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DOES VIOLENT DISSENT AFFECT POLICE MILITARIZATION?

On reasons for increasing the ability to coerce

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Abstract

The last decade has seen the police becoming more militarized around the globe, retracing the steps back to the institutions violent historical roots. The phenomenon has been under scrutiny for 20 years, but the research isn't connected to other fields into which it might fit. This thesis asks if police militarization is caused by violent dissent, thereby connecting the field to wider theories of coercive capacity building and repression. I propose that the mechanism through which dissent acts on police militarization is threat to the state. 106 countries from 1960 to 2010 are examined, but I find no significant correlation of violent dissent with police militarization. The attempt is not fruitless though, as I instead discover robust but puzzling correlations relating to democracy and economic development.

Introduction **4**

Literature review **5**

Police as guardians 6

Public support 7

Threat perception 8

Research Question 9

Theory **10**

Definitions 10

Mechanism 11

Hypothesis 13

Data and Model **14**

Data sample 14

Police militarization 14

Violent internal dissent 16

Model 17

Controls 17

Results **19**

Descriptive statistics 19

Regression analysis 23

Robustness tests 26

Conclusion **27**

References **28**

Introduction

Freedom from corporal punishment and the right to a fair trial is a sign of well functioning states. However, in the last decade, there has been an increase in a trend that uproots these two foundations of modern society within a single phenomenon. That phenomenon is police militarization. It generally refers to equipping, training, organizing and changing the culture of the regular police forces to be more similar to the military. The phenomenon also includes expansion of paramilitary police forces and even using the military in civilian policing. It massively increases the ability and propensity of law enforcement to inflict bodily harm, and even without due process, leaving matters of life, death, and lifelong disability in the hands of the workers of an intensely stressful and sometimes dangerous job. A more pessimistic interpretation is that police militarization better enables the state to repress it's citizens should it find doing so desirable.

The police militarization literature is near 30 years old and repeatedly finds that the effects of police militarization is police brutality and repression. Nonetheless, the existing explanations are often unsatisfactory by leaving unanswered questions as to the greater significance of the findings. There is not much explaining whether there's a purpose to the process, in what ways it's similar to other instances of coercive capacity expansion, nor if there are empirical connections to the violent history of the police institution. Where does police militarization slot into wider theories of state repression, and how?

This thesis will attempt to fill this gap by examining if violent dissent can serve as a viable explanation for police militarization. Apparently unknown to the police militarization literature there is another field that deals with very similar questions, namely the repression-dissent literature. Within this literature there is consensus that when the political order is challenged, the state responds with repression. By asking if violent dissent affects police militarization I connect this literature to wider theories of coercion and repression, and bring the focus to the police in the repression literature, which is often overlooked therein. Answering this question will bring deeper understanding of the role of the police in society, and whether there is a reason to adopt a pessimistic view of law enforcement.

Literature review

Police militarization generally means three things in the literature. Progressive militarization of civilian police, paramilitary police forces, and military forces with policing jurisdiction. All three of these types have been called police militarization. I will continue to generally and collectively refer to these differing types as police militarization. To differentiate the types I copy Flores-Macías' & Zarkin's (2021) typology. They use a continuum of policing practice from fully non-militarizied, fully civilian police to military forces with civilian policing jurisdiction. A step up from completely civilian police is militarized police, which incorporate some characteristics of the military, most notably specialized militarized units, but overall maintain low hierarchical structure, operate under civilian law, with low access to military equipment, and are mainly tasked with public order. The next step is paramilitary police. This type has more variety. They are still police mainly tasked with public order but rely on military weapons and equipment. Paramilitary police still mostly operate under civil law but with exceptions, are hierarchically organized, and are not rarely organized under the department of defence. The most militarized form is military forces with civilian policing jurisdiction, operating under military law. It follows that they do so with full access to military equipment, fully hierarchical structure, and crucially, their methods usually entails target elimination. Other types of policing forces exist outside of this typology, like coast and border guard, state run militias, and intelligences services, but the interest of this thesis is regular policing forces.

The consequences of police militarization are consistent across all three types. Stavro & Welch (2024) show that police militarization increases the likelihood of repression, particularly torture and extrajudicial killings. Blair & Weintraub (2023) find that there is little evidence that military policing improves security, and that it likely exacerbates crime. Jefferson (1990) outlines how paramilitary policing likely amplified disorder and that unjust violence was the experience of the policed. Post-conflict, increasing police militarization as a form of deterrence capacity increases police violence (Tiscornia, 2024). Magaloni & Rodriguez (2020) find that institutional and organizational norms influence the use of violence by the police, supported in detail by Huggins et. al. (2002). If the objective is increasing state power or control through coercion, the answer is clear that militarization of policing would fulfill that aim.

Even though there are good arguments for promoting some specific aspects of the armed forces such as accountability and professionalism, see Wood (2015) and Den Heyer (2014), it cannot be contested that militarization entails blending of military and civilian functions. Den Heyer (2011), while positively inclined to the use of militarized force, does find that there are several deficiencies in public security provision. Police militarization then, fits easily within the literature about the building and use of coercion by the state. Howard (1957) writes that "…societies are orderly and peaceable only in so far as they have solved this double problem, of the subordination of military force to the political government, and of the control of a government in possession of such force by legal restraint and the popular will." The existing literature shows that police militarization is highly relevant to this, as are the neighbouring fields of counter-terrorism, counter-insurgency, civil conflict, and criminal justice.

Police as guardians

The institution of the police is, at least in theory, one that exists to safeguard the citizenry against internal threats. Because crime is an issue of public safety, it should be the first stop to explain police militarization. The literature has little, and at best mixed results on the effectiveness of police militarization on crime (Bove & Gavrilova 2017; Blair & Weintraub, 2023). As some police militarization research in Latin America has found, militarizing anti-drug efforts can cause a corresponding escalation of the use of violence by

drug-trafficking gangs (Flores-Macías, 2018). Schneider (2015) summarizes some well known anti-terror campaigns, and the side effects that counter-terrorism can have, especially in regards to human rights. Such effects can even be the purpose of terrorism according to Findley & Young (2012). Furthermore, lack of percieved effective responses to terrorism can bring about oppositional terrorist groups, like the right-wing death squads of Latin America (Schneider, 2015). In extrapolation, police militarization as a response to violent crime could itself increase violent crime though both stepping up violent operations and in decreases in effectiveness, promoting even more police militarization.

