Public Safety, Private Harm: The Impact of Police Militarization on Mortality and Suicide *

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Abstract

We quantify the impact of transferring productivity-enhancing military surplus equipment to law enforcement on suicide and mortality in the United States. Our strategy relies on federal budget allocations to military within a state to instrument for the value of equipment transferred to law enforcement within the state. We find evidence that the average state-level annual transfer of surplus military equipment to local law enforcement agencies (about $2 million) reduces suicide rates by 0.28 standard deviations. The majority of the reduction in suicide rates stems from a reduction in firearm suicide rates, suggesting more effective police forces reduce the need for households to secure their own property with firearms. For robustness, we show our results do not change in consideration of an alternative instrument or different measures of militarization, are robust to concerns about the timing of transfers and simultaneity, and that our strategy does not spuriously explain mortality from causes of death unrelated to public safety.

JEL classification: H56; H51; H72; H76

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1 Introduction

The economic approach to suicide has a long history, going back to at least Hamermesh and Soss (1974). In response to increasing rates of suicide in the United States, economists have recently renewed their interest in understanding the determinants of suicide, with greater emphasis on empirical studies. Suicide rates have increased dramatically in the U.S. over the last two decades, as can be seen in Figure 1. The overall suicide rate increased by nearly 25% from 1999 to 2014, and increased for both men and women (**?**).

There has been notable work on the impact of public investment on suicide rates (for example, Minoiu and Andres (2008) and Ross et al. (2012)), however there has been relatively little work done on the impact of public security on suicides. In this paper, we explore the mortality and suicide consequences of one particular aspect of investment in policing: police militarization. Using data on the transfer of productivity-enhancing military surplus equipment to law enforcement via the 1033 Program, we consider the impact of a more capital-intensive police force on suicide and mortality. At the same time as the noted increase in suicide rates, the transfer of excess military equipment to local police forces has increased dramatically (see McQuoid and Haynes Jr. (2018)), as is documented in Figure 1.

In the present work, we strive to uncover any causal relationships that may be present. We proceed by searching for a plausible source of exogenous variation in surplus military equipment transfers to law enforcement agencies (LEAs). A source of exogenous variation will permit us to make causal statements about the impact of military surplus transfers to law enforcement agencies on well-being, as measured by suicide rates. We use federal budget allocations to the military as an instrument as such allocation decisions are made years in advance of the actual disbursal, and are primarily related to military equipment cycles and international security objectives. As such, the military budget allocations are unlikely to be determinants of well-being within the state, and will therefore satisfy the exclusion restriction necessary for a valid instrument.

There are several important conclusions that can be drawn from our empirical analysis. First, we find that OLS estimates suffer from significant bias, as they fail to detect any effect of enhanced

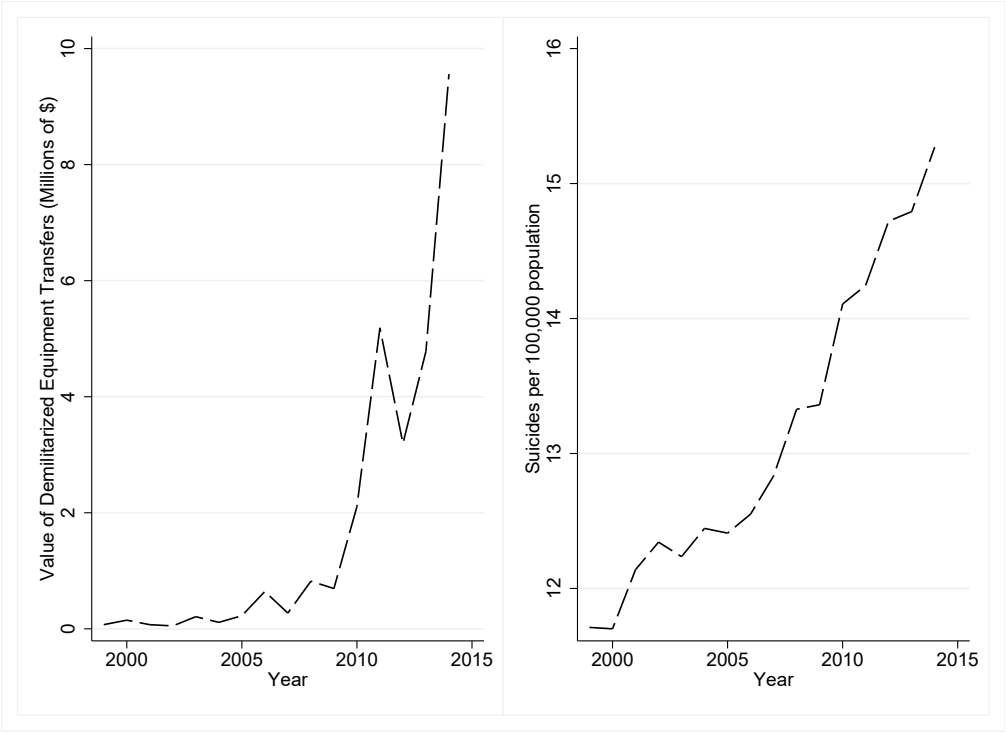

Figure 1: Variation in demilitarized equipment transfers and suicide rates over time, average across states in given year. Notice the acceleration in both variables shortly after 2005.

public safety on suicide within the state. Second, with use of a plausibly exogenous instrument, we find a significant public safety effect of police militarization. On average across states, each year there are $2 million of military surplus transfers to a state and this reduces suicide by $0.28\sigma_w$, where σ_w represents the within-state standard deviation in suicide rates.

When we decompose this into effects on firearm suicide rates versus non-firearm suicide rates, we find that most of the reduction in suicide rates is occurring on account of fewer firearm suicides. For the same average transfer of $2 million of military surplus equipment, firearm suicide rates go down by $0.33\sigma_w$ for males and $0.4\sigma_w$ for females. The same average transfer reduces non-firearm suicide rates in males and females by $0.09\sigma_w$ and $0.19\sigma_w$ respectively. Our results are robust to alternative specifications, various error term assumptions, alternative instruments, and placebo tests, as discussed in our. In addition, we provide possible mechanisms consistent with the totality of evidence presented.

