	0.100***	0.051**
	(0.032)	(0.020)	(0.032)	(0.028)	(0.024)
Crime rate	-0.862	-2.773	-0.793	4.857	-0.054
	(11.194)	(9.310)	(8.967)	(15.347)	(8.110)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3106	3106	3106	3106	3106
Kleibergen-Paap F-stat	34.5	24.5	41.8	13.1	35.6
Panel B: controlling for mass shootings					
ln(equip)	0.079**	0.057***	0.070**	0.101***	0.051**
	(0.032)	(0.020)	(0.032)	(0.028)	(0.024)
Shootings dummy	0.028	0.059	0.059	0.018	0.073**
	(0.057)	(0.037)	(0.042)	(0.066)	(0.033)
State FE	Yes	Yes	Yes	Yes	Yes
Observations	3110	3110	3110	3110	3110
Kleibergen-Paap F-stat	33.9	24.5	41.1	13.3	35.7

Notes: Second stage estimates. Controls as in Table 2, estimates not reported to save on space. Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

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