Some argue that police militarization is a contemporary aspect of race-making (Gamal, 2016). In existing studie, variations of racial discriminatory control variables are near universal. Evidence towards race being a factor is mixed, but this likely depends on the specifics of a given paper. A few scholars investigate racism as a specific cause of militarization. Jimenez et. al. (2022) links racial prejudice to support of police militarization at the local and regional level in a survey of 1006 US citizens. A follow up study links racial prejudice to actual military equipment recieved. Lawson (2019) finds a curvilinear relation between the predicted level of militarization in dollars and the percentage of non-white population in a county. The level of militarization increases to a tipping point of about 50%, after which it again decreases. Lawson suggests several mechanism, generally based on perception of threat by the majority population. However, he admits that he lacks the data for determining which of his theoretical mechanisms are responsible. Offering mixed support to Lawson's findings, Ajilore (2015) finds a negative correlation of large minority populations and police militarization in terms of armoured car procurement, but at the same time that greater residential segregation is associated with increased armoured car procurement.

Public support

Public opinion offers some additional explanation for police militarization because support for harsher law enforcement is a first-order effect of crime. In a review on if public opinion and policy responsiveness is affected by crime rate, Picket (2019) finds that the public's support for punitive policies does respond to changes in the crime rate over time, and that capital punishment policy support and use is strongly responsive to the punitive mood of the public. Turner & Fox (2017), finds that in a survey of the share who supports various forms of police

militarization, of the 25 members of congress respondents, no less than 40% support any individual form, though only 16% supported the general concept. Since Picket (2019) also finds that that public opinion indisputably matters to public officials, it implies that public opinion on police militarization may manifest through decisions by public officials. Similarly, by including the 150 police executives and 253 police officers interviewed by Turner & Fox (2017), no less than 57% of police executives and 51% of police officers support a given measure of police militarization, and 61% and 62% respectively support the general concept. This shows that a driving factor of police militarization can be the police themselves, or acting through their representatives.

In another review, Steidley & Ramey (2018), summarizes the literature on direct public support for various forms of police militarization. Their central finding is that police legitimacy is the most important factor influencing its support. Moule Jr. et. al. (2019) argues that legitimacy increases willingness to empower the police, because legitimacy is positively associated with just treatment by police. The reasoning is that just policing increases trust in further empowerment being effective, or that just policing reduces concerns about civil rights abuses. Additionally, use of specialized militarized units in hostage situations and terror attacks increased support. Howewer, as Steidley & Ramey (2018) note, none of the studies in their review include the level of police militarization in the respondents local area. This casts some doubt on whether support for police militarization extends to respondents themselves being affected by it. As Mummolo (2018) finds, news coverage of militarized police can hurt police reputation. Even so, Tiscornia & Pérez Bentancur (2024) show that support for punitive policing in the urban poor can be high even if the policing is directed towards themselves, if other forms of violence are believed to decrease as a result.

Threat perception

A recurrent object of analysis for explaining police militarization is the perception of threat by the state. Kirisci (2019) shows that paramilitary policing is higher in states with more terror incidents. Lieblich & Shinar (2017) presents a comprehensive argument. They reason that police militarization implies a presumption of internal threat by the state, a threat which is responded to by police militarization. When police militarization becomes normalized it reveals that it is no longer in response to specific circumstances, but the public collectively.

Because police militarization in this manner is not in response to an individual threat, it must be preventative. Taken together, this creates a distinction between friend and foe, and an exclusion of the foe from politics. The various anti-communism initiatives of history are the most obvious example. Huggins et. al. (2002) reveals that a universally present factor for the the police working as executioners and torturers in Brazil's military dictatorship was that their victims were designated as enemies to national security. In another vein, De Bruin (2021) comprehensively shows how states will attempt to balance their security forces to avoid coups d'etat, including various forms of police militarization. Moreover, relating to self-reinforcing effects, police militarization could reasonably be expected to create its own threat. Carriere & Encinosa (2017) show that operational militarization of police increases assaults on police officers, as does Masera (2021).

Naturally, the state's perception of threat is reflected at the individual level. Oftentimes the reasoning around national or public security is mirrored in the opinions of officers (Huggins et. al., 2002). Most prominent is perceived threat to the officers themselves. Lawson (2019) finds support for the theory that police militarization is a response to a percieved higher threat of racial minorities. Riots and civil unrest are intuitively the circumstances when threat as percieved by the police would be the highest, and it's in these cases where militarization is the most obvious. Psychiatric research indicates that police work can be severly stressful, e.g McCaslin et. al. (2006), which would ostensibly support the idea of police militarization for protection. Krajevski et. al. (2024) finds that perceived subject threat predicts the level of force used. But as Wahl (2017) and Huggins et. al. (2002) show, though in the context of extensive torture and human rights abuse, threats to officers are just as often justifications or rationalizations for the suffering they inflict. In the case of the US, Wang's (2015) data on police officers killed since 1961 that shows that policing has become much safer, and Mummolo (2018) finds evidence that it does not depend on militarization.

Research Question

To summarize, there are a few well grounded explanations dealing with crime, minority prejudice, and threat perceptions. However, explanations such as these are narrow conceptualization of the issue. For instance, Howell (2018) connects police militarization to broader issues of violence and other forms of militarization. McMichael (2017) uses the

historical development of the policing institution in arguing that there is no real difference between the police and military. As on of the state's two violent arms the police is as much a part of a state's institutionalized coercive capacity as much as the military is. Militarizing the police only reveals that capacity and the repressive nature of the institution.

Existing research focuses on domestic and proximate drivers of police militarization, it pays less attention to how police militarization fits into broader theories of coercive institution-building and state repression. With coercive capacity comes the ability to repress. However, repression is only half the story. The other half is composed of those who disagree with the political order. Together this forms the repression-dissent nexus. The repression-dissent literature is vast, but only in the last few years has the police been of interest as a perpetrator. For example, Stavro & Welch (2024) shows that police militarization increases the likelihood of repression, but what causes police miltiarization? Repression is violent, but the police doesn't need to be militarized to be violent. Since militarizing the police is undeniably increasing its violent potential, could it be in response to violence? Since dissent is met with repression, could violent dissent be met with increasing the state's ability for even more violent repression? The question is therefore: *how does violent dissent affect police militarization?*

Theory

Definitions

Keeping with the typology of Flores-Macías & Zarkin (2021), police militarization is characterized by regular police forces displaying the increasing prevalence of military equipment, tactics, and organization, with increasing legal leeway in the use of violence, in pursuit of increasingly military-focused objectives that treat citizens as enemies. Police militarization also means further militarization in the same manner, or expansion, of already militarized paramilitary police forces. Finally, police militarization includes the tasking the military with civil policing duties, or expansion of their scope. This culminates in an increase in the coercive capacity of the state, a shift from implicit to explicit state violence, and the use of violence as a first resort. Summarily, as a result of increasing killing potential with military

means, police militarization should be understood as an increase in the coercive capacity of the state.