The remainder of this paper is organized as follows. Section 2 reviews related literature on

suicide, public health, and police militarization. Section 3 discusses the data we use to measure police militarization and mortality. Section 4 formalizes our empirical strategy, discusses results, and conducts a sequence of robustness checks to demonstrate the strength of our result in light of potential criticisms. Section 5 concludes.

2 Background

2.1 Economics of Suicide

While the theoretical approaches of Hamermesh and Soss (1974), and later Marcotte (2003) and Becker and Posner (2004), utilize a purely individual rational framework, more recent empirical work has focused on embedding individual decisions within a broader social fabric, as in Daly et al. (2011) and Daly et al. (2013). Cutler et al. (2001) studies youth suicide determinants through the lens of signaling and contagion, where suicide is impacted by peer group pressures and often is a signal for help.

Suicide attempts are often linked to impulsive behavior, and when unsuccessful, subsequent attempts are uncommon (see for example, Seiden (1977), Simon et al. (2002), Rich et al. (1986), Chapdelaine et al. (1991), and Peterson et al. (1985)). These findings suggest that if public interventions are able to reduce suicide attempts, suicide rates would likely decline as follow up attempts are far less likely. A more robust public intervention in the form of a more effective police force would thus be expected to reduce (successful) suicide rates.

In a well-known finding, Seiden and Spence (1984) analyze data from suicide patterns at the Golden Gate Bridge and the Oakland Bay Bridge, and found that differences were related to symbolic factors as well as availability. A more effective public safety force could thus reduce the prevalence of suicide rates by reducing opportunities at public and symbolic locales. If suicide was purely rationally, this would like just lead to substitution into more private methods. However, if suicide is less rational, we would expect a decline in overall suicide rates rather than perfect substitution into alternative methods.

2.2 Public Investment and Public Health

While evidence on the direct relationship between police and suicide is sparse, there is more robust work on the relationship between public investment more broadly defined and suicide. Minoiu and Andres (2008) look at the share of U.S. state spending on welfare and health, and find greater public investment leads to reduced suicide rates. Further, they find that the impact is larger for male suicide than female suicide. On the other hand, Ross et al. (2012) find that state-specific spending on mental health programs have no statistically significant effect, and suggest alternative public policy programs related to income support and socio-economic stability would be more effective.

At the international level, Antonakakis and Collins (2014) show that general fiscal austerity in Greece was associated with higher suicide rates, although the effects are age and gender-specific. Antonakakis and Collins (2015) find similar evidence for five periphery euro-zone countries. Matsubayashi and Ueda (2011) look at differences in national suicide prevention programs across a panel of 21 OECD countries, and find a notable reduction in suicide rates from increased government intervention, suggesting that public policy plays an important role in reducing suicides.

The impact that public safety can have on suicide is related to a broader relationship between confidence in social structure and individual well-being. Bjørnskov et al. (2010) study the relationship between formal institutions and happiness across countries, and find that for middle-income and rich countries, higher quality political institutions result in higher reported measures of happiness and quality of life. Yamamura et al. (2012) study the relationship between corruption and suicide rates in a panel of OECD countries, and find that lower corruption is correlated with lower suicide, with the effect larger for males. Fischer and Rodriguez-Andrs (2008) use Swiss cantons as a case study to consider the extent to which government structure impacts well-being and suicide, and find that fiscal decentralization plays an important role. Further, they find that the effect is driven by local budgetary control, rather than specific health spending.

Taken together, these results suggest that a broader understanding of the role of government in reducing suicide is warranted, and that investment in public safety could be an important channel.

2.3 Police Militarization and Selection Bias

Although the 1033 program, and its predecessor the 1208 program, has been active for over two decades, only recently has its impacts been studied carefully. No doubt, recent academic interest in the 1033 program followed from the events in Ferguson, MO and subsequent media attention. Early work on the transfer of military equipment focused on descriptive analysis or historical studies, along with anecdotal evidence such as Balko (2013).

More careful empirical analysis, including attempts at causally identifying impacts of police militarization have been undertaken by McQuoid and Haynes Jr. (2018), Masera (2016), Bove and Gavrilova (2017), and Harris et al. (2017), who find that police militarization has reduced violent crime, increased drug arrests, and reduced citizen complaints, consistent with the view that more militarized police are more effective police forces. The developing consensus and early evidence is thus that police militarization has notably increased the capability and effectiveness of police forces.

Insler et al. (2018) consider aspects of civic engagement, including charitable giving and volunteering, and find that police militarization has a socially fragmenting effect, reducing civic engagement by black households and weakly increasing white household engagement. Justifying these papers emphasis on identification, Ajilore (2016) finds that police forces participating in the 1033 program differ in important observable ways, suggesting they differ in unobservable ways as well.

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 health concerns. The spatial variability of federal military spending is related to historical circumstances, as documented by Braddon (1995), and generates notable differences in military engagement across localities. As our first stage result presented below suggest, federal military spending is highly correlated with participation in the 1033 program, but as we argue unlikely to

be directly impacting suicidal ideation after controlling for economic and social forces.

Our identification strategy relies on federal budget allocations to the military within a given state as an instrument for military surplus transfer to law enforcement agencies within the same state. Such allocation decisions are made years in advance of the actual disbursal, and are primarily related to military equipment cycles and international security objectives. Our first stage represents an "information channel" by which the federal budget allocation to military within a state makes military culture more present within the state, and therefore makes the availability of productivity enhancing surplus equipment for law enforcement agencies more explicit. Our second stage captures the causal net impact of a more capable and effective police force on suicide rates.

Our usage of federal military spending for identification for state level outcomes is reminiscent of Nakamura and Steinsson (2014), Hooker and Knetter (1997), Barro and Redlick (2011), while Creasey et al. (2015) use a conceptually related approach based on variation in military foreign aid to study nation building and growth across countries.

2.4 Theoretical Mechanisms

Although we cannot identify the exact channel, the totality of our results suggest some plausible mechanisms at work. As shown in McQuoid and Haynes Jr. (2018), Masera (2016), Bove and Gavrilova (2017), and Harris et al. (2017), police militarization leads to a more effective and capable police force, significantly reducing violent crime. Those papers hypothesize two possible channels through which militarized police forces reduce violent crime, a deterrence effect and a capability effect.

