

From Broken Windows to Broken Bonds: Militarized Police and Social Fragmentation*

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Abstract

Expansion of police militarization in the U.S. raises questions about how such policing affects society and minority communities. We estimate the impact of police militarization on civic engagement—which we capture primarily by examining charitable giving among households—via an instrumental variables approach. The instrument stems from plausibly exogenous variation in federal defense spending, which affects awareness of military culture and capabilities, and thus encourages the adoption of military equipment and tactics by local police departments. Estimates show that police militarization has a fragmenting effect on society: As police militarization increases, black households reduce their total charitable donations more than all other households.

JEL classification: D10; D64; H31; H57; H73; J15

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1 Introduction

In August of 2014, as images of well-armed police in camouflage uniforms patrolling the streets of Ferguson, Missouri were broadcast nationally, many Americans were first introduced to an American militarized police force, a development that did not spring up overnight, but rather has been evolving over the last few decades. As the New York Times wrote at the time, “Police militarization is a growing national threat. If the federal government doesn’t act to stop it, the future of law enforcement everywhere will look a lot like Ferguson.” (Beavers and Shank (2014))

Since then a steady national debate has emerged about the proper role of policing in communities, and the blurring of lines between military and police functions. The debate has mostly proceeded without careful evidence regarding the impact of this foundational change in policing. In this paper, we consider the effect of increasing police militarization on aspects of civic participation beyond issues of law and order, and study how these impacts may differ across race.

The fact that people can be generous and give to others with minimal apparent direct reward is well-attested in everyday life, but our understanding of what influences the decision to give is still limited. The decision to give sits more uneasily for economists, who tend to view human interactions through the lens of individual optimization. This has led economists to study the choice of giving in a number of ways, including theoretical explorations of mechanisms, laboratory experiments, observational empirical analysis, and most recently, field experiments. While progress into our understanding of the determinants of charity has been steady, empirical studies using large representative samples with a focus on causal identification have been scarce.

In this paper, we draw on a nationally representative panel dataset to study the impact of increased police militarization on charitable giving, and we utilize an instrumental variables approach based on institutional features of the federal program that has facilitated much of the expansion over the last two decades. Our interest is understanding the roles of social distance and social cohesion in the individual choice to give to charity, and how this differs across socio-economic categories, outside the context of artificial environments such as laboratory experiments or even small scale “field” experiments.

We find that enhanced police militarization has a socially fragmenting effect. Black households reduce charitable giving in response to increased police militarization, particularly charity to organizations that support the needy and educational organizations. We estimate that each additional million dollars of military equipment transferred to local police results in a 7.4% decline in overall charitable giving for black households. While we find that non-black households also reduce overall contributions, the effect is far smaller (about 1.4%) and less robust. Across a wide range of charitable activity, we find consistent evidence of a fragmenting effect: the relative impact of police militarization is consistently worse for black households compared to non-black households. We posit that enhanced police militarization increases social fracture by increasing social distance for black households, resulting in less social engagement.

Our interest is in the act of giving, and how giving responds to individual experience of social distance. The concept of social distance has been introduced into the economics literature through the theoretical approach of [Akerlof \(1997\)](#), who notes that conventional economic decisions and social decisions differ in that social decisions have social consequences whereas economic decisions do not. The choice to invest in education, raise children, and donate to charity will have impacts on self-conception and how agents associate with family and friends, and indeed with potentially who might be friends. Social choices are thus characterized by how they influence relationships and relational status.

This idea of social distance and social decisions has been studied in a number of laboratory environments. [Hoffman et al. \(1996\)](#) studied the determinants of social distance within the boundaries of the Dictator Game, and they found that as subjects' belief in the degree of reciprocity between agents decreased, that is, as perceived social distance increased, subjects were more willing to keep a larger portion of the pie controlled by the dictator. As social distance increased and the subjects felt more isolated, they reduced their charitable contribution to the other party.

[Bohnet and Frey \(1999\)](#) provide an alternative (but similar) definition of social distance within the context of the Dictator Game. Whereas [Hoffman et al. \(1996\)](#) emphasize norms of reciprocity in their conception of social distance, [Bohnet and Frey \(1999\)](#) focus on "other-regardedness inde-

pendent of social norm.” They conceive social distance as identity awareness in the sense that as the “other” moves from an unknown among an anonymous crowd to an “identifiable victim,” social distance has declined. This identity-based articulation has similar properties to reciprocity-based social distance, and for the present purposes, either conception captures our investigation into how individuals respond to a more militarized police force in terms of social engagement.

Militarized police forces differ from more traditional police forces in that they tend to embody more capital-intensive equipment with a focus on power projection. Implicit in militarized police forces, in terms of both equipment and tactics, is aggression, dominance, and threat of punishment. As noted by Kraska and Kappeler, militarism is “defined as a set of beliefs and values that stress the use of force and domination as appropriate means to solve problems and gain political power, while glorifying the tools to accomplish this - military power, hardware, and technology.” (Kraska and Kappeler (1997))

Combining enhanced police militarization with police tactics focusing on aggressive and pre-emptive policing, commonly referred to as the broken windows theory of policing (Wilson and Kelling (1982)), has the potential to escalate interactions between citizens and police. Rather than support order and lawfulness, as the theory predicted, there is the potential to undermine social trust and cohesion.

This mode of law enforcement differs from more community-oriented approaches that emphasize local knowledge and the cultivation of local relationships to prevent crimes from happening, and to leverage community knowledge after a crime has been committed to capture the offender (see Corder (2014)). One concern in adopting military tactics and equipment is therefore creating more social distance between the personification of the law and the citizens they are sworn to protect. The military equipment and uniforms adopted by local police forces can create a strong sense of anonymity among police officers and distrust among communities, creating a clear wedge between police officers and citizens. Such a concern was summed up by Dr. Tom Nolan, a criminal justice scholar and long-time member of the Boston Police Department: “Police in the United States are militarizing, and in many communities, particularly those of color, the message is being

received loud and clear: 'You are the enemy'." (Nolan (2014)).

Historically in the United States, the level of trust and confidence in police forces have varied across communities. This is particularly true when comparing self-reported attitudes of black and white citizens. A Pew Research Poll conducted in 1995 found that only 49% of black respondents had "a great deal" or "a fair amount" of confidence in local police to enforce the law, compared to 78% of white respondents (Kohut et al. (2007)). Twelve years later in 2007, when the same survey was given, the gap between white and black respondents was nearly unchanged (78% compared to 55%). Furthermore, only 38% of black respondents agreed "a great deal" or "a fair amount" that local police could be trusted not to use excessive force, compared to 73% of white respondents. Given the underlying differences in trust and social distance between communities and the police, the introduction of a more militarized police with an emphasis on power projection may exacerbate and amplify societal fractures, leading to differential responses in civic participation.

On the other hand, a more capital-intensive militarized police force may have greater policing capacity and may therefore be a more productive police force, particularly by deterring criminal offenses through the threat of punishment. If citizens feel protected and secure to begin with and have a social affinity for law enforcement agencies, then an enhanced militarized police force that reduces crime may have a net positive effect on social distance by increasing a sense of security, thereby improving the environment in which social decisions occur.

We explore these possible trade-offs using a representative sample of households in the U.S. over time. One of the major conduits for enhanced police militarization in the United States over the last two decades has been through the Department of Defense (DoD) Excess Property Program 1033. This program has transferred nearly 6 billion dollars of surplus equipment from the DoD to law enforcement agencies. The 1033 program was initially introduced in 1997, and is still in operation at present, although the types of equipment available for transfer was limited by Presidential Executive Action in May 2015. Given campaign promises of the current administration to undo these restrictions, it is reasonable to expect the 1033 Program to continue to equip local law enforcement agencies with military equipment for the foreseeable future. For reasons of trans-

parency and accountability, all transfers during this time period were recorded and cataloged. From the roster of transferred equipment through the 1033 program, we construct measures of enhanced police militarization using list price values of equipment.

Participation in the 1033 Program is a choice and is likely subject to selection bias in that law enforcement agencies participating in the program may be different in both observable and unobservable ways from law enforcement agencies that do not participate (or participate less). To deal with concerns that selection bias is contaminating the estimated impact of police militarization on charitable giving, we instrument for the degree of participation in the 1033 program using state exposure to militarization through federal spending. Federal military spending differs across states and over time, and is driven by international military objectives and historical political economy motivations, both of which are unlikely to be directly related to current household charitable giving decisions. Furthermore, Congress sets federal military spending often years in advance making it unresponsive to current charitable giving household decisions.

However, participation in the 1033 program is influenced by exposure to military culture, language, personnel, and equipment. This exposure increases awareness of and interest in the 1033 program, and increases knowledge of specific available military equipment. As such, federal military spending at the state level is a viable instrument for militarization of law enforcement agencies through participation in the 1033 program.