Dissent should be understood as credible opposition to the current political order posed by groups within the territorial bounds of the state. It can refer to elites or citizens. It does not always mean active conflict, as we see police militarization in more states than those that are in violent conflict with an internal group. Armed conflict over internal territorial integrity is on one end of the scale. I reason that the other end is merely some sort of opposition to the ruling order that the use of military means may remedy. Opposition doesn't need to go as far as demanding changes to the political order, nor anti-state violence, to be threatening enough to be met with violent or deadly force. However, it should increase the probability. Thus, *violent* dissent is what I will primarily examine.

I will also be talking about threat perception by the state. Threat refers to how the state perceives dissent. Dissent is a source of threat, but it's not the only source of threat, The repression-dissent literature often talks of dissent events, which refers to anything from protests to to riots to rebellion. Attributes like size and tactics of a specific event is what defines different types of events. These are also sources of threat. Hence, threat is a mechanism through which dissent acts on the state. I am concerned with the threat of violent dissent specifically, but not any specific violent event type.

Mechanism

börjar bra

men går lite överallt

pm ökar kapacitet

mer om hur dissent orsakar pm, i en kedja

spell it out

The repression literature attempts to explain the type of coercion that states use, as well as its frequency and scale. In a seminal paper, Davenport (2007a) reviews the literature on state repression and political order. His foremost finding is that when challenges to the status quo appears, states take repressive action to counter or eliminate the behavioural threat. This finding is so consistent that he calls it a law, the law of coercive responsiveness.

I argue that police militarization is a way of enabling the coercive response by increasing coercive capacity. Violent dissent causes the public or elites to feel threatened and demand a response from the state, offering two alternative ways for threat to influence police militarization. Perhaps the people prefer Leviathan over the state of nature, or the elite wish to guarantee their privileged positions. On the frontline of the violence, the police could be an especially powerful voice. As the literature review has shown, public support matters in questions of crime and internal security policy. Pushing the state to respond not only causes a crackdown, but legitimizes the expansion of coercive capacities. The police is the obvious choice to reform. They bring both expertise and legitimacy to the task, and increasing the violent potential of the institution assigned to deal with the violent dissent is logical. Effectiveness in some areas may be lost, but are made up for in context of more violent circumstances. Mimicing the institutions that are already trained in such matters is the intuitive development. In case of greater violence, paramilitary forces or actual soldiers can be brought in or given greater civilian jurisdiction.

The last decade of the repression literature has focused on the finding that non-violent protest appears to improve democratic development, e.g Kim & Kroeger, (2019). This would suggest that more violent protest would correlate with more repressive regimes, and larger coercive capacities. Carey (2010) finds that the only dissent event type statistically related to repression is guerrilla attacks, a violent type more organized than a riot but smaller in scale than a revolution. Regan & Henderson (2002) argue that democracies, by allowing for channels for voicing dissent, reduces the opposition's need for violence. Autocracies then, by not providing such channels, incentivizes violent dissent, which is naturally percieved as more threatening, ending with more repression.

Greitens (2013) argues that ruling autocrats must balance their security forces between ability to deal with massed opposition and rival elites. Countering rival elites entails a fragmented but violent security force pulled from specific sources, while popular unrest requires the opposite. Autocrats design their coercive institutions based on the dominant threat. Police militarization would therefore seem to be in response to dissent posed by the citizenry. However, Greitens shows that autocrats threatened with popular unrest use less violence, and because they design institutions for that purpose, which could be evidence against the argument.

There are other theoretical arguments for violent response to violent dissent which don't depend as much on threat as percieved by elites or the state as a whole. Those deciding to repress are not the same ones that act it out, instead relying on specific coercive forces. Violent dissent can act as a threat to both, creating a drive to increase coercive capacity from both above and below (Turner & Fox, 2017). As covered in the literature review, there is public support for police militarization. This could be characterized as instances when the populace feels threatened by violent dissent, as in the case of Tiscornia & Pérez Bentancur (2024). The same goes for threat percieved by the police (Masera, 2021). Of course, this feeling of threat may have been articficially created by the state in the first place, like in the case of violence workers motivated by national security, but nonetheless it may have an effect.

On the other side of the coin of threat perception is legitimizing the state's response to it. The threat that violent dissent poses is the reason for a response, and the legitimacy it lends is what allows it to happen. Violent dissent can send signals that the state is unable to protect the public (Mason & Krane, 1989). If the subsequent repression is successful and without too heavy a burden on whoever supports it, the opposite happens. Absent the violence though, dissent should decrease repression as without cause the legitimacy of the regime is questioned (Davenport, 1995). A common assumption in the repression literature has been the rationalist idea that rulers carefully weigh costs and benefits of coercion before choosing the specific repressive measure, e.g Moore (2000). Carey (2010) reasons that violence and high levels of organization are two attributes of dissent that she expects to increase the risk of repression. Severe repression to low grade dissent events like demonstrations is too costly in terms of public and international legitimacy. Guerrilla warfare and revolutions are instead both violent and organized, so the cost of not using widespread violence is then the higher. Violent dissent thus legitimizes increases in coercive capacity, and its use. By bringing violence to the minds of the populace it may produce calls for militarized presence specifically.

Hypothesis

Militarizing policing is arguably a rational response to violent dissent. It balances the need for less violent police work at a wider societal level, with preparation in case of need for greater coercive force. As the level of violent dissent grows, so too does the call or need for coercion,

and the scope increases from militarizing civilian police to paramilitary expansion to assigning civilian policing jurisidiction to the military. The policing institutions are the first choice for militarization in the face of violent dissent because they are provide expertise that the military lacks. For example, the police generally have stronger intelligence-gathering capacities compared to the military. (Davenport, 2015). Furthermore, they are an obvious choice because they are part way to being soldiers already, as the internally violent arm of the state. Using the armed forces in civilian policing flips this on its head, but if the magnitude of dissent is great enough it's not unreasonable to expect states to favour even greater killing ability power over law enforcement ability. Thus, militarization represents an increase in killing ability. Militarizing policing directs that killing ability towards violent dissenters. As the scale of dissent grows, so too will the militariziation of the police grow to match it. My hypothesis is therefore: *violent dissent contributes to police militarization.*

Data and Model

Data sample

To measure militarized police and paramilitary police I will use De Bruin's (2021) State Security Forces (SSF) dataset. The dataset covers the years 1960-2010, and 375 security forces in 110 countries. To remedy the absence of military forces with policing jurisdiction in the SSF dataset I will also use the Multidimensional Measures of Militarization (M3) dataset, complementing the measurement of police militarization (Bayer et. al., 2025). The dataset covers the years 1990-2020 and 156 countries. To measure violent dissent I will use the Opposition Movements and Groups (OMG) dataset by Dahl et. al. (2025). The dataset covers the years 1789-2019 and 183 countries. After trimming countries not covered by either dataset I was left with 114 countries and 5 389 observations total, from 1960 to 2010.