Exposure to violence and crime has been identified as a cause of depression, anxiety, stress, and risky behaviors that spur suicidal ideation, as documented in Singer et al. (1995) as well as Mazza and Reynolds (1999). At the same time, Benjamin et al. (2014) establishes that feelings of safety and security correlate highly with subjective measures of well-being, introducing scope for policing to impact well-being. We hypothesize that as law enforcement agencies become more capable as a result of adopting military equipment, they are better able to promote a sense of

security and stability within a community by reducing violent crime and exposure to violent crime. A safer and more stable community may thus reduce the prevalence of emotional states linked to suicidal ideation.

In addition to the actual reduction in crime, the deterrence effect may increase the sense of security of citizens even in the absence of a reduction in crime. By projecting power, militarized police may make citizens feel safer regardless of the actual level of crime. Given that Americans systematically overstate the true prevalence of crime in the U.S., as described in Doherty et al. (2016), anxiety and stress may be reduced simply by reducing fear of crime. Although "security theater" (Schneier (2006)) is often ridiculed as wasteful when it does little to reduce objective risk, these critiques ignore the mental health benefits associated with reduced stress and anxiety as subjective fear of crime declines. Thus, if the ties between feelings of physical security and safety and self-reported measures of well-being found by Benjamin et al. (2014) do represent a causal channel, enhancing policy productivity with capital transfers has the potential to increase social welfare.

Additionally, our results suggest that the biggest impact on suicide comes from reduced firearm suicide. Evidence in McDowall and Loftin (1983) and Guha (2013) suggests if a more capable police force is present in a community, the need for privately owned guns for protection or sense of security may decline. Since gun availability and suicides have been directly linked (see for example,Balestra (2018), Lang (2013), Edwards et al. (2018), and Vitt et al. (2018))), by reducing the need for firearm ownership, firearm suicides would be expected to decline. Additionally, a more efficient police force likely will reduce the supply of illegal firearms, which may similarly be used to commit suicide.

Finally, our findings of reduced suicide rates in response to a more militarized police force are consistent across genders. In most studies on suicide determination, gender differences are notable, and the impact is mostly for males (as in Blair-West et al. (1999)). For increased police militarization, however, the relative decline in suicide rates is very similar for both men and women. This suggests that the mechanism at work is a very general one, such as reducing exposure to

violence or increasing the sense of security, which is common to both genders.

2.5 Department of Defense 1033 Excess Supply Program

The variable of interest in our study is a measure of militarized police. This measure is constructed with data covering the entirety of equipment transferred to local law enforcement agencies from the Department of Defense (DoD) as provisioned by the 1033 Excess Equipment program. The prior surplus equipment program, known as 1208, was replaced by the 1033 program under the National Defense Authorization Act for Fiscal Year 1997. Starting with the 1033 program, any bona fide law enforcement activity justified a request for surplus military equipment.

Oversight of the process for requesting equipment occurs at the state level. The state, in order to benefit from the 1033 program, must create a Memorandum of Agreement (MOA) with the Defense Logistics Agency (DLA), the supply agency of the Department of Defense. The state then appoints a DLA State Coordinator who oversees the program within the state and ensures proper use of the equipment transferred under 1033. With these provisions in place, Law Enforcement Agencies (LEAs) inside the state are eligible to apply to participate in 1033. All states currently have a MOA with the DLA. With approval by the DLA and the State Coordinator, representatives from state LEAs may visit a Disposition Services Center or a DLA website to see the equipment available. A request by the LEA is prepared for the desired equipment, along with a justification for why the equipment is needed. The request is reviewed by the State Coordinator, and then by the DLA Law Enforcement Support Office for final approval.

Nearly $2 billion of equipment has been transferred from the Department of Defense to various local, state, and federal law enforcement agencies since the creation of the 1208-1033 program. LEAs are required to utilize all equipment transferred via 1033 within one year, and are required to submit proof of possession in the form of pictures and serial numbers of all equipment transferred that is valued over $20,000 or requires special demilitarization. All equipment not meeting these last criteria are assigned a demilitarization code A, and do not require reporting after the initial year of ownership. For equipment valued over $20,000, or equipment that had to undergo special

demilitarization prior to 1033 transfer, LEAs never receive de jure ownership.

A key feature of the 1033 program is accountability. Due to the overwhelming power of some of the special demilitarized equipment, the DLA maintains meticulous records of all DoD equipment transferred to LEAs via the 1033 program. To maintain transparency, the DLA provides a roster of all equipment transferred from the DoD to LEAs since 1990. The record is organized by state or territory, and includes the specific agency receiving the item, the exact equipment transferred, the quantity transferred, the value of the equipment at time of DoD purchase, when it was transferred, and whether the equipment requires special demilitarization or return to the DLA.

As an example record for such transfers, the state of Florida received 8 Mine Resistant Ambush Protected vehicles (MRAPs, national stock number 2355-01-553-4634), with a total value of $5.26 million dollars between August 29, 2013 and October 15, 2013. These vehicles were designed to more adequately protect service members from improvised explosive devices, small arms fire, and land mines. MRAPs carry a DEMIL code "C" so they must be returned to the DLA for further demilitarization when no longer in use. Not all equipment transferred under the 1033 program is special military equipment like MRAPs. The roster of transfers includes a large amount of DEMIL code "A" items, one example being the transfer of 60 pairs of "Underwear and Nightwear, Mens" (national stock number 8420-DS-MUN-DERW) to the Summit City Sheriff Department in Ohio.

While there has been more than $2 billion of equipment transferred to LEAs through the 1033 program, slightly more than 17% of it is equipment with DEMIL code "A". Examples of items with code "A" include shredders, guitar amplifiers, all-terrain vehicles, pliers, brooms, underwear and various clothing. The overwhelming majority of equipment carried DEMIL codes B-Q, requiring return to the DLA for demilitarization. The most expensive equipment on the roster are various aircraft: search and rescue helicopters, cargo-transport airplanes, etc. Participation in the 1033 program is a choice for LEAs. Figure 2 displays total value of all transfers from 1990-2014 for each state via the 1033 program. All states have benefited from the 1033 program transfers. Florida clearly behaves as an outlier, with nearly $300 million of transfers from 1990 to 2014. On the other end of the spectrum are states like Alaska (AK on Figure 2), with a sum total value of surplus

equipment transferred of $793,926.81.