Our charitable giving data come from the PSID module on charity, which surveys over 10,000 households every two years from 2001 to 2013. This gives us over 50,000 observations covering a nationally representative sample of U.S. citizens, and includes individual and family-level information that may be relevant for determining charitable giving. The typical approach to studying the decision to give to a charity is through laboratory games or large scale observational studies. The former is plagued by concerns of external validity, while the latter is hampered by the lack of identification. Recently, researchers have taken experimental approaches to the real world through small-scale field experiments. Our approach is particularly noteworthy in that we utilize a large representative panel of American households, and combine with this a clear identification

strategy using instrumental variables to study the effect of enhanced police militarization on social decisions, in particular, the decision to donate to charity.

To test that our identification is well-founded, we conduct a series of placebo and validity tests. When we look at outcome variables that should be not be impacted by police militarization such as monthly car payment or Christian identity, we find no effect. For validity tests, we consider a range of definitions of police militarization, all of which yield findings that are qualitatively consistent with our primary fragmentation results. Finally, we rule out unintended political economy mechanisms proxied by earmark spending, which may be correlated with our federal defense spending instrument. When we exclude years where congressional earmarks were eliminated, we find effects similar to those estimated on the entire sample, suggesting our identification claim based on awareness of the 1033 program is valid.

The rest of the paper proceeds as follows. Section 2 reviews the relevant literature related to social distance, determinants of charity, and police militarization. Section 3 discusses the data, while Section 4 develops the identification strategy. Results are presented in Section 5. Section 6 concludes.

2 Related Literature

2.1 Police Militarization

Although the 1033 program has proved controversial and has generated significant media attention, its impacts have not been extensively studied. Typical reporting on the 1033 program focuses on descriptive analysis of the types of equipment transferred or characteristics of the police forces requesting equipment. Additionally, there have been some attempts to understand the historical development of police militarization in general and more specifically the legal and institutional evolution of the 1033 program. These attempts have been primarily anecdotal in nature. The most extensive treatment can be found in [Balko \(2013\)](#). Our approach differs in that we consider the universe of transfers through the 1033 program rather than sensational anecdotes. The 1033 Pro-

gram has transferred nearly 6 billion dollars in equipment and resources during its existence. Our approach provides a better understanding of the impact of the entire program to date.

In related work, [Haynes and McQuoid \(2016\)](#) evaluate the impact of the 1033 program on state-level crime rates, and find contra the conventional wisdom, the 1033 program has significantly reduced crime rates, especially violent crime rates. Furthermore, they find that empirically accounting for selection into the 1033 program is essential as military equipment transfers are not approximately randomly assigned across police departments. This underscores that the types of law enforcement agencies participating in the 1033 program are different from those that participate less or do not participate at all. [Masera \(2016\)](#) finds similar evidence that police militarization reduces crime, and furthermore, police militarization increases police killings. [Ajilore \(2016\)](#) provides further evidence that police forces participating in the 1033 program differ in important ways that may matter for civic engagement.

[Harris et al. \(2017\)](#) look at the impact of the 1033 program on local interactions between police officers and citizens, and find that the program reduced the number of citizen complaints, assaults on police officers, and increased drug arrests. We expand the purview of social consequences of police militarization by considering the impacts on the broader community through measures of civic engagement.

To properly deal with the selection bias concern, we employ an instrumental variables strategy that relies on the politics of military spending at the federal level. Military spending is highly political, as documented by [Mintz \(2002\)](#), and we draw on these institutional features to drive our identification strategy. National military spending is driven by geopolitical events rather than local social dynamics related to civic participation. This spending however differs significantly across localities, and this spatial variability is related to historical circumstances that are unlikely to respond to current changes in civic participation (see [Braddon \(1995\)](#)). The nature of congressional pork changed during our sample, and we use the timing of this change to test the robustness of our results below. Taken together, this buttresses our confidence in the validity of the exclusion restriction. Since federal military spending is highly correlated with intensity of participation in the

1033 program, as we document below, our approach effectively deals with selection bias concerns.

Our identification strategy is reminiscent of [Nakamura and Steinsson \(2014\)](#), who use the differential impact of military procurement at the state level to identify fiscal multipliers, and [Hooker and Knetter \(1997\)](#) who study the impact of state level military spending on broader measures of economic activity. [Barro and Redlick \(2011\)](#) use military spending changes related to wars to identify the effects of government spending and taxes on output fluctuations. In a cross-country context, [Creasey et al. \(2012\)](#) use variation in military foreign aid to study nation building and growth.

The relationship between race and policing is complicated, as a recent paper by [Fryer \(2016\)](#) makes clear. Using highly disaggregated data, Fryer looks at racial differences in police use of force across ten U.S. cities, and finds blacks and Hispanics are more than fifty percent more likely to experience some form of force in interactions with police. For the most extreme use of force, officer-involved shootings, there were no apparent racial differences conditional on being stopped by the police. Our paper seeks to understand how a more aggressive militarized police force may impact an individual sense of social distance, how this may differ across races, and what it might imply for civic engagement. The fact that blacks are more likely to experience force from interactions with police make the experience of a more militarized police force different when compared to a typical white experience with the police. It is this asymmetric experience that can lead to greater social distance for black households and greater social integration for white households, yielding very different effects in terms of civic participation.

2.2 Charitable Giving

The determinants of charitable giving have been extensively studied in economics (for a more complete overview of the charitable giving, see [List \(2011\)](#) or [Andreoni and Payne \(2013\)](#)). One key source of evidence regarding influences on the decision to give comes from experimental evidence in a laboratory environment, such as [Hoffman et al. \(1996\)](#) and [Bohnet and Frey \(1999\)](#). More recently, [Andreoni and Bernheim \(2009\)](#) reconsider the idea of norms and fairness within

the context of an experimental dictator game, but their approach leverages a theoretical structure to interpret the results. They find that social perception rather than fairness influences the decision to contribute.

A typical criticism of experimental approaches is that they lack external validity. The environment of laboratory games often fails to replicate realistic situations, which calls into question the applicability of the results. [Charness and Gneezy \(2008\)](#) worry about the true anonymity of laboratory participants, and suggest that even social interactions that might appear anonymous often include social clues and cues about the characteristics of others. In their experimental approach, they alter the degree of anonymity by providing additional information in the form of social cues such as the last name of the participant. They find that even small cues like last name can have a significant effect on giving in the Dictator Game, consistent with the view that social distance and degrees of anonymity matter for social choice. One of the notable aspects of militarized police is the power of the uniform in creating anonymity, which is often coupled with a refusal to name individual police officers.

To try to better bridge the gap between laboratory and real world, recent studies have focused on small scale field experiments. These settings combine the experimental control of the lab with real world scenarios. For example, [DellaVigna et al. \(2012\)](#) study the effect of social pressure on charitable giving via a door-to-door fund raising drive where households were made aware of the time of asking, and thus given the option to avoid interacting with the charity. They find that social pressure plays a role in giving, implying that greater social cohesiveness and integration should lead to more charitable giving. As society fractures, people give less not only because they feel less attached to the community, but because the power of social pressure to give is reduced.¹

One drawback of both lab and field experiments is that they tend to focus on narrow groups and situations in the name of causal identification. An alternative approach is to focus on larger,

¹The seeming contradiction between the apparent joy people receive from charitable giving and strategic behavior to avoid situations where they are asked to give is taken up in [Andreoni et al. \(2011\)](#). Based on the results of their field experiment, they argue for a sophisticated understanding and awareness of the link between empathy and altruism, so that individuals know that they respond to feelings of empathy with desires to give, and knowing this, they will avoid environments in which they feel their empathy might be manipulated.

more representative data on charity and charitable giving, although often without a clear identification strategy. An exception is [Hungerman \(2009\)](#), who looks at differences in giving across diverse communities, and considers both a preference effect (less altruistic preference for public goods sharing with more diverse communities) and a “cooling off” effect (greater social fragmentation lowers the warm glow individuals get from volunteering). Using an unexpected shock—a Supreme Court ruling that expanded government supplemental security income (SSI) in 1991—he finds that while more diverse communities exhibit lower charitable spending overall, they also experience less crowd-out from government intervention. Crowd-out in response to public spending was concentrated almost entirely in racially homogenous communities. We similarly focus on the interaction of government policy and charitable giving across racial differences, but in our case government policy exacerbates underlying social fragmentation. [Andreoni et al. \(2016\)](#) look at ethnic and religious diversity in Canada using tax record data, and find that diversity has a detrimental effect on charitable donations. The estimated effect comes through the intensive margin of reducing the amount of giving as they find no effect on the fractions of households that donate.