The countries in the final sample are fairly evenly dispersed across the globe. There's a mix of regime types and levels of consolidation, dominant types of geographic features, economic development, etc, meaning that generalizations beyond the sample have merit. The time period is also favourable for generalizing. It stretches across 51 years from the near the

present to the waning years of colonialism without intruding on the immediate post WW2 period. The data includes several collapsed states providing additional breadth.

The unit of analysis is the country year. However, the country year versions of the OMG and SSF dataset are lacking in variation. I therefore recoded the country-movement-year dataset and country-force-year dataset respectively, and adding the values by country year. Unlike the SSF dataset, the M3 dataset does not include a country-force-year version but was still incorporated into the combined recoded country year dataset, which I elaborate on below.

Police militarization

All forces in the SSF dataset are under state control and their members carry weapons. The SSF dataset is the first choice for measuring police militarization. It is not the only choice, as security forces that are strictly military are ignored, a problem which I will return to shortly. Relevant security forces that this dataset includes are militarized police, paramilitary police forces, armed intelligence services, border guards, state run militias, and interior troops. Militarized police and paramilitary police forces are the main focus. Interior troops are paramilitary policing forces usually found in post Soviet states and will be included in the analysis. State run militias, border guards, and armed intelligence services will not be included. Though they are all engaged in policing, they do not serve the same function as the other types.

Introducing the M3 dataset poses three problems. First, the M3 dataset does not distinguish between domestic civilian military policing and wartime non-domestic military policing which is beyond the scope of this thesis. I account for possible wartime non-domestic military policing by confirming there is no larger, international, territorial occupation (or higher level conflict) by a country employing military policing, using the Correlates of War Militarized Interstates Dispute dataset. Second, the M3 dataset does not differentiate between different military forces, meaning that I cannot tell how many different military forces are active in policing, even if they are readily distinguishable. The M3 dataset only shows if the military as a whole is involved in civilian policing. Therefore, if the M3 dataset reports the armed forces as active in policing, I will count it as one observation. Third, the dataset also counts forces that operate under the ministry of defence of a given country, overlapping with

the SSF dataset. Thus I will exclude country years that the SSF reports as containing paramilitary forces. While the risk of under counting the number of forces is probably large, the risk of double counting them is zero. For this reason, and that the M3 dataset does not go back further than 1990, it increases the risk of type II error. The purported effect of dissent that police militarization represents will not appear as often. Scaling linearly, the number of military forces with civilian policing jurisdiction would have been 2.55 times higher over the full 51 years.

Police militarization is operationalized as the number of active policing forces active in a given state in a given year. While number of militarized police forces does not offer deeper insight into the phenomenon, it is still a measure of the expansion of militarized policing units within the state's coercive apparatus. Though there is no measure of size or equipment of the forces in a country year, police militarization will be reflected in the increasing number of militarized police forces, as well as an increasing number of types of forces. Both reflects an increase in militarization because they are a move towards the militarized end of the scale and an increase in killing capacity through military means.

Violent internal dissent

For inclusion in the OMG dataset, a campaign is defined as: "*one or more related and temporally contiguous events of coordinated mass collective action within a polity unit, aimed at least in part at altering or stabilizing/strengthening the current political regime, removing the head of state or government, or altering the territorial composition of the polity.*" (Dahl et. al., 2025). The smallest number of participants is 1000, but the count may reach as low as 500 if the campaign has at one point uncluded 1000 participants. Both pro government and anti government campaigns are present in the dataset, but campaigns that mobilize from within the government are not. Both violent and non violent campaigns are present, with violent campaigns being chararacterized as predominantly. Pro government campaigns are excluded from the analysis, as is non-violent campaigns. The shortest period of time a campaign can be active is one day. The OMG dataset is chosen above other similar datasets for its the non-maximalist demands of included campaignts which is a better fit for my definition.

Operationalizing violent internal dissent presents a problem. As has been argued extensively in the repression literature, there is a multidimensinality of threat in what level of

repressive response is enacted, meaning that one variable cannot capture the entire picture (Davenport, 1995). However, to keep the number of variables manageable I will restrict the analysis to one. The number of days of active violent campaigns in a year stands out for its breadth. Operationalizing internal dissent as the number of violent campaign days allows for capturing more aspects of violent dissent than other single variables. First, magnitude, as the OMG dataset restricts itself to atleast 1000 active participants per campaign, there is still significant threat in an active day of a violent campaign. Thus, counting number of days in some ways captures the number of participants, and number of events. Second, dispersion, Davenport (2007a) notes that country years are problematic because only small parts of a country could be involved in dissent. A larger number of days accounts for dissent events spread in time and space, and could additionally be seen as much more threatening even with a lower number of total participants. Since there sometimes are multiple campaigns active in a country year, there will also sometimes be multiple campaigns active in the same country day. This enhances both dispersion and magnitude, and decreases the distance between indicator and variable. Multiple simultaneous campaigns should increase threat perception. Capping the number of violent campaign days at 365 per year is an artificial limitation on the level of threat percieved and hides a truer relationship. Finally, because of data limitations, I will also be counting only the coordinated campaigns, meaning that a lower amount of participants may still be effective. The non coordinated alternatives indicators will act as controls.

Model

To prove the hypothesis I will run a regression of the number of police forces in a country year on the number of days of active violent campaigns in a year. Since the number of police forces in a year is a discrete variable in a specific timespan, a Poisson model will be used. A Poisson distribution shows the probability of a given number of events occurring within a period of time. Preliminary visual analysis shows that the police force in a year distribution resembles a Poissont distribution. Random subsamples of the police forces sample shows that the variance hovers around the mean, which is a requirement. For the full sample the difference is less than 2%. Every police force observation also needs to be independent of each other. This is difficult to assume, since the definition of police militarization entails a gradual escalation of coercive capacity, a decision which would presumably be based on what

there already is. Still, in the case where there is no militarized police forces in a country year, this is fulfilled.