3 Data

First, we define a measure of militarization of police forces. We follow McQuoid and Haynes Jr. (2018) in this regard. We rely on the DLA roster of all items transferred from the DoD to law enforcement agencies in all 50 states, from 1990 to 2014. A unit of militarization is a dollar of military equipment provided to LEAs by the DLA in a given state and year. We only consider the value of equipment with demilitarization codes B,C,D,E,F,G, and Q. These demilitarization codes are associated with equipment that requires "special demilitarization" and must be returned to the DLA when not in use. Due to the special nature of this equipment, we believe these categories represent the highest potential for enhancing LEA capability.

Suicide rates by specific method and by demographic group are from the Center for Disease Control's mortality records. Specifically, we use the Public-Use files for Multiple-Cause-of-Death (MCD) records. Data are drawn from all death certificates filed in the given state in the given year. Causes of death are classified according to International Classification of Disease 10th edition (ICD-10) standards. Since our focus is on self harm, we rely on the Intentional Self Harm codes along with some other internal causes of death for robustness checks. CDC suicide data has been relied on for a variety of economic investigations, ranging from international trade in Pierce and Schott (2016) and McQuoid and Vitt (2017), to pain epidemics in Case and Deaton (2015), to gun policy in Vitt et al. (2018).

The MCD data, in addition to reporting causes of death, report demographic variables of interest like age, sex, and race. We use the mortality data to construct the total number of suicide deaths in a state for each year, as well as to partition suicides by firearm or non-firearm suicide. Partitioning suicides deaths in this way allows us to investigate whether any possible safety effects of police militarization have differential effects on types of suicide.

We collect additional state and year controls in order to account for additional social and

economic factors that could be correlated with suicides and our instrument. If these factors are indeed correlated with suicide rates and our instrument, failure to include them would bias our instrumented estimates. Demographic data such as median income, the percentage of the population between 18-24, population estimates, and the share of the population with veteran status are sourced from the U.S. Census Bureau. Data on state unemployment rates is sourced from the U.S. Bureau of Labor Statistics.[1] The control for crime (total crime rate) is taken from the FBI's Uniform Crime Reporting (UCR) program. Summary statistics for all variables are reported in Table 1. After combining all key data sources, our sample encompasses all states (and DC) from 2003-2013.

4 Empirical Analysis

To get an initial sense of the relationship between police militarization and suicide rates, we consider an empirical strategy without instruments in columns 1-3 of Table (2). The primary takeaway from column 1 is that a naive approach that fails to fully exploit the panel data information would conclude that there is no relationship between police militarization and suicide rates. Column 2 estimates the relationship using only variation within a state over time and controlling for linear and quadratic trends in both suicide rates and in police militarization. With concerns over omitted variables, we introduce a parsimonious set of controls in column 3, and would still fail to find sufficient evidence that police militarization has an impact on well-being, as measured by suicide rates.

This initial exercise provides some evidence contrary to the trends presented in Figure (1). To address potential attenuation bias from particular types of measurement error, and to potentially address any selection bias in our sample, we cast aside our results in columns 1-3 of Table (2) and proceed with a more appropriate strategy.

To estimate a causal relationship, we proceed by explaining our instrumental variable strategy.

[1] Andrés and Halicioglu (2010) discusses the relationship between suicide and unemployment, which may be a channel through which government spending could directly impact suicides if government defense spending local employment shocks.

Our preferred specification estimates the change in the suicide rate for a given increase in the value of demilitarized equipment transferred to state law enforcement agencies, while controlling for additional factors that may influence suicide rates and be correlated with our instrument. Our preferred specification is given by:

$$\begin{aligned} \textit{Suicide Rate}_{st} = \beta_0 + \beta_1 \, \widehat{\textit{Militarization}}_{st} + \mathbf{Controls}_{st}\beta_{\mathbf{z}} \\ + \alpha_s + \beta_2 t + \beta_3 t^2 + \epsilon_{st} \end{aligned} \tag{1}$$

where the dependent variable is the number of suicides per 100,000 population in state s for year t. Our variable of interest, $\widehat{\text{Militarization}}_{st}$, is the value (in millions of USD) of all military equipment requiring "special demilitarization" that was transferred to law enforcement agencies in state s during year t. Included in $\mathbf{Controls}_{st}$ are time-varying state characteristics that are possibly correlated with our instrument and with suicide rates. In Eq (1), α_s represents a state fixed effect that accounts for any time invariant determinants of suicide as well as average differences in suicide determinants across panels. One example of such effects would be differences in attitudes towards suicide across geographic borders, as explored in Neumayer (2003).

The second stage represents a path from law enforcement agencies acquiring surplus military equipment to a more capable police force whose productivity is augmented by the new capital, and therefore a greater sense of security or well-being within the state. Time trends are included to account for unobserved national forces driving both suicide rates and demilitarized equipment transfers. The most flexible specification for unobserved common time trends is to use yearly dummies, however this approach is not always efficient. Our goal is to increase efficiency while maintaining flexibility, which we do by imposing a parametric specification in the form of a quadratic time trend to control for common unobserved time effects. Since the aggregate data is consistent with the quadratic trends assumption over time, we make this assumption to improve the efficiency of our estimation (see Greene (1997)).

We note our selection of quadratic trends in (1) over year fixed effects is based on a variety of evidence. First, an information criterion based comparison of (1) with a model including

year fixed effects suggests that the quadratic trends model has comparatively less information loss. Second, we note that a model otherwise identical to (1) with year fixed effects in place of quadratic trends has estimated coefficients on the year dummy variables that are monotonic. The monotonicity and information criteria comparison results together suggest efficiency gains from using quadratic trends in place of year effects. To speak more to identification concerns, in such a specification with year fixed effects, the coefficient on our variable of interest, $\hat{\beta}_1$, carries the same sign and magnitude ($\hat{\beta}_1 \approx -0.24$) as our estimates in the preferred specification (1), with marginal statistical significance ($p = 0.068$). Were there some large unobserved common components driving our result, we would expect both the sign and magnitude of our estimates to differ, but this fails to occur. We attribute the small change in standard errors to the associated increase in the number of parameters. The totality of these circumstances suggests that quadratic trends are a more efficient approach to model estimation in this context.