To better understand differences across racial groups, and how determinants within racial groups may differ for charitable giving, [Fong and Luttmer \(2009\)](#) use an experimental setting where the recipient was impacted by forces beyond their control, namely Hurricane Katrina. They find that while the average amount given does not depend upon the race of the person receiving the charity, this masks important aspects of empathy and charitable giving. Respondents who expressed feeling closer to their racial or ethnic group gave substantially more when the recipient was of the same race, while those who did not report a feeling of closeness gave significantly less. In a related paper, [Fong and Luttmer \(2011\)](#) use an experimental setting to manipulate the perceived worthiness and race of the charity recipient. They decompose the overall charitable giving effect into a component based on worthiness and a component based on race. They find that higher perceived worthiness does increase giving, while racial differences themselves have no effect on giving. However, there was a racial bias in perceived worthiness as white respondents reported black recipients as less worthy on average, leading to lower charitable giving overall when the

recipient was black.

The data used in our paper draw on a publicly available and nationally representative panel survey dataset. The U.S. Panel Study of Income Dynamics (PSID) introduced a charity module in the 2001 survey wave, collected by the Center on Philanthropy Panel Study (COPPS). [Wilhelm \(2006\)](#) compares the PSID charity data to restricted charity data using IRS tax data, and finds that the new data are high quality. [Wilhelm \(2007\)](#) provides further analysis and discussion of the quality and comparability of the data, and concludes the PSID charity dataset is a good substitute for restricted tax data.

The PSID charity data have been regularly used to study charitable giving of individuals and households over time. [Yörük \(2013\)](#) looks at the impact of charitable subsidies on religious giving and religious attendance, and finds that the giving of time (volunteering) and the giving of money (charitable donations) to religious activities are complements. Complementarity between volunteering and charitable donations was also found in [Feldman \(2010\)](#), who uses an alternative household survey covering two years in the late 1990's. Feldman concludes that it is the income effect of charitable donations that offsets the substitution effect of a tax subsidy, leading to an observed positive correlation between time and money giving.

An additional useful feature of the PSID charitable giving module is that subcategories such as charity for needy or for education are also recorded. [Brown et al. \(2012\)](#) look at charitable giving in the U.S. after an unexpected national disaster abroad (Indian Ocean Tsunami of 2004) to study how charitable giving responded across different categories including giving to organizations that focus on the needy. [Helms and Thornton \(2012\)](#) use the subcategory of religious organization charity as well as household information of religious preferences to study the impact of religiosity on charitable behavior. They find that secular givers, when compared to religious givers, are more sensitive to relative price and income changes.

Related to issues of social fragmentation, [Osili and Xie \(2009\)](#) study differences between immigrants and natives in terms of charitable giving using the PSID data. They find that apparent gaps in charitable giving between immigrants and natives vanish once other factors such as income

and education are accounted for properly.

Lastly, social fragmentation in highly polarized societies where there has been contestation over ruling authority has been less well studied. One exception is Northern Ireland, where policing occurred within a deeply divided society prone to sectarian bias and where police legitimacy was contested. In this extreme environment, [Weitzer \(1985\)](#) studies the role of aggressive, militarized policing and how it can exacerbate rather than resolve social tensions. While the U.S. experience is less contested and extreme, the risk of militarized policing enhancing already existing social fissures is relevant.

3 Data Description

3.1 Panel Study of Income Dynamics

Our source of family level data is the biennial Panel Study of Income Dynamics from 2001-2013, which includes philanthropic information on charity and volunteer work from the Center on Philanthropy Panel Study (COPPS) subset of the PSID. There are other similar sources for charitable behavior data, including surveys and IRS tax-data, however, the PSID is considered to be the highest quality data-set. This is due to characteristics of higher interviewer experience, ability to estimate high into the income distribution, availability of other socioeconomic control variables, and a nationally representative sample.² The COPPS module—which is administered to the same nationally representative sample as the main PSID—began in 2001. Variables pertaining to charity include households’ dollar amounts given annually in 11 donation categories, from which we will primarily focus on total contributions.³ The PSID also includes some indicators of other types of philanthropic participation. We create a binary variable on whether the head of household performed volunteer work over the past year, as well as a variable reporting the amount of financial

²For further discussion of the quality of the charity data, see [Wilhelm \(2007\)](#)

³The 11 individual categories include religious donations, combined purpose organization donations, donations to the needy, health related donations, education related donations, youth organization donations, cultural donations, community donations, donations to benefit the environment, international/peace donations, and “other” types of donations.

help given annually to family members not in one's own household.

The PSID also provides basic socioeconomic and demographic information, including sex, race, age, education, state of residence, wealth, and income. These background variables come from the survey respondent within each household, in most cases the "head of household," except for wealth and income which are household aggregates. Our final sample includes 52,643 household-year observations, although various models omit up to a few hundred observations due to non-response.⁴

Table 1 presents summary statistics for the final sample, for the sub-sample of observations with a black respondent, and for those with a respondent of any other race. Total charitable donations average \$1,210 per year across all households; sub-sample average donations are \$1,459 for the non-black households and \$739 for black households. This disparity persists across all donation categories, as well as other forms of charity such as volunteering and intra-family transfers.

Household background characteristics appear reasonable for a nationally representative survey. 30 percent of respondents identify as female (the survey is typically administered to the head of household), and in 35 percent of households the respondent reports as black (at some point during the 2001-2013 panel).⁵ The average respondent is 46 years old with one year of post-secondary education. On average, households earn \$65,000 per year and possess net worth of \$228,000, while black households have lower income and are less wealthy.

3.2 State-level Data

The rest of our data are measured at the state level. The key variable of interest is our measure of militarized police, which is drawn from data covering the entire roster of equipment transferred from the Department of Defense to local law enforcement agencies through the 1033 Excess

⁴The exception here comes when using volunteering data. Questions on volunteering were asked by the PSID only in the 2001, 2003, 2005, and 2011 waves, so the sample size when using the volunteering information is limited to 26,091.

⁵The initial motive for collecting in-depth household data over time was to study the dynamics of poverty. Thus the original 1968 sample included a disproportionately large number of low-income households. Oversampling of families poor in the late 1960's resulted in a sizable sub-sample of black households.

Equipment program. The 1033 program was officially authorized under the National Defense Authorization Act for Fiscal Year 1997, which expanded the prior surplus equipment program known as 1208. Prior to this change, surplus military equipment was earmarked for specific drug interdiction activities as part of the broader national policy commonly referred to as the “War on Drugs.” Starting in 1997, any bona fide law enforcement activity was sufficient justification for requesting surplus military equipment.⁶

The process for requesting equipment requires state level oversight, with a Memorandum of Agreement between the Defense Logistics Agency (DLA), which is responsible for oversight of the 1033 program, and the state. The state then appoints a DLA State Coordinator tasked with oversight of all transferred surplus equipment to ensure accountability and proper use. Thereafter, any representative of a Law Enforcement Agency (LEA) can go online through the DLA website and request equipment available for transfer. The LEA is financially responsible for all costs associated with transportation of the equipment, and then all future maintenance, but pays no additional “mill price.”

Since the creation of the 1033 program, over \$6 billion dollars worth of equipment (valued at the original acquisition price by the military) have been transferred from the DLA to state and local law enforcement agencies. There are two broad classifications of surplus equipment related to disposal. Some equipment requires special demilitarization when it is no longer of use, and must be returned to the DLA. Such equipment is best thought of as regulated loan of capital since ownership is never completely transferred to the LEA. For other items, which are of low value or do not require special demilitarization, the LEA takes full ownership of the equipment after one year and is free to dispose of the equipment as desired after that point.

To best measure increases in police militarization, we focus on equipment requiring special demilitarization. This includes equipment such as firearms, firearm components, firearm optics, tactical vehicles, aircraft, boats, and night vision or infrared equipment. For example, one of the most popular pieces of requested equipment is the Mine Resistant Vehicle, or MRAP, which

⁶For a more complete description of the history of the 1033 Program, see [Haynes and McQuoid \(2016\)](#).

was originally designed to protect personnel in Iraq from Improvised Explosive Devices (IEDs), land mines, and small-arms fire. In our data, over 559 MRAPs were transferred to local police departments, with configurations ranging in value from \$400,000 to over \$1.3 million per unit. MRAP transfers comprise over 20% of the total value of surplus equipment transferred in our data.

We choose to measure police militarization as the total yearly value of surplus equipment requiring special demilitarization transferred through the 1033 program to a given state. Our view is that equipment requiring special demilitarization is most consistent with the idea of a “militarized” police force as weapons and vehicles are more directly observable and consistent with the concept of power projection.⁷ We therefore exclude equipment that was transferred under the 1033 program that did not require special demilitarization such as shredders, sleeping bags, office supplies, and even guitar amplifiers.⁸

Our use of state level values is driven by two considerations. First, program oversight takes place at the state level through the program coordinator. Much of the transferred equipment is to LEAs that span multiple law enforcement jurisdictions, including county and state agencies. Capital equipment can therefore be easily redeployed across law enforcement agencies within a state, but not across states. The state level measure therefore captures most of the meaningful spillovers across localities. Second, the PSID geographic data is relatively sparse due to privacy concerns. As such, state data is the most disaggregated geographic identifier available. Finally, we use the previous two years of transfers since the PSID survey takes place biennially.