As mentioned in the literature review, the repression literature is in fact the repression-dissent literature. A central pillar of this literature is the repression-dissent nexus, e.g Pierskalla (2010). In short, the repression-dissent nexus deals with the back-and-forth effects of repression and dissent, meaning that cause and effect isn't always clear. One of these unclarities is naturally the time aspect. To ensure that violent dissent precedes police militarization, the number of days of violent campaigns in a year will be lagged by one year.

Stavro & Welch (2024), using the SSF dataset, shows that the number of countries with militarized police forces varies over time. Moreover, country years are not a perfect measure. Davenport (2007a) notes, the speed of response is an empirical measure and can differ between countries and time periods. Additionally, as per the discussion about regime types and the effects of democracy, among other factors, I expect to see differences in police militarization that vary with the country. To account for such effects, the model will include country and year fixed effects.

Controls

As the repression literature has repeatedly shown, the level of violent dissent not the only important variable affecting the state's response. Most important is the presence of political democracy. Like the law of coercive responsiveness, the finding that democratic institutions so consistently decreases repression across space, time, and repression types has caused it to also received a name, the domestic democratic peace (Davenport, 2007b). The relationship is not a linear one though, as democracy below a certain level does not decrease repression (De Mesquita et. al., 2005). Likewise, which exact aspect or type of democracy is responsible is debated. For example, Hill Jr. & Jones (2014) concludes that competitiveness of participation is the more important, while De Mesquita et. al. (2005) finds that its executive contstraints. So to control for the slightly unclear but robust effect I will use the Varieties of Democracy electoral index to get at the foundations of political democracy.

Economic correlations are also a common find. The most prominent economic factor is the negative effect of GNI (GNP) per capita on repression (Poe & Tate, 1994). Yet there is controversy as to the mechanism or even the meaning of this finding. However, the fact that it

is GNI that correlates and not GDP may provide a clue, as Hafner-Burton (2005) shows that the most convincing evidence relating to international economic effects is for net FDI inflow. To control for this effect, I will include a control for both GNI per capita from the World Bank World Development Indicator database and FDI from the IMF Balance of Payments database.

Coordinated leadership could prove influential, as a coordinated leadership may be much more effective and therefore threatening, as opposed to relying on the campaignt participants to organize themselves. Hence, leadership is an aspect of tactics, or the efficacy of violent dissent. An organization that has a leader also has invested power in them, or that leader already has power and is hence seen as a competent candidate for leadership. Leadership thus provides a way to measure dissenting elite participation which combined with popular unrest is the two main internal sources of dissent. Finally, an obvious obfuscating factor for how the state perceives violent dissent is the magnitude of the non-violent dissent. Not accounting for this may hide a truer relationship, that it's not violent dissent but dissent generally that contributes to police militarization. The OMG dataset provides the data for controlling for this aswell.

Results

Descriptive statistics

Table 1, 3, and 4 shows summary statistics. Table 2 shows global averages for select variables for the same time period. Table 1 is for all observations. Table 3 shows country years where there is no coordinated violent campaign days. Table 4 shows country years where there are only coordinated violent campaign days. Figures 1 and 2 are the number of militarized police forces and the number of violent campaign day distributions, respectively.

Variable	Obs	Mean	Std. dev.	Min	Max	Median
Number of militarized police forces per country year	5,389	1.201707	1.08883	0	8	1
Number of coordinated violent campaign days per country year	5,389	55.48214	165.3449	0	1464	0
Number of non-violent campaign days per country year	5,389	25.32492	131.8017	0	2848	0
Number of non-coordinated violent campaign days per country year	5,389	44.32474	96.95011	0	2562	0
V-dem electoral index	5,388	.3732244	.2821944	.007	.91	0.253
GNI per capita, PPP $US	5,242	506.9947	403.6095	1	1247	477
FDI Net Inflow, $US	5,242	1350.581	1069.634	1	3491	1083.5

Table 1.

Variable	Obs	Mean	Std. dev.	Min	Max	Median
V-dem electoral index	8,401	5	.2860985	0.007	0.923	0.283
GNI per capita, PPP $US	11,067	917.3978	898.0293	1	2738	728
FDI Net Inflow, $US	11,067	1955.832	1750.885	1	5860	919

Table 2.

Starting with table 1 observation count, the GNI per capita and FDI data lacked some of the historical countries, which is why their observation count is slightly lower. This is the same for every table. For militarized police forces count, the mean and variance is near equal as mentioned in the data section. The maximum is quite high, but will be elaborated on shortly. The mean number of both coordinated and uncoordinated violent days is surprisingly high, but the median likely tells a truer story. As for the actual day count, all three day variables show a max much higher than 365. The cause is the multiple simultaneous campaigns per year. Comparatively, the V-dem electoral index sample values are very close to the global values for the same time period, showing that the level of democracy in the sample average is about the same as global levels. GNI per capita sample standard deviation and max is roughly half of global values which makes sense as the sample is half the size and evenly distributed. The mean and median reveals that the sample countries are poorer on average. The FDI net inflow sample values differs from global values varies more. The median is roughly the same. The mean is 1.5 times the size, and the max and standard deviation is about 1.75 the size. The sample size is likely the cause again. However, the mean and median change in different directions, increasing the median by 17.5%. I reason that this is because the sample includes some countries that are less developed, but promising enough to invest in.