To address possible concerns of undersized standard errors of our estimates when shocks are correlated at a geographic level, as discussed in Bertrand et al. (2004), we cluster observations at the state level. We view the state level as the most appropriate level for likely correlations in shocks to suicide, although results are robust to alternative assumptions about the correlations in the error term.

Given possible concerns of attenuation bias from measurement error in *Militarization*, and to address concerns of omitted time varying confounding factors, we rely on an instrumental variables strategy. We instrument $\text{Militarization}_{st}$ with the value of federal defense spending allocated to the state. This allows us to isolate exogenous variation in surplus military equipment transfers with the following first stage specification:

$$\begin{aligned} \textit{Militarization}_{st} = \delta_0 &+ \delta_1 \textit{Federal Defense Spending}_{st} \\ &+ \mathbf{Controls}_{st}\delta \\ &+ \alpha_s + \delta_2 t + \delta_3 t^2 + u_{st} \end{aligned} \tag{2}$$

Our first stage represents an awareness effect. Our instrument for $\text{Militarization}_{st}$ is the federal

defense spending on the military in state s during year t. This budget allocation decision is made years in advance of year t and is primarily a function of international objectives and product cycles. Variation in the federal budget allocation over time within a state primarily stems from the need to build and acquire new equipment. As this new equipment is built, it displaces old equipment which enters the 1033 program. We hypothesize that this federal defense spending allocation proxies awareness of military culture in the state as well as awareness of the surplus inventory. As this awareness rises, so too should petitions for transfers of the surplus military equipment to law enforcement agencies.

We argue that federal spending on military within the state, which varies with international objectives and product cycles within the state, influences well-being in the state only through its impact on transfers of surplus equipment to local law enforcement agencies. Any direct effect of federal military spending on income or employment within the state will be captured by the inclusion of median income, the unemployment rate, and the veteran share of population as controls. To mitigate concerns that state size might be correlated with suicide risk and participation in the 1033 program, we include population as a control. Finally, state fixed effects will pick up any state-specific military culture effects that are time-invariant, while time trends will capture changes in national sentiment towards the military.

Regarding the relevance of our instrument, our hypothesis regarding the relationship between federal defense spending and the value of surplus equipment transferred to law enforcement agencies is supported in column 5 of Table (2). The instrument has a statistically significant relationship with the endogenous regressor; the traditional F statistic testing the coefficient on the instrument is zero is itself above 10. This high F statistic suggests that any bias from weak instruments will be minimal.

Armed with a suitable empirical strategy to deal with measurement error and other endogeneity concerns, we are able to paint a clearer picture of the relationship between police militarization and suicide. First, consider column 4 of Table 2, which presents IV estimates. This column includes state fixed effects as well as linear and quadratic time trends, and has a point estimate of -0.2.

The effect is significant at the 95% confidence level.

Interpreting the point estimate in column 4 of Table 2 goes as follows. Consider that the average change in 1033 transfers for a state year over year is \$2 million, and that the standard deviation of suicide rates within a state, denoted σ_w , is 1.47. Using the point estimate in column 4 of Table 2, for a \$2 million transfer, suicide rates decline by $0.28\sigma_w$ ($= \Delta\text{Militarization} \times \hat{\beta}_1 =$ 0.4 deaths per 100,000 residents). This is a notable result, foremost on account of it being two orders of magnitude larger than the non-instrumented (and insignificant) result in column 3. Additionally, this result stands in defiance of the trend in Figure 1, reinforcing the need for a credible identification strategy that addresses confounding factors. Lastly, it suggests that the demilitarized capital increases the productivity of police forces, making them more capable at increasing feelings of security, and that the resulting increase in well-being from these feelings are significant enough to be measurable in outcomes like suicide rates.

We proceed by considering the robustness of this result when further disaggregating along demographic lines. In Table 3, we consider whether the effect is present for both males and females. Column 1 estimates the effect of police militarization on the male suicide rate, while column 4 estimates the effect of police militarization on the female suicide rate. Both are significant and negative, suggesting that both sexes benefit from the increased productivity of the police force measured by militarization. This finding is in contrast to most of the literature that considers public policy interventions on suicide rates, which typically finds greater effects for males. For the average annual increase in 1033 transfers, male suicide rates decline by $0.28\sigma_w$, while female suicide rates decline by $0.37\sigma_w$, using the σ_w from Table 1 that corresponds to the suicide rates for these subsamples and methods of suicide.

Columns 2/5 and 3/6 of Table 3 compare the firearm and non-firearm suicide rates for males and females respectively. For the average annual 1033 transfer to a state, male firearm and non-firearm suicide rates decline by $0.33\sigma_w/0.09\sigma_w$ respectively, while female suicide rates decline by $0.4\sigma_w/0.19\sigma_w$ respectively. The takeaway from these columns is the majority of the reduction in suicide rates is coming from a reduction in firearm suicides rather than from a reduction in non-

firearm suicides. One channel possibly qualifying this reduction in firearm suicides is the possibility that increasingly capable police forces reduce the need for households to purchase weapons to secure their personal effects. With increasing accumulation of safety capital via 1033 transfers, households are able to better rely on police forces to keep their property secure. Decreased availability of firearms as a result of this possibility would also mean fewer opportunities for a rash decision like suicide by firearm, as explored in Vitt et al. (2018), citetgriffin, or Balestra (2018).

Lastly, we consider the safety effect for white males and white females. Motivation for focusing on disparities between white males and white females follows from the differential trends in suicide rates among white males, as explored in Case and Deaton (2015). Table 4 presents results for overall suicide rates, firearm suicide rates, and non-firearm suicide rates for working-age white males and white females. Given the average annual 1033 transfer of $2 million of equipment, white male suicide rates, firearm suicide rates, and non-firearm suicide rates decline by $0.3\sigma_w$, $0.4\sigma_w$ and $0.1\sigma_w$ respectively. For white females, the impact on suicide rates, firearm suicide rates, and non-firearm suicide rates are $0.35\sigma_w$, $0.3\sigma_w$, and $0.38\sigma_w$, respectively.