Table 1 provides summary statistics for the militarization variables. The mean state observation in our data receives 5 million dollars in transferred military equipment, with a standard deviation of 18.5 million dollars. Our interest will focus on differences across black and non-black households, and as can be seen in the table, there is no aggregate evidence that black households in our data come disproportionately from states experiencing unusual levels of military transfers. In some specifications, we will focus on a more narrow definition of military equipment. Based on the

⁷Haynes and McQuoid (2016) find that increased participation in the 1033 program has no effect on sworn police officer rates, suggesting that the program results in a more (military-) capital intensive police force.

⁸Our results are robust to a broader definition of police militarization that includes all transferred equipment. We also discuss more narrow definitions of militarization in our analysis below.

national stock number (NSN) classification, a 13-digit code assigned to each type of equipment, we consider all transfers with an NSN starting in the 1000s: rifles, pistols, small arms parts and accessories including weapons sightings, helicopters and other types of aircraft and aircraft parts. This weapon-based measure of militarization is also reported in Table 1, and the typical state receives 2.5 million dollars of surplus weapons for a given observational period, or roughly half of the total value of all transfers requiring special demilitarization.

Our instrumental variables approach, which we discuss in more depth in the empirical implementation section below, relies on exposure to military culture. The idea is that police agencies that are more aware of military personnel, language, customs, and equipment are more likely to participate in the 1033 program, and to participate more heavily. Exposure to military culture is captured by federal expenditures on national defense at the state level. These expenditures are in part determined by historical accidents (and continuing political economy considerations) and in part by international security objectives. As such, changes in federal defense spending are often set years in advance based on congressional authorization and are unrelated to local dynamics in civil society and civic participation. Table 1 provides summary statistics for federal military spending within a state in our dataset.

Additional state level variables used as controls include median household income as well as the two-year average unemployment rate and percentage of population in prison. These state controls capture alternative social forces that may be correlated with police militarization or federal government spending within a state and which may impact social cohesion and civic participation. Our preferred specifications will also include year and state fixed effects that will capture any time-invariant state effects as well as national trends.

4 Econometric Specification

The goal of our paper is to understand how aspects of civic participation change in response to increased police militarization. We focus on household charitable giving of time and money, and

consider differential responses for black households. Law and order institutions are at the center of social fragmentation and polarization in the United States, and a more robust, aggressive police force may exacerbate these underlying fractures, leading to a greater sense of social distance for black households.

The central identification problem for our study is that militarized police are not randomly assigned across the country, but rather, police departments actively choose to participate in the 1033 program to acquire surplus military equipment. We do not directly observe the determinants of this choice, but suspect that police departments that are more actively engaged in the 1033 program are different from those that are not. This matters for identifying true causal effects of program participation if these differences are related to determinants of civic participation. For example, areas featuring greater social fragmentation may be experiencing a breakdown of social norms and bonds, which could lead to a withdrawal from civic life and an increase in crime. If police departments are responding to current or anticipated social fracture by acquiring more military equipment, we might observe a negative relationship between police militarization and civic engagement, but not necessarily a causal relationship.

To properly deal with this selection bias concern, we instrument for state participation in the 1033 program using federal military expenditure within a state over time. The idea behind the instrument is that greater exposure to the military, including people, equipment, culture, and customs will lead to greater awareness of the 1033 program and the equipment available through the program. This awareness, all else equal, will result in greater participation in the program. Furthermore, federal military spending is driven by a combination of historical circumstances that pre-date the present analysis, and international security objectives that are unrelated to local civic engagement. For example, the decision to invest in production of the F-35 Joint Strike Fighter will be unrelated to civic participation in Fort Worth, Texas, but more federal spending on the fighter program is likely to lead to more military manpower in and around Fort Worth, and a greater awareness of weapons and equipment innovations in this area.

As an first pass on the relevance of our instrument, consider Figure 1. The instrumental variable

approach taken here has a local average treatment effect interpretation. That is, the IV approach estimates the impact of police militarization on charitable giving specifically based on states who respond to an increased federal military presence with increased police militarization. Something peculiar about these states—either observed or unobserved—would limit our ability to extend our interpretation of the causal effect more broadly to the entire country. Fortunately, Figure 1 shows some states respond very strongly to federal military spending while the vast majority of states have reasonably large positive correlations. Moreover, the states with the strongest correlation are not confined to any particular geographic region, suggesting the estimated treatment effect is broad-based.

Following the logic of our identification strategy, our first stage predicts participation in the 1033 program based on changes in federal military spending at the state level over time. The predicted participation in 1033 is then used in the second stage to study how police militarization impacts different aspects of civic participation, and how this differs across races. Our first stage is given by the following,

$$\text{Police Militarization}_{st} = \alpha_s + \alpha_t + \alpha * \text{Federal Military Spending}_{st} + \alpha_{st} * X_{st} + \epsilon_{st} \quad (1)$$

where X_{st} includes other state level predictors such as median income, unemployment, and prison population rate. Although our instrument varies at the state level, our second stage predicts charitable outcomes at the individual level. As such, our first stage includes individual and family level variables although these do not influence state level militarization directly.

Results from the first stage can be found in Table 3. Federal military spending has a positive and highly significant effect on predicting the value of transferred equipment for a state in a given year. All specifications include both state and year fixed effects as well as both state-level and individual-level covariates. Areas with higher unemployment rates, higher prison populations, and higher median incomes tend to have greater participation in the 1033 program. The finding that some

individual-level covariates are significant determinants of participation in the 1033 program point to sorting across states on observable characteristics, raising the specter of sorting on unobservable characteristics. This further justifies our concern about selection into the 1033 program being driven by unobservable factors that may also be correlated with social choices including civic engagement.

Our second stage considers the impact of this predicted participation in the 1033 program on civic engagement outcomes:

$$\text{Civic Engagement}_{ist} = \beta_i + \beta_s + \beta_t + \beta_{ist} * X_{ist} + \beta_{st} * X_{st} + \beta * \text{Police Militarization}_{st} + \nu_{ist} \quad (2)$$

where *Civic Engagement*_{ist} measures various aspects of civic engagement including the giving of time and money for individual *i* living in state *s* in time *t*. Individual covariates include age, educational attainment in years, income, and wealth (or debt). To study differences in racial responses, we will consider the interaction between race of the individual giver and police militarization (or we will restrict estimation to subsamples grouped by race). Individual fixed effects are included to account for unobserved time-invariant individual factors that influence charitable giving.

Finally, we will consider alternative outcomes that are unrelated to social distance and therefore should not be impacted by our hypothesized channel leading from social fragmentation and enhanced police militarization to greater social distance. Our results are robust to additional validity tests and multiple measures of police militarization.

5 Results

We start by looking at determinants of the total amount across all donation categories for households surveyed in the PSID. Column (1) of Table 2 uses the entire sample of households, and includes individual covariates along with household, year, and state fixed effects. Additionally, the model includes state level covariates. The results are consistent with previous research, which finds

that charitable contributions increase as both income and wealth increase. We find no correlation between education and total donations, but this may in part be related to our usage of individual fixed effects which would leave little variation in education over time. There is no apparent age effect, which may be washed out by year controls or because two-year windows may not be a sufficiently long horizon for identifying the association between aging on charity. For state-level covariates, only unemployment has a notable (negative) relationship with total donations. Our variable of interest, police militarization, has no apparent effect on charitable giving in this baseline specification.

In column (2), we instrument for police militarization using federal military spending at the state level, and the sign of the coefficient changes from positive to negative, and is now statistically significant. The negative coefficient implies that increased police militarization results in reduced overall charitable giving, which we interpret as being driven by increased social distance. For every additional million dollars in police militarization, the typical household reduces its charitable giving by \$27, or a 2% decline in average charitable donations based on the sample mean of \$1200. A more militarized police thus appears to impose social costs counter to the intended focus of improved crime prevention.

To explore this overall finding further, we next consider differential impacts by race by first excluding black households from the sample in columns (3) and (4). While the point estimates are not very different from their counterparts in columns (1) and (2), neither estimate is statistically significant, although in the case of the IV estimate, this is mostly driven by larger standard errors. When we consider black households in columns (5) and (6), we find a small negative (and insignificant) effect using the FE model, but find a much larger negative effect of police militarization using an IV approach. When comparing the results of columns (2), (4), and (6), the impact of police militarization of charitable giving appears most clearly for black households. However, the estimate of the white households is also negative (though not significant).