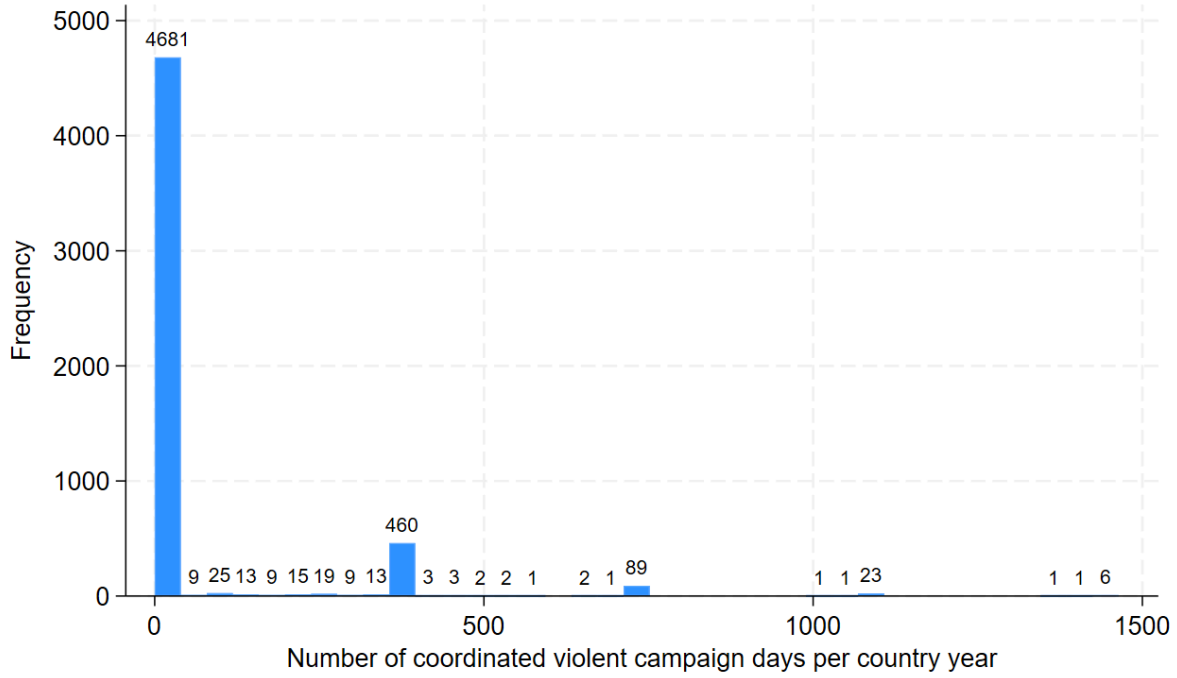

Figure 1. The bars of figure 1 represent the number value up to but not including the next value.

Variable	Obs	Mean	Std. dev.	Min	Max	Median
Number of militarized police forces per country year	4660	1.209442	1.093561	0	8	1
Number of coordinated violent campaign days per country year	4660	0	0	0	0	0
Number of non-violent campaign days per country year	4660	19.77296	115.8549	0	2848	0
Number of non-coordinated violent campaign days per country year	4660	42.52468	193.3029	0	2562	0
V-dem electoral index	4660	0.385968	0.289695	0.007	0.91	0.263
GNI per capita, PPP $US	4516	503.4178	400.9969	1	1246	476.5
FDI Net Inflow, $US	4516	1366.16	1072.768	1	3491	1108.5

Table 3.

Variable	Obs	Mean	Std. dev.	Min	Max	Median
Number of militarized police forces per country year	729	1.152263	1.057482	0	5	1
Number of coordinated violent campaign days per country year	729	412.8477	233.9858	5	1464	365
Number of non-violent campaign days per country year	729	60.81481	203.0065	0	1702	0
Number of non-coordinated violent campaign days per country year	729	55.83128	166.476	0	732	0
V-dem electoral index	728	0.2849753	0.2079231	0.007	0.828	0.212
GNI per capita, PPP $US	726	529.2438	419.0954	1	1247	939
FDI Net Inflow, $US	726	1253.669	1045.445	1	3490	919

Table 4.

Table 3 and 4 are included to show the differences between the samples containing violent days and those without. Figure 1 shows that 86% of country years had no violent coordinated campaign activity counted in days, hence analysing differences between the subsamples being important. For the number of militarized police forces in table 4, the

difference between mean and variance is 3%. Compared to the full sample this is a 5% percentage point difference. This indicates that the Poisson model is still valid. The max number drops by three, but is not very interesting as per the Poisson distribution. The table 4 mean number of coordinated violent days jumps by 800% compared to table 1, showcasing the skew. The number of non-violent campaign days of table 4 versus table 3 is three times higher, meaning that country years with many violent campaign days also has many non-violent campaign days, indicating a rise in overall dissent. Finally, the V-Dem electoral index falls by 26% between table 3 and table 4. In combination with the increase in the dissent indicators, this suggests that a decrease in democratic quality is the source of the difference in dissent that is captured by the subsamples. Also, that the mean number of militarized police forces falls in falls in table 4 versus table 3 bodes ill for the hypothesis.

Continuing with the skew of the coordinated violent campaign days count, a couple of things stand out about figure 1. Apart from the no violent days, observations are clustered around multiples of 365, showing that by far the most common duration of a campaign is more than a full year. Moreover, there is rarely more than one active in a year. The size of the intermediate bars reinforce this, as they represent years when a campaign comes to a close. The intermediate bars after the first full year reflects campaigns with one full year of violent campaign days, and one campaign that comes to a close. The 730 days bar represents countries with atleast two simultaneous full year campaigns. This, and the decreasing size of the intermediate bars reinforces the implication that there very rarely are more than two active campaigns. This is repeated twice more.

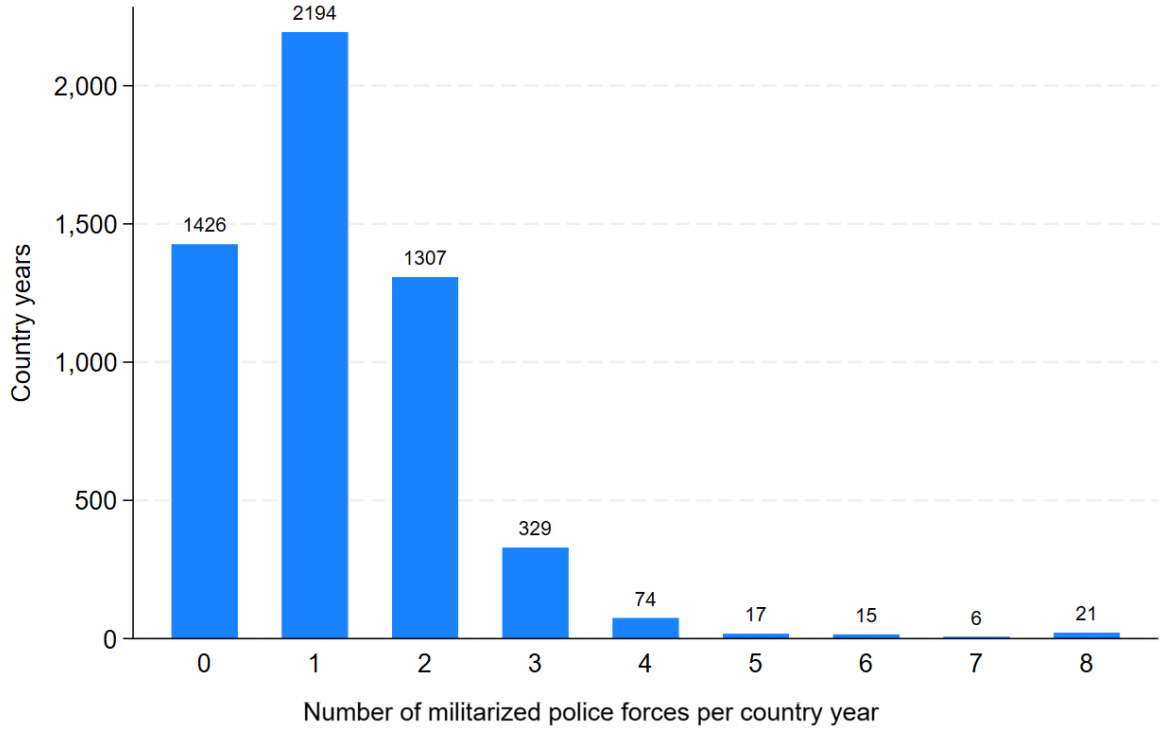

Figure 2.