4.1 Robustness Checks

In this section, we consider a sequence of alternative approaches to verify the robustness of our findings. First, we show that our instrumental strategy is meaningful and is consistent with our general mechanism by demonstrating our results do not spuriously replicate when considering an alternative cause of mortality. Then, we provide further evidence to increase confidence of the excludability of our instrument by showing that our results are robust to using similar instruments that circumvent a particular excludability concern. We then show that our results are not sensitive to concerns regarding timing of the transfer: contemporaneous and lagged transfers yield effectively identical results. In addition, we explore alternative measures of militarization including all transfers, not just special demilitarized equipment, and use a count rather than value measures of military equipment transferred. Results are qualitatively and quantitatively similar using these alternative measures. Finally, we demonstrate that our results are generally robust to different

assumptions regarding the variance of errors.

One concern might be that a time-varying, state-specific effect such as the capability of state and local government is driving both the ability to capture federal funding and public health investments that impact suicide rates. First, note that since we include state fixed effects, national time trends, and numerous socio-economic state-level controls, this concern about state capability would have to vary in ways independent of these forces. Furthermore, as discussed above, our instrument of federal military spending at the state level is set years in advance while our policy variable of interest (police militarization) is estimated to be contemporaneoulsy impacting suicide rates, so the nature of this type of "state capability" would have to vary over time, but with an appropriate time lag consistent with the observaed variation in military transfers (which both increase and decrease from year to year).

While we consider the likelihood of this kind of state capability to be extremely low, such an evolution of state capability is not impossible. If this type of time-varying state capability was correlated with the capability of health institutions and the value of 1033 transfers, then we would expect this state capability to impact other dimensions of health that are influenced by public health institutional efficacy. Mortality from acute myocardial infarction (heart attack) is driven in large part by the capability of health institutions, in the sense that mortality from this specific cause of death is primarily a function of access to and distance from hospitals, as documented in McClellan et al. (1994) or Buchmueller et al. (2006). If such a (lagged) time-vary, state specific confounding factor like government capability were to exist, one would expect to find a correlation between mortality from heart attacks and the value of 1033 transfers.

In Table 5, we estimate (1), but replace Suicide Rate$_{st}$ with the number of acute myocardial infarction deaths per 100,000 citizens. Column 1 presents results without an instrumental variable, and column 2 uses the first stage outlined in (2). In comparing these columns, we note that there is no estimated impact of police militarization on heart attack death rates. The first stage is identical to the previous investigations, and the high F-statistic reinforces the strength of the first stage. This combination of a strong first stage and a lack of a significant effect of militarization

on heart attack mortality rates provides evidence against some sort of time-vary, state specific confounding effect related to capability of government and reinforces the quality of our instrument and strategy.

We further validate our results by considering an alternative instrument for variation in the value of 1033 transfers to law enforcement agencies in the state. One motivation for doing so is to alleviate any fleeting concerns that there is a channel through which military presence in the state increases the feeling of security and safety within the state beyond that accomplished by the presence of law enforcement agencies. We do not feel this is a compelling story: LEAs, not the Marine Corps, enforce property rights. It is neither the Coast Guard nor the Army who respond when someone is trying to forcefully make your possessions their possessions, response comes from LEAs. Mean differences in perceived safety from our armed forces are captured in state fixed effects, while any trends in safety from this channel are likely to be identical across states and picked up by our quadratic trend.

Nonetheless, to alleviate any possible concerns we consider a different instrument. Variation in budget allocations for military in states other than s will not directly make the military more present in state s. Budget allocations for the military in other states are related to military equipment cycles in those other states: they fund new equipment, the new equipment displaces old equipment that becomes surplus. Furthermore, there is no reason to believe federal budget allocations towards military in other states will have an impact on perceived safety in a given state.

Table 6 presents the results from using an alternative instrument and additional measures of militarization. Column 1 presents the effect on population suicide rates with the original instrument, while column 2 presents the same second stage regression with the new instrument in the first stage. First, we note that the alternative instrument is strong by conventional F test rules of thumb, so bias from weak instruments is inconsequential. Second, we note the takeaway is the same: police militarization reduces suicide rates in the overall population. Using this instrument, for the average 1033 transfer each year, population suicide rates decline by $0.39\sigma_w$, which is not

significantly different from the estimate using our original instrument (p=0.43).

Current investigations on the effects of transferring surplus equipment to law enforcement agencies on crime, for instance Harris et al. (2017) and Bove and Gavrilova (2017), use lagged values of surplus military equipment transfers in place of contemporaneous values. The standard justification is an appeal to avoiding simultaneity bias: acquisition of capital for law enforcement depends on crime, while crime may depend on the productivity of the police force, and lagging the explanatory variable may help to correct for this problem. We feel that since our dependent variable is not a measure of crime, we are likely not subject to the same simultaneity bias. Nevertheless, in column 3 of Table 6 we estimate a specification similar to (1) with all variables on the right hand side being lagged a single period. This replicates the simultaneity adjustment in previous work. We note that the sign and magnitude of our result in this check, along with the strength of our instrument, is entirely comparable to any of the other specifications with the same dependent variable (e.g. columns 1-2 of the same table, column 4 of Table 2, or the entirety of Table 7 discussed below.)

In columns (4)-(6) of Table 6, we consider alternative measures of militarization. In column (4), we include the value of all transfers, including those that do not require special demilitarization (code A). We note that the sign and magnitude of this estimate is very comparable to the estimates in column (1). In columns (5) and (6), we measure militarization as the number of units transferred, similar to the count model in Harris et al. (2017) and Bove and Gavrilova (2017), and note that the sign of our estimates are unchanged. We believe that the magnitude difference between columns (5) and (6) is explained by the differential productivity of the equipment in codes B-Q. The lower point estimate in (5) relative to (6) suggests that a unit of men's underwear and a unit of MRAP do not contribute equally to police capability. Notice that, upon excluding code A equipment in column (6), the magnitude of the partial effect increases.

As a final robustness check, we focus on ensuring that our statistical significance is not being driven by assumptions regarding the error term or outliers. Table 7 presents estimates of our preferred specification with a variety of ways to estimate the standard errors of point estimates.