In our preferred specification, we return to the full sample, but introduce possible fragmentation through an interaction term between race and police militarization. In column (7), the effect

of police militarization is positive but significant for non-black families, and negative and statistically significant for black households. When we instrument, as in column (8), the results are very different. Non-black households reduce charitable donations in response to increased police militarization, but black households reduce charitable giving even more. That is, police militarization reduces overall charitable giving, but it has a particularly damaging effect for black households.

For non-black households, every additional million dollars in police militarization implies an individual household's charitable giving would decline by \$20, or about 1.4% of total donations. For black households, the effect is notably larger, where the impact of an additional million dollars in police militarization would result in a reduction of charitable giving by \$55 (or about 7.4% of total donations given the mean total donations of \$738 for black households). To offset this effect, for example, would require a 25% increase in income.

The first stage regression results are reported in Table 3. For each of the models, federal military spending is statistically significant at the 1% level and predicts that as federal military spending increases in a state in a given year, participation in the 1033 program increases, even after controlling for state and year fixed effects as well as other state level covariates. The state level covariates imply that as median income goes up, participation in the program increases. In addition, the unemployment rate and the prison population rate are positively associated with increased state level participation in the 1033 program.⁹

One interpretation of these results is that richer states as well as states with more social concerns (crime, unemployment) have a greater interest in a well-equipped police force. However, for our purposes, our interest lies in including additional channels that might independently impact civic engagement and which could be correlated with federal (military) spending. If states that are more highly dependent upon federal transfers (of any kind) are also states with weaker local institutions, changes in civic engagement may be responding to these alternative forces rather than enhanced police militarization per se.

⁹Columns 4 and 5 should be interpreted together since our interaction model requires predicted values for both relevant endogenous variables, our measure of militarization and our measure of militarization interacted with black households. The joint effect yields the expected positive correlation between federal military spending and participation in the 1033 program.

Overall, the first stage is highly significant and predicts state level participation in the 1033 program well, regardless of the specific model employed. This robustness reinforces the connection between changes in federal military spending at the state level and participation in the 1033 program. Greater spending on military results in more local resources dedicated to the mission of the DoD, and these resources generate a wider awareness of military equipment and military culture that impacts local police departments engagement with the 1033 program. Given the strength of the first stage, our IV results are valid under the exclusion restriction that changes in federal military spending are unrelated to local civic engagement and participation.

Having found that increased police militarization impacts all households, but particularly black households, we next explore alternative forms of giving in Table 4. We proceed by considering different outcomes related to civic engagement using our preferred interaction model with instrumental variables. In column (1), we consider the extensive margin of charity using a linear probability model with instrumental variables. While we do not find any effect for non-black households on the extensive margin, black households are more negatively impacted by police militarization than non-black households, although the effect is small in absolute terms as well as relative to the average proportion of households that donate. Given the myriad of forces that drive a person to donate, it may not be surprising that the intensive margin is more responsive than the extensive margin.

In column (2), we consider the charitable giving of time rather than money. For people who have a desire to contribute to their community, volunteering is often a substitute for monetary donations. The impact for black households is similar to the impact on the extensive margin of monetary donations. Again, we find no significant effect for non-black households, but find a marginally significant effect for black households. The share of households volunteering is lower than the share donating money (0.24 vs. 0.57 in the sample), but the pattern that police militarization impacts black households differently than non-black households is confirmed.

In column (3) we consider a third dimension of charity: giving to extended family members. We consider the possibility that while police militarization may reduce civic engagement, it may increase more inward forms of charitable giving through greater intra-family giving. However, as

column (3) makes clear, this is not the case. The point estimates are highly insignificant for both black and non-black households, providing no evidence that the reduction in civic engagement is being offset by an increase in engagement within the family unit.

In columns (4) and (5), we break out two of the most important components of charitable giving recorded in the dataset, donations to the needy and donations to education. These two measures of giving are integral to the idea of community— helping those in need and investing in the next generation. For both subcategories of charitable donations, the effect of police militarization on black households is negative and statistically significant. For every one million dollars of increased police militarization, black households reduce their donations to the needy by 17% and educational donations by roughly 25%. These findings are particularly concerning both in the short run and in the long run. Investment in education leads to higher returns in the future, while contributions to the needy help to offset troubles in the short-run. The impact of police militarization on black families is therefore likely to be felt not only today, but well in to the future.

Table 4 confirms that a variety of dimensions of civic engagement are impacted by police militarization for black families. The national concerns that police militarization is systematically impacting black families negatively and leading to greater social fracture are borne out in the data.

To test the robustness of our results, we consider a series of placebo tests. In Table 5, we first consider the monthly car payment of a household, which should have no relation to police militarization. But perhaps what our main results (in Table 2) are picking up is expected economic opportunity, which current income or wealth does not capture. For example, expectations about future income may alter decisions to donate, but not show up in current income data. These expectations would also influence durable good purchases, such as cars, which depend on present and future ability to pay. Column (1) suggests that such an unobserved economic opportunity effect is not driving our results, as we estimate no significant relationship between police militarization and monthly car payment, either for black or non-black households.

In the next three columns, we consider measures of self-identification that should not be directly impacted by police militarization to confirm that our approach is capturing our hypothe-

sized, rather than spurious, effects. Column (2) considers self-reported health, which should not be related to police militarization, and again we find no effect for households. Columns (3) and (4) look at self-reported religious identity and we again find no effect of police militarization for any type of household. Here we present both fixed effects and random effects models since religious affiliation may not change much over time.

Column (5) considers a slightly different type of placebo effect. Public transportation is a necessity for many people, but it also an explicit, personal, shared engagement with other people. To the degree that one has the choice between public and private transportation, choosing public transportation is a social choice in the language of [Akerlof \(1997\)](#). When we consider the impact of police militarization on public transportation costs, we find that for non-black households, increased police militarization leads to lower social engagement. That is, as police militarization increases, we find non-black households reducing their consumption of public transportation. We find no effect for black households, who are three times as likely to use some form of public transportation compared to non-black households. One interpretation of these results is that for many black families, public transportation is a necessity rather than a luxury. For non-black households, however, public versus private transportation contains more elements of a social choice, and the results suggest that these households are withdrawing from public transportation in response to increased militarization.

Finally, we consider a variety of IV validity tests in [Table 6](#). Columns (1) and (2) replicate our preferred specification from [Table 2](#) and the associated first stage from [Table 3](#). In columns (3) and (4), we test the political economy aspect of our identification. Our instrument depends on the idea of federal military spending being driven by international security concerns and historical accidents, but it may be the case that particularly influential congressmen could extract more pork than others, and redirect this pork to local areas. If federal military spending is correlated with these particularly talented congressmen, and these congressmen also bring in other forms of federal spending that impact local institutions or are proxies for more effective state governments, our exclusion restriction may be violated. To consider this possibility, we note that earmarks were

eliminated starting in 2011 with the election of a Republican-controlled House of Representatives. As such, the ability to acquire pork was severely curtailed. If our instrument was compromised by possibly talented congressmen, we should expect different results before and after the ban on earmarks. Columns (3) and (4) show that the results estimated when the 2013 wave is omitted are consistent with the full sample in Columns (1) and (2), suggesting that our instrument was not compromised in this way.

In columns (5) and (6), we consider a more narrow definition of police militarization by only including “weaponized” equipment. Each piece of equipment has a unique 13-digit National Stock Number (NSN), which starts with a 4-digit Federal Classification System (FCS) number followed by a 9-digit item code. The FCS is useful in that it groups equipment into related categories. We focus on the equipment that has a 1xxx FCS, which includes weapons and aircraft. Such equipment proxies for the more muscular idea of police militarization, as opposed to other forms of capital equipment such as computers and radios that can enhance police productivity, but which are not usually directly observable by citizens. Results stemming from this narrower instrument are similar but suggest that more weaponized police militarization is significantly more detrimental for black household charity. The point estimate is nearly twice as large for black households compared to column (1), -99 compared to -55. This implies that an additional one million dollars in weaponized police militarization results in a 13.4% decrease in charitable giving. The impact on non-black households is slightly larger as well, but estimated less precisely and thus not statistically significant at conventional levels. Compared to the broader definition, it is this overt, muscular measure of police militarization that seems to matter for social fragmentation.

Finally, we consider a third measure of police militarization by looking at the stock of equipment as opposed to yearly flows. Equipment transferred through the 1033 program is capital equipment that can be utilized over many years, not just the year of transfer. When we measure police militarization through the rolling sum of transfers (columns (7) and (8)), we again find evidence of social fracture. The estimated coefficient for black households is smaller by about 50%, but still statistically significant and economically meaningful. The estimated coefficient for

non-black households is 75% smaller and no longer statistically significant, although it remains negative. In all three approaches, the general validity of the instruments are confirmed as the same basic patterns of social fragmentation emerge.