Before continuing to the regression analysis there are a few comments to be made about figure 2. As promised, it very much resembles a Poisson distribution with a mean rate of about 1, and as table 1 shows that is the case here aswell. There is one deviation though, the growing tail end, which is not a feature of a Poisson distribution. The observations in the last bar, showing 21 country years with 8 active militarized police forces comes exlusively from India. Extreme cases aren't rare but it's worth mentioning in this case because it very clearly affects the results of the following regression. Had India been a smaller country, or had it not been a single one, it might not have mattered. This time the extreme case is also based in an extreme country, and is the cause of a difference between a significant result and an insignificant one.

Regression analysis

Variable	Incidence Rate Ratio		
	With India	Without India	Without Eco
Number of violent coordinated campaign days per country year	1.0001761	1.000182	1.0002050
	(0.000122)	(0.0001224)	(0.0001214)
Number of non-violent campaign days per country year	1.0000643	1.0000598	1.000073
	(0.0000942)	(0.0000943)	(0.0000929)
Number of violent non-coordinated campaign days per country year	1.000168*	1.0002492	1.000212**
	(0.0000656)	(0.0001367)	(0.0000647)
V-dem electoral index	1.176527	1.177924	1.303839**
	(0.089355)	(0.0992899)	(0.1032095)
GNI per capita, PPP $US	1.0001399**	1.0001424**	
	(0.000359)	(0.000366)	
FDI Net Inflow, $US	1.0000309*	1.0000318*	
	(0.0000129)	(0.0000132)	
Number of observations	5034	4984	5175

*** p<0.001 ** p<.01, * p<.05

Table 5.

Table 5 shows the main results of regressing the number of militarized police forces on the number of violent campaign days, as well as the controls. As it is a Poisson regression model, the actual effect is the exponentiated coefficients, also known as incidence rate ratios. As a reminder, independent variables are lagged by one year. The effects sizes may seem small, but the variables capture occurrences in the thousands.

Unfortunately there is no significant correlation found for the main variables, which disproves the hypothesis. Continuing with the significant results of the full model, and following up on the figure 2 discussion, we find that the number of violent non-coordinated campaign days is significant at the 5% level. However, this is entirely due to the extreme case of India. There were no days with another combination of dissent attributes for the 21 year period, indicating a very unusual country year period. Looking at the next colum over, we see that without the influence of India, the effect is much smaller. The effect size again increases in the model without economic controls, but this is again including India. Excluding India and economic variables again causes the uncoordinated violent campaign days to be non significant, though it is not included in the table.

The effect of the V-dem electoral democracy indicator is unexpectedly not significant despite the repression literature findings. However, the significant effect of GNI per capita and FDI net inflows provides a clue. The correlation between democracy and economic development is well documented. As we see in the third column, removing the economic

variables coincides with the democracy indicator becoming significant at the 0.1% level. The implication that it's not democracy that is relevant for police militarization, but economic development. Concerning effect size, there is on average a 0.14% (0.00014) chance in seeing an additional militarized police force for every additional purchasing power adjusted, per capita GNI US dollars in the following year, all else equal. Based on the mean and median values, that is equal to about half a militarized police force per country year on average. For FDI the effect is smaller. An increase in the net inflow FDI measured in US dollars would be associated with an average 0.031% change of seeing another militarized police force in the following year all else equal. Based on the median and mean values, the average of the sample countries should be about a third of a police force. I reasoned earlier that FDI was the dominant aspect of GNI, but this should dispel that idea, as both effects are significant.

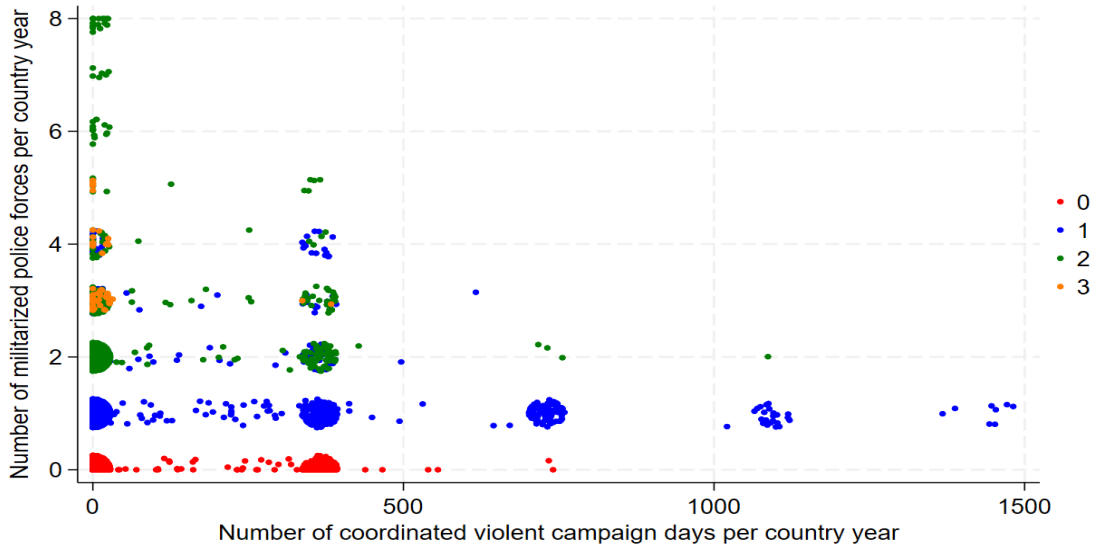

Figure 3.

Figure 3 shows the scatterplot of the number of militarized police forces per country plotted against the number of coordinated violent campaign days per country year. Each datapoint is randomly offset by a small amount to improve readability. Datapoints are distinguished by colour, indicating the number of different types of militarized police forces in a country year. I would have liked to fit the results of the regression but I was unable to. The scatterplot reveals the logarithmic nature of the trend, and the coefficient table shows that the correlation was not large enough to be significant, so there is not much lost by not

including the trend. As the figure shows, for each value of coordinated violent campaign days there are several corresponding values of the number of police militarization, indicating that there is no clear relationship. However, this decreases more and more as the number of days does, indicating a logarithmic trend. This is centered around the single militarize police force per year line, which could explain the non-significant results. It is worth remembering the distribution of the number of days and that it's heavily skewed towards no days of violent campaign, the most likely factor of the significance results.