Columns 1-3 present conventional, White (1980), and regional clusters for the standard errors respectively. In column 4, we drop Florida, who is a significant outlier in terms of military transfers as demonstrated in Figure 2. We note that the significance of our finding is largely stable across these differing standard error assumptions, and that our point estimates are not being driven by Florida acting as an outlier.

5 Conclusion

Current empirical research on suicide has mostly focused on how economic outcomes have impacted suicide. This research has investigated a new channel, public safety, that acts as a determinant of suicide rates. Our results provided a more credible test of previous research that finds a relationship between self-reported feelings of physical security, security about the future, freedom from injustice, and increased well-being.

Understanding the impact of surplus equipment transfers to LEAs on public safety and well-being is an important social policy question given the renewed focus on the growing social cost of suicide in the United States. A previous lack of rigorous empirical analysis stood in the way of a gaining a clear understanding of the causal connections between the two. Our paper attempts to make this causal impact more clear: an increase in transfers of military surplus equipment makes police more capable in the present period and increases general feelings of public safety and well-being, in turn reducing the appeal of socially costly and impulsive decisions like suicide.

Our strategy relies on instrumental variables in order to address possible omitted variable bias inherent in models of suicide determination. The IV strategy provided an improvement over OLS estimates, and we find that an increase in transfers of surplus equipment to state LEAs via the 1033 program indeed causes a significant and sizable decrease in the rate of suicide within the state. Additionally, we showed that when suicides are partitioned by firearm suicide and non-firearm suicide, that the majority of the reduction in suicide rates from police militarization is through a reduction in firearm suicide. While most studies on suicide find differential policy effects

across genders, our results suggest the public safety channel identified here are comparable across men and women. Future work should consider additional dimensions through which public safety can impact public health.

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Tables and Figures

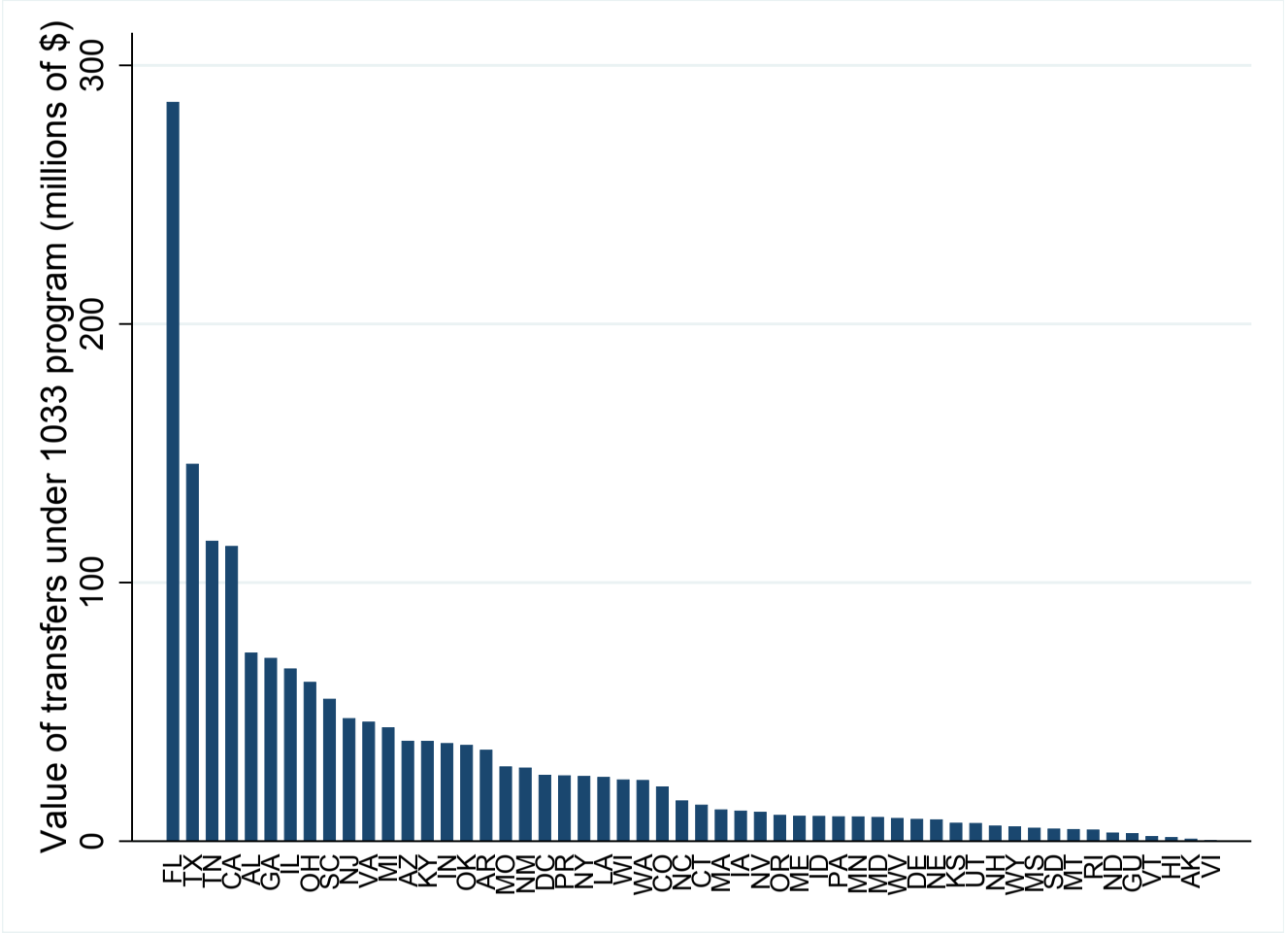

Figure 2: Value of all equipment transferred to each state under the 1033 program in millions of dollars.