6 Conclusion

The expansion of militarized police in the United States has evolved steadily over the last few decades, but picked up steam since the introduction of the Department of Defense Excess Property Program 1033 in 1997. The program dramatically reduced the cost of acquiring militarized equipment by local law enforcement agencies, and expanded the mission scope beyond drug interdiction. This has led to a very different social engagement with police, which events in Ferguson drove home to the public at large.

Previous work on the impact of police militarization in general and the 1033 program in particular have mostly been descriptive, with an emphasis on anecdotal evidence. We contribute to the evolving policy discussion regarding police militarization by examining the impact of police militarization on measures of civic engagement, with a particular focus on charitable giving.

We estimate the causal impact of transfers through the 1033 Program on civic engagement by using an instrumental variables approach to deal with the non-random assignment of equipment across police departments. Our identification strategy focuses on exposure to military customs, personnel, and equipment leading to greater participation in the 1033 program, where variation in exposure is driven by federal military spending across states over time. The initial distribution of federal military spending has historical and political economy determinants, leading us to focus on year-to-year changes in federal military spending which are driven by international strategic objectives and often set years in advance by congressional legislation. This exclusion restriction claim, along with a powerful first stage, comprises our identification strategy.

Our results suggest that police militarization has a fragmenting effect on civic engagement. Black households reduce charitable giving in response to increased police militarization, with each

additional million dollars of military equipment transferred to local police resulting in a 7.4% decline in overall charitable giving for black households. The effect for non-black households is smaller and less robust, but a pattern emerges. The relative impact of police militarization is consistently worse for black households compared to non-black households.

The estimation approach is shown to be robust, with alternative definitions of police militarization leading to qualitatively similar estimates. Additional placebo and instrumental validity tests further support the robustness of the methodology.

Police militarization is not a new phenomenon in the United States, but it has increased in scale and scope over the last two decades. Law and order institutions are central to a well-functioning society, but these institutions have the power to divide as easily as unite. In the case of police militarization, underlying fractures have been amplified rather than reduced, leading to lower civic engagement for black households.

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Figures

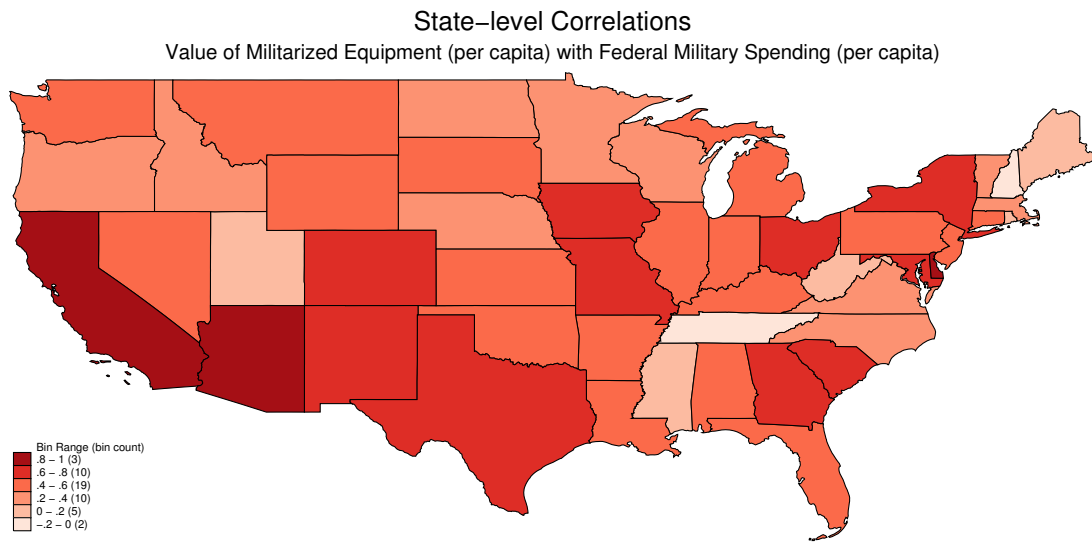


Figure 1: Federal Military Exposure and 1033 Participation

Tables

	All		Not Black		Black	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Charity-related individual-level variables:						
Total donations	1,210	3,362	1,459	3,843	739	2,093
Donated to charity (binary)	0.57	0.50	0.65	0.48	0.42	0.49
Religious donations	778	2,504	890	2,823	565	1,729
Combo donations	119	663	151	760	59	413
Needy donations	119	620	151	732	58	304
Health-related donations	45	331	62	395	14	140
Education-related donations	61	860	85	1,057	15	129
Youth organization donations	18	208	24	246	8	102
Cultural donations	14	198	20	238	3	80
Community donations	7	93	9	105	5	65
Environment-related donations	12	146	17	178	1	40
International/peace donations	11	149	15	179	3	53
Other types of donations	26	359	36	433	8	131
Volunteered (binary)*	0.24	0.43	0.27	0.44	0.19	0.39
Financial help to family **	753	15,022	938	18,491	400	1,864
Car payment (USD per month) ***	107	552	119	634	85	343
Any bus or train fares paid (binary) ****	0.10	0.30	0.06	0.25	0.16	0.37
Other individual-level variables:						
Female (binary)	0.30	0.46	0.21	0.41	0.48	0.50
Black (binary)	0.35	0.48				
Age (years)	45.7	16.2	47.1	16.9	43.1	14.7
Education (years)	13.0	2.7	13.4	2.8	12.5	2.3
Household income (annual USD)	64,996	97,722	77,908	115,652	40,504	37,112
Household wealth (USD)	227,922	1,136,839	319,936	1,385,284	53,392	239,549
State-level variables:						
Median Household Income (in ten thousands of US dollars)	5.4	0.8	5.5	0.7	5.2	0.8
Unemployment rate (% , average over past two years)	6.6	2.1	6.5	2.1	6.8	2.1
Population in prison (% , average over past two years)	0.005	0.001	0.004	0.001	0.005	0.001
Value of all transferred equipment (millions USD, sum of past two years)	5.0	18.5	4.9	18.8	5.1	17.9
Value of specific "weaponized" transfers (NSN1000s, millions USD, sum of past two years)	2.5	17.5	2.6	17.8	2.3	16.8
Federal Military GDP by state (millions of current USD, sum of past two years)	13,675	14,901	13,492	15,690	14,021	13,269
Number of observations		52,643		34,470		18,173

Notes: Table contains sample means and standard deviations for individual and state-level variables. Individual-level variables are from the PSID 2001-2013, unless noted otherwise. The first two columns display statistics for the full sample; the next four columns show statistics for subsamples of families with a non-black head of household and black head of household, respectively. All charity related variables are in annual USD, reported on the previous year, unless otherwise noted. Volunteered (binary) has a narrower sample size, 28,852, 19,142, and 9,710, respectively. Financial help to family variable has a narrower sample size, 52,207, 34,266, and 17,941, respectively. Monthly car payment has a narrower sample size, 52,149, 34,224, and 17,925, respectively. Any bus or train fares paid variable has a narrower sample size, 52,420, 34,425, and 18,045, respectively.

Table 1: PSID and Militarization Variables - Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsample of households:	All	All	Not Black	Not Black	Black	Black	All	All
Dependent variable: Total donations	FE	IV FE	FE	IV FE	FE	IV FE	FE	IV FE
Endogenous covariates:								
Value of equipment	0.188 (0.501)	-26.82** (11.66)	0.367 (0.641)	-24.85 (15.75)	-0.332 (0.750)	-30.27** (14.18)	0.995 (0.620)	-20.33* (11.92)
x Black							-2.512** (1.004)	-34.97*** (7.302)
Individual-level covariates:								
Age	-31.18 (27.58)	-31.47 (28.34)	-12.58 (39.97)	-5.885 (41.90)	-70.63* (39.82)	-81.50* (42.02)	-32.00 (27.60)	-42.87 (30.62)
Education (years)	21.24 (15.40)	31.04* (16.49)	4.480 (20.11)	10.55 (20.76)	54.78** (23.65)	72.58*** (27.61)	21.50 (15.39)	36.40** (17.05)
Income <= 0 (binary)	2041.3*** (220.7)	2096.0*** (229.7)	2770.3*** (382.1)	2861.5*** (399.9)	1048.0*** (157.4)	1069.4*** (170.3)	2039.6*** (220.8)	2082.7*** (234.7)
log(Income) if >0, = 0 otherwise	225.9*** (21.98)	233.3*** (23.00)	315.2*** (37.23)	323.0*** (38.46)	105.9*** (15.92)	111.4*** (17.48)	225.7*** (21.99)	231.5*** (23.39)
Debt <=0 (binary)	660.9*** (77.47)	645.3*** (79.66)	858.8*** (131.5)	881.9*** (135.0)	392.7*** (65.79)	337.9*** (73.84)	660.2*** (77.46)	633.5*** (80.98)
log(Wealth) if >0, = 0 otherwise	79.62*** (9.166)	75.32*** (9.530)	103.2*** (14.86)	102.0*** (15.10)	46.67*** (8.608)	39.40*** (9.704)	79.58*** (9.164)	74.10*** (9.623)
State-level covariates:								
Median Income	81.04 (74.20)	268.9** (112.6)	38.85 (103.4)	268.5 (180.5)	129.7* (73.42)	230.3*** (85.72)	79.55 (74.19)	281.1** (113.0)
Unemployment rate	-36.28** (16.34)	137.9* (75.54)	-44.91** (21.70)	136.3 (113.7)	-3.703 (23.06)	148.0** (74.38)	-36.97** (16.33)	158.8** (75.58)
Population in prison (%)	-48027.6 (50162.3)	52291.2 (73277.0)	-59601.9 (68693.2)	31934.0 (98259.3)	-83704.1 (59378.4)	31484.0 (84628.4)	-48918.0 (50162.5)	57518.2 (73656.7)
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	52643	52643	34470	34470	18173	18173	52643	52643