The purpose of the colouring is for spotting trends in the number of different types of police forces employed in a country year. If this number increases with the number of coordinated violent day, it would provide evidene that police militarization is in response to violent dissent. Figure 3 shows that this is not the case. The number of different types is more closely associated with the number of forces, with a few outliers. Additionally, the country years with all three types of militarized police forces are almost all in within the zero violent days group. This is not too surprising given the distribution of the days, and the smaller number of country years data on military forces with policing jurisdiction.

Robustness tests

The original intention was to include all relevant dissent variables in the model as discussed with the violent internal dissent operationalization. They will instead be implemented as robustness tests. Replacing the number of violent coordinated campaign days with the lower bound of estimated number of violent coordinated participants shows some improvement in effect, but not enough to be significant. The effect of democracy flips to significant at the 5% level while the economic variables remain the same, showing democracy may still matter despite earlier findings. Substituting in the number of violent campaigns per country year yields much the same. Combining all three variables and their controls has the same effect once again. More importantly, it increases their coefficients substantially, but alongside the standard errors, with no significance reported. The interaction variable of all main dissent variables again fail to find significant results. Finally, using different lags matters little. No lag increases the estimated coefficient of the number of violent coordinated campaign days, but not enough to be significant. Every year of lag decreases the estimated size of the coefficient.

Only GNI per capita remains significant with a lag of three years, while with no lag, electoral index flips to significant. In conclusion, the results are robust.

Replacing the number of militarized police forces with the number of different types of militarized police forces once again shows similar results. However, this time the V-dem electoral index shows significance at the 1% level. Tests with different levels of democracy produces some interesting results. A V-dem electoral index of 0.8 or higher leaves only GNI per capita significant, and massively inflating the standard error of both coordinated and uncoordinated violent campaign days. It is also one of the few regressions that produces causes the number of violent days estimate to be smaller than one, showing a decreasing effect. Between a democracy index of 0.8 and 0.2 the results are similar to the main model. Below 0.2 they are very peculiar, with an estimated IRR of 11.5 and a standard error of 7.9, and the only significant effect. Below 0.5 only the two economic variables are significant, and above 0.5 only democracy is, with a 2.7 IRR.

Discussion

Based on the result I can fairly confidently say that the hypothesis is disproven. Considering the distribution of violent days, the results are not too surprising. The distributions of the other two main violent dissent variables are of course very similar. That the wide array of robustness tests in addition to the main and arguably strongest measure of violent dissent both failed to produce significant results is conclusive. The operationalization of police militarization could be to blame as it is focused on national police forces only, subject to large variation in structure, without much in the way of data regarding size or actual military equipment. As discussed, the M3 dataset is limited to 1990-2010. However, it is one of many operationalizations that has successfully found similar effects on repression, implying that it is a valid measure. There could yet be something to the theory, since Carey (2010) connects violent dissent to repression, and Stavro & Welch (2024) connects police militarization to repression. Therefore, police militarization should be considered an expansion of coercive capacity, though likely not for the reasons I hypothesized.

Still, the attempt to connect police militarization to wider theories of coercive capacity has not entirely failed. The effect of democracy and economic development has proved thoroughly consistent, mostly like the literature suggested it would. The direction of that

effect is the surprising part. There is some robustness aswell, when changing the number of forces to the number of different types of forces. The size of the effects are also thoroughly confusing when testing for different levels of democracy. Since we know that democracy and economic development decreases repression, and that police militarization increases repression, it does not make much easy sense that democracy and economic development also increases police militarization. I see two possible explanations. The first is based on Greitens' (2013) conclusions. The popular unrest that police militarization would be in response to could be more common in democracies relative to the needs to balance the influence of competing elites in autocracies. Alternatively, in regards to the economic significances of the regressions, it's richer countries with more equal wealth distribution, mirroring the power balance of the regime type with economic means instead. The second possible explanation is that there is a middle ground where the level of democracy does not matter. This is mirrored in the repression literature. For instance, Davenport & Armstrong (2004) finds that democracy only matters above a certain level, below which there is no discernable effect of regime type on repression. Fein (1995) finds that this holds for democracies and autocracies both, which would explain the regressions using different levels of the V-dem electoral index.

My prime worry about drawing conclusions based on this work is that it's scale may be too broad. In data terms the choice was an absolutely necessary one. Still, the results and the discussion on democracy and economic development shows that there may be significant variation between states. The average effect of the correlations may be just that, an average, and not applicable outside a few select countries in a specific time. On the other hand, the comparison between the global and sample averages suggest otherwise. Nevertheless, what this thesis has shown is that there circumstances are not as clear as they seem, however it is examined. The intuitive simpleness of the hypothesis being disproven and the controversial effect of democracy and economic development indicate many interesting directions for further research. Considering the results of the below 0.2 electoral index score countries, there is likely considerable complexity to be discovered. If it's not violent dissent that police militarization is in response to, then what is, and what is the purpose of police militarization? My second worry is in regards to methodology. A more experienced researcher will likely have found many faults, and been able to remedy them. For example, I was unable to fit the estimated regression model onto figure 3, possibly missing information that would affect the

conclusion. Other types of statistical models may have shown less robust results were I aware or capable of implementing them, again altering the conclusions. Lastly, since I have not tested the proposed mechanism of threat it's possible that the hypothesis suffers from confounding omitted variables.

Conclusion

In this thesis I have examined whether police militarization increases in response to violent dissent. I fail to find a connection and the results are robust. With some certainty I can conclude that the hypothesis is false. Though caution always warranted when dealing with violent state actors, in this case there is slightly less reason to be pessimistic. While it's very likely that public opposition to the political order is met with a crackdown, expansion of that ability does not seem to result, atleast not in the form of militarizing the police. Furthermore, I have made interesting discoveries which are antagonistic to the established expectations of the relevant literature. The effects of democracy and economic development on the ability to repress at larger scales seem to be the opposite of the effects on repression itself. I can see two main implications. Either police militarization is not a problem and the literature has problems of omitted variables, such as selection bias towards more violent states. Alternatively, democracies and economically developed states are more effective at repression, through greater coercive capacity, preempting any need for repression. Whatever the case, that is for another thesis, and the door for it has been opened.

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