Notes: Table contains FE and IV FE estimates of equation (2). The dependent variable is the total value of charitable donations reported by a family in the past year, and the primary independent variable of interest is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local law enforcement agencies (in millions of USD) within the two-year time period. All models control for state and year fixed effects. Standard errors are clustered by household. Columns 1-2 present estimates from the full sample. Columns 3-4 display estimates from the subsample of families with a non-black head of household; columns 5-6 show estimates from families with a black head of household. Columns 7-8 again use the full sample but include an interaction of spending on military equipment with a dummy for a black head of household. Significance: * 10 percent; ** 5 percent; *** 1 percent.

Table 2: Police Militarization, Social Distance, and Charitable Giving - FE and IV Results

Subsample of households: Dependent variable:	(1) All Value of equipment	(2) Not Black Value of equipment	(3) Black Value of equipment	(4) All Value of equipment	(5) All Value of equipment x Black
Instruments:					
Federal Military Spending	0.000452*** (0.0000167)	0.000454*** (0.0000217)	0.000397*** (0.0000299)	0.000475*** (0.0000180)	-0.000111*** (0.0000094)
x Black				-0.0000740*** (0.0000279)	0.000645*** (0.0000276)
Individual-level covariates:					
Age	-0.00500 (0.305)	0.288 (0.370)	-0.378 (0.499)	-0.0129 (0.305)	-0.258 (0.240)
Education (years)	0.375*** (0.121)	0.252* (0.139)	0.607** (0.237)	0.377*** (0.121)	0.211** (0.0980)
Income <= 0 (binary)	1.803 (1.700)	3.375 (3.186)	0.505 (2.166)	1.787 (1.700)	0.0909 (1.126)
log(Income) if >0, = 0 otherwise	0.246* (0.140)	0.286 (0.184)	0.154 (0.219)	0.243* (0.140)	0.0168 (0.0992)
Debt <=0 (binary)	-0.516 (0.629)	0.907 (0.959)	-1.698** (0.809)	-0.525 (0.629)	-0.352 (0.402)
log(Wealth) if >0, = 0 otherwise	-0.149** (0.0704)	-0.0488 (0.102)	-0.224** (0.0950)	-0.151** (0.0704)	-0.0491 (0.0424)
State-level covariates:					
Median Income	5.953*** (0.404)	8.036*** (0.571)	2.521*** (0.566)	5.942*** (0.405)	1.547*** (0.249)
Unemployment rate	6.100*** (0.287)	6.745*** (0.368)	4.945*** (0.467)	6.091*** (0.287)	1.808*** (0.179)
Population in prison (%)	5772.8*** (400.3)	5937.4*** (484.4)	5196.1*** (704.7)	5763.5*** (400.4)	1302.0*** (258.6)
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
F-statistic	170.08	119.98	41.9	168.44	54.29
Observations	52643	34470	18173	52643	52643

Notes: Table contains first stage estimates for the IV FE models presented in the previous table. The dependent variable is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local law enforcement agencies. The instrument is aggregate federal military spending at the state level over the two-year time period (in millions of USD). All models control for state and year fixed effects. Standard errors are clustered by household. Column 1 presents estimates from the full sample. Column 2 displays estimates from the subsample of families without a black head of household; column 3 shows estimates from families with a black head of household. Columns 4 and 5 use the full sample to estimate predicted values for the two endogenous covariates in column (8) of Table 2, value of equipment and value of equipment interacted with black head of household. Significance: * 10 percent; ** 5 percent; *** 1 percent.

Table 3: First Stage Results

Dependent variable:	(1) Donated	(2) Volunteered	(3) Family transfers	(4) Needy donations	(5) Education donations
Endogenous covariates:					
Value of equipment	0.00210 (0.00163)	0.00174 (0.00171)	82.30 (136.4)	-5.088 (3.143)	0.0774 (2.639)
x Black	-0.00304** (0.00132)	-0.00245* (0.00137)	-42.01 (41.60)	-4.757*** (1.467)	-3.916** (1.731)
Individual-level covariates:					
Age	-0.00178 (0.00492)	-0.00601 (0.00712)	7.225 (101.2)	-0.948 (6.725)	-9.364 (9.486)
Education (years)	0.00161 (0.00299)	0.00520 (0.00446)	-7.730 (37.22)	1.989 (3.962)	-2.864 (2.268)
Income <= 0 (binary)	0.382*** (0.0360)	0.0132 (0.0540)	-1160.8 (2496.4)	130.3** (53.04)	193.9*** (53.55)
log(Income) if >0, = 0 otherwise	0.0423*** (0.00340)	0.00728 (0.00460)	-118.2 (268.7)	18.28*** (5.588)	21.47*** (5.851)
Debt <=0 (binary)	0.0699*** (0.0169)	0.0711*** (0.0216)	-1035.8 (1616.5)	71.92*** (19.14)	22.06 (20.70)
log(Wealth) if >0, = 0 otherwise	0.0103*** (0.00178)	0.00809*** (0.00232)	-162.2 (226.7)	8.338*** (2.408)	2.445 (2.454)
State-level covariates:					
Median Income	0.00201 (0.0159)	-0.00685 (0.0366)	-138.1 (729.1)	63.06* (33.03)	59.34 (50.32)
Unemployment rate	-0.00468 (0.0112)	0.00186 (0.0212)	-439.2 (874.6)	40.67** (20.73)	4.030 (16.11)
Population in prison (%)	-9.349 (9.820)	-22.05 (36.79)	-23057.6 (536097.5)	6722.1 (13574.3)	2349.0 (12830.5)
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Observations	52643	26091	52166	52014	52491

Notes: Table contains IV FE estimates of versions of equation (2) with various alternate measures of charitable donations as dependent variables. In all models, the primary independent variable of interest is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local law enforcement agencies (in millions of USD) within the two-year time period, and its interaction with the black head of household dummy. All models control for state and year fixed effects. Standard errors are clustered by household. Column 1 adopts a binary dependent variable for whether a family reports any charitable donations in the past year. Column 2 uses a binary outcome of whether a family reports any time spent volunteering for charitable purposes in the past year. Column 3 uses the value of intra-family transfers in the past year as the dependent variable. Columns 4 and 5 report results when the dependent variable is the value of charitable donations to the needy and to education in the past year, respectively. Significance: * 10 percent; ** 5 percent; *** 1 percent.

Table 4: Alternative Forms of Giving

Dependent variable:	(1)	(2)	(3)	(4)	(5)
	Monthly car payment	Self-reported Health	Christian (FE)	Christian (RE)	Any regular bus/train fares
Endogenous covariates:					
Value of equipment	1.387 (3.434)	-0.00153 (0.00367)	0.000225 (0.000441)	0.000107 (0.000289)	-0.00191* (0.00107)
x Black	1.082 (1.588)	-0.00468 (0.00335)	-0.000464 (0.000428)	0.000196 (0.000224)	0.00156 (0.00112)
Individual-level covariates:					
Age	-26.19** (12.32)	-0.0121 (0.0132)	-0.00421 (0.00276)	0.00350*** (0.000203)	-0.00129 (0.00437)
Education (years)	-2.491 (5.022)	-0.00997 (0.00776)	-0.000433 (0.00107)	-0.000932* (0.000481)	0.00320 (0.00221)
Income <= 0 (binary)	190.1*** (23.68)	-0.318*** (0.0961)	0.00725 (0.0113)	0.00963 (0.00728)	-0.0230 (0.0322)
log(Income) if >0, = 0 otherwise	20.75*** (2.500)	-0.0383*** (0.00811)	0.000248 (0.000921)	0.000557 (0.000615)	-0.00278 (0.00253)
Debt <=0 (binary)	48.03*** (12.97)	-0.0611* (0.0368)	-0.00186 (0.00412)	-0.00204 (0.00310)	0.0230* (0.0129)
log(Wealth) if >0, = 0 otherwise	5.690*** (1.578)	-0.0146*** (0.00383)	-0.000106 (0.000419)	-0.000122 (0.000320)	-0.00237* (0.00126)
State-level covariates:					
Median Income	-13.77 (29.33)	0.0865** (0.0419)	0.00370 (0.00410)	0.00311 (0.00298)	-0.00357 (0.0102)
Unemployment rate	-6.254 (17.45)	0.0209 (0.0288)	-0.000674 (0.00303)	-0.00109 (0.00199)	0.00786 (0.00753)
Population in prison (%)	-1122.8 (17699.5)	35.13 (41.77)	-3.106 (2.424)	-3.098* (1.686)	-6.500 (6.365)
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Observations	52111	43877	51582	51601	52449

Notes: Table contains IV FE estimates of versions of equation (2) with various outcomes on which we might expect no impact of police militarization, at least for certain subpopulations. In all models, the primary independent variable of interest is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local law enforcement agencies (in millions of USD) within the two-year time period, and its interaction with the black head of household dummy. All models control for state and year fixed effects. Standard errors are clustered by household. Column 1's dependent variable is a household's monthly car payment (in USD). Column 2 uses a measure of self reported health. Columns 3 and 4 use self-reported identification as Christian, with column 3 using a fixed effects (FE) specification and column 4 using a random effects (RE) specification. Column 5's dependent variable is a binary indicator of whether a household reports any money spent on regular bus or train fares in the past year. Significance: * 10 percent; ** 5 percent; *** 1 percent.

Table 5: Placebo Tests

Model: Dependent variable: Stage:	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		
	Original Total donations Second	Value of equipment First	Omitting 2013 wave Total donations Second	Omitting 2013 wave Value of equipment First	Narrower Main Effect Total donations Second	Value of "weaponized" equipment First	Narrower Main Effect Total donations Second	Omitting 2013 wave Value of equipment First	Narrower Main Effect Total donations Second	Value of "weaponized" equipment First	Omitting 2013 wave Value of equipment First	Narrower Main Effect Total donations Second	Rolling sum of equipment value Total donations Second	Rolling sum of equipment value Total donations Second	Rolling sum of equipment value First		
Endogenous covariates:																	
Value of equipment	-20.33* (11.92)		-22.82* (12.71)														
x Black	-34.97*** (7.302)		-41.24*** (9.446)														
Value of "weaponized" equipment																	
x Black																	
Rolling sum of equipment value																	
x Black																	
Instruments:																	
Federal funding		0.0004475*** (0.0000180)		0.0004475*** (0.0000223)													
x Black		-0.0000740*** (0.0000279)		-0.0000746* (0.0000404)													
Individual-level covariates:																	
Age	-42.87 (30.62)	-0.0129 (0.305)	-43.26 (34.01)	-0.267 (0.350)	-44.98 (35.84)	-0.0118 (0.305)	-43.29 (28.26)	-0.283 (0.349)	-0.0118 (0.305)	-43.29 (28.26)	-0.283 (0.349)	-43.29 (28.26)	-0.283 (0.349)	-43.29 (28.26)	-0.283 (0.349)	-43.29 (28.26)	-0.283 (0.349)
Education (years)	36.40** (17.05)	0.377*** (0.121)	34.92* (18.90)	0.376** (0.172)	40.80** (18.99)	0.308*** (0.117)	31.14* (16.40)	0.652** (0.258)	0.308*** (0.117)	31.14* (16.40)	0.652** (0.258)	31.14* (16.40)	0.652** (0.258)	31.14* (16.40)	0.652** (0.258)	31.14* (16.40)	0.652** (0.258)
Income ≤ 0 (binary)	2082.7*** (234.7)	1.787 (1.700)	1967.7*** (280.9)	2.084 (1.980)	2068.4*** (254.7)	1.679 (1.680)	2094.2*** (226.7)	3.808 (2.691)	1.679 (1.680)	2094.2*** (226.7)	3.808 (2.691)	2094.2*** (226.7)	3.808 (2.691)	2094.2*** (226.7)	3.808 (2.691)	2094.2*** (226.7)	3.808 (2.691)
log(Income) if >0, = 0 otherwise	231.5*** (23.39)	0.243* (0.140)	222.3*** (27.30)	0.274* (0.166)	232.4*** (25.11)	0.228* (0.137)	225.8*** (22.26)	0.154 (0.218)	0.228* (0.137)	225.8*** (22.26)	0.154 (0.218)	225.8*** (22.26)	0.154 (0.218)	225.8*** (22.26)	0.154 (0.218)	225.8*** (22.26)	0.154 (0.218)
Debt ≤ 0 (binary)	633.5*** (80.98)	-0.525 (0.629)	610.2*** (93.54)	-1.078 (0.723)	615.9*** (87.12)	-0.525 (0.617)	625.5*** (79.05)	-3.099*** (0.962)	-0.525 (0.617)	625.5*** (79.05)	-3.099*** (0.962)	625.5*** (79.05)	-3.099*** (0.962)	625.5*** (79.05)	-3.099*** (0.962)	625.5*** (79.05)	-3.099*** (0.962)
log(Wealth) if >0, = 0 otherwise	74.10*** (9.623)	-0.151*** (0.0704)	71.66*** (11.37)	-0.236*** (0.0864)	71.42*** (10.39)	-0.150*** (0.0691)	74.04*** (9.398)	-0.469*** (0.113)	-0.150*** (0.0691)	74.04*** (9.398)	-0.469*** (0.113)	74.04*** (9.398)	-0.469*** (0.113)	74.04*** (9.398)	-0.469*** (0.113)	74.04*** (9.398)	-0.469*** (0.113)
State-level covariates:																	
Median Income	281.1** (113.0)	5.942*** (0.405)	377.3*** (143.2)	8.004*** (0.527)	443.6*** (161.2)	6.883*** (0.386)	173.4*** (84.05)	6.327*** (0.561)	6.883*** (0.386)	173.4*** (84.05)	6.327*** (0.561)	173.4*** (84.05)	6.327*** (0.561)	173.4*** (84.05)	6.327*** (0.561)	173.4*** (84.05)	6.327*** (0.561)
Unemployment rate	158.8*** (75.58)	6.091*** (0.287)	217.5*** (92.22)	7.084*** (0.333)	258.9*** (112.4)	5.851*** (0.284)	47.82 (35.13)	7.530*** (0.337)	5.851*** (0.284)	47.82 (35.13)	7.530*** (0.337)	47.82 (35.13)	7.530*** (0.337)	47.82 (35.13)	7.530*** (0.337)	47.82 (35.13)	7.530*** (0.337)
Population in prison (%)	575.18.2 (73656.7)	5763.5*** (400.4)	302456.8** (143722.9)	11503.9*** (649.4)	236616.6* (128794.8)	7162.2*** (384.4)	-14871.5 (54810.5)	9609.1*** (794.1)	7162.2*** (384.4)	-14871.5 (54810.5)	9609.1*** (794.1)	-14871.5 (54810.5)	9609.1*** (794.1)	-14871.5 (54810.5)	9609.1*** (794.1)	-14871.5 (54810.5)	9609.1*** (794.1)
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	52643	52643	43954	43954	52643	52643	43954	43954	52643	52643	52643	52643	52643	52643	52643	52643	52643

Notes: Table contains IV FE estimates and corresponding first stage estimates of equation (2) that help verify instrument validity. The primary independent variable of interest is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local law enforcement agencies (in millions of USD) within the two-year time period, and its interaction with the black head of household dummy. All models control for state and year fixed effects. Standard errors are clustered by household. Column 1 reproduces the main model from Table 2 for easy comparison; column 2 contains corresponding first stage estimates for the non-interacted term. Column 3 omits the final time period (2012-2013) during which earmarks were abolished; column 4 contains corresponding first stage estimates for the non-interacted term. Column 5 replaces the usual key independent variable (the state-level aggregate value (list price) of equipment transferred from the DOD to state and local police agencies) with a more narrow aggregation of equipment that is strictly limited to weapons, ammunition, and assault machinery (all transfers with National Stock Number (NSN) Federal Classification System (FCS) 1xxx); column 6 contains corresponding first stage estimates for the non-interacted term. Column 7 reports results from an alternate form of the endogenous variable; here the primary independent variable of interest is the state-level aggregate value (list price) of equipment transferred from the DOD to state and local police agencies (in millions of USD), aggregated on a rolling basis across all years of observation. Column 8 contains corresponding first stage estimates for the non-interacted term. Significance: * 10 percent; ** 5 percent; *** 1 percent.

Table 6: IV Validity Tests