Table 1: Summary Statistics

	mean	within std. dev (σ_w)	min	max	p25	p75
Population Suicide Rate	13.14	1.47	4.02	29.67	10.69	15.11
Male Suicide Rate	21.28	2.25	8.51	47.23	17.54	24.44
White Male 20-59 Suicide Rate	29.84	4.31	12.42	61.40	24.44	34.81
Male Firearm Suicide Rate	12.73	1.49	1.93	30.58	9.96	15.96
White Male 20-59 Firearm Suicide Rate	16.678	2.42	3.24	37.88	13.00	20.94
Male Non-Firearm Suicide Rate	8.53	1.35	3.00	18.52	6.90	9.81
White Male 20-59 Non-Firearm Suicide Rate	13.61	4.32	4.77	31.82	11.16	15.62
Female Suicide Rate	5.30	1.00	1.32	12.07	4.10	6.16
White Female 20-59 Suicide Rate	8.60	1.79	2.83	19.91	6.62	10.24
Female Firearm Suicide Rate	1.78	0.48	0	7.10	0.95	2.44
White Female 20-59 Firearm Suicide Rate	3.38	0.74	0.37	10.98	2.14	4.34
Female Non-Firearm Suicide Rate	3.51	0.80	0.64	8.18	2.61	4.19
White Female 20-59 Non-Firearm Suicide Rate	5.70	1.33	2.15	15.55	4.35	6.64
Militarization (code B-Q 1033 transfers, millions of $)	1.76	6.84	0.00	154.79	0.001	0.89
Federal Defense Spending	3489.50	1321.97	0.00	31391.00	861.00	3961.00
Unemployment Rate	5.81	1.73	2.30	13.80	4.30	6.90
Median Income (tens of thousands)	5.47	0.29	0.00	7.86	4.88	6.07
Young Population Share	9.39	0.007	7.22	14.05	8.79	9.97
Total Crime Rate	3683.79	437.49	1946.00	8067.00	2870.90	4353.20
Log Population	15.08	0.05	13.08	17.46	14.27	15.70
Veteran Share of Population	8.67	0.83	4.51	12.50	7.86	9.70

Table 2: OLS and FEIV Regression of Suicide Rates on Militarization

	(1) Population Suicide Rate	(2) Population Suicide Rate	(3) Population Suicide Rate	(4) Population Suicide Rate	(5) First Stage Regression Militarization
Federal Defense Spending					0.000803****
					(0.000219)
Militarization	0.0214	-0.0104	-0.00309	-0.206**	
	(0.0132)	(0.00655)	(0.00310)	(0.103)	
Unemployment Rate			0.0452	0.118	0.294
			(0.0297)	(0.107)	(0.489)
Median Income			-0.0998	-0.162	-0.229
			(0.299)	(0.302)	(0.444)
Young Population Share			0.00393	0.0351	0.0511
			(0.0570)	(0.0684)	(0.130)
Total Crime Rate			0.000454**	0.000228	-0.000781
			(0.000202)	(0.000216)	(0.000604)
Log Population			0.0895	0.795	-0.583
			(3.782)	(4.596)	(11.79)
Veteran Share of Population			0.359*	0.242	-0.927
			(0.213)	(0.336)	(1.270)
Instruments	None	None	None	federal defense spending in state	First Stage
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weak Instrument F				13.46	13.46
Observations	816	816	561	561	561

Standard errors in parentheses. All models (except column (1)) include state fixed effects and a quadratic trend. First stage regression (2) given in column 5.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 3: FEIV regression of suicide rates by gender on police militarization

	(1) Male Suicide Rate	(2) Male Firearm Suicide Rate	(3) Male Non-Firearm Suicide Rate	(4) Female Suicide Rate	(5) Female Firearm Suicide Rate	(6) Female Non-Firearm Suicide Rate
Militarization	-0.318	-0.246	-0.0607	-0.184	-0.0971	-0.0754
	(0.157)**	(0.118)**	(0.0620)	(0.0661)***	(0.0370)***	(0.0434)*
Unemployment Rate	0.227	0.151	0.0692	0.0603	0.0306	0.0237
	(0.170)	(0.129)	(0.0517)	(0.0863)	(0.0469)	(0.0403)
Median Income	-0.0131	0.420	-0.458	-0.275	-0.0562	-0.234
	(0.513)	(0.342)	(0.303)	(0.184)	(0.122)	(0.186)
Young Population Share	0.0208	-0.000641	0.000105	0.0828	0.0347	0.0518
	(0.111)	(0.0889)	(0.0687)	(0.0554)	(0.0367)	(0.0516)
Total Crime Rate	0.000354	-0.000278	0.000593	0.0000955	0.0000202	0.0000812
	(0.000382)	(0.000353)	(0.000324)*	(0.000166)	(0.000110)	(0.0000966)
Log Population	-1.446	-0.914	-1.366	4.878	2.270	2.477
	(7.372)	(5.453)	(2.905)	(3.132)	(1.560)	(2.174)
Veteran Share of Population	0.574	0.373	0.189	-0.198	0.0641	-0.236
	(0.552)	(0.389)	(0.286)	(0.256)	(0.154)	(0.166)
Instruments	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weak Instrument F	12.92	12.92	13.46	12.92	12.92	13.46
Observations	556	556	561	556	556	561

Standard errors in parentheses. All models include state fixed effects and a quadratic trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 4: Second Stage regression of white working age suicide rates on police militarization by gender

	(1) Suicide Rate White Males 20 to 59	(2) Firearm Suicide Rate White Males 20 to 59	(3) Non-Firearm Suicide Rate White Males 20 to 59	(4) Suicide Rate White Females 20 to 59	(5) Firearm Suicide Rate White Females 20 to 59	(6) Non-Firearm Suicide Rate White Females 20 to 59
Militarization	-0.678	-0.513	-0.245	-0.349	-0.128	-0.0484
	(0.282)**	(0.209)**	(0.134)*	(0.113)***	(0.0670)*	(0.0726)
Unemployment Rate	0.680	0.430	0.285	0.230	0.0920	0.0401
	(0.514)	(0.385)	(0.208)	(0.230)	(0.103)	(0.0694)
Median Income	0.603	1.162	-0.493	-0.0601	-0.0463	-0.0197
	(0.894)	(0.603)*	(0.627)	(0.339)	(0.198)	(0.208)
Young Population Share	0.136	-0.0499	0.158	0.168	-0.0444	0.0957
	(0.172)	(0.131)	(0.125)	(0.121)	(0.0640)	(0.0681)
Total Crime Rate	0.0000335	-0.000674	0.000574	0.0000829	0.0000451	0.000577
	(0.000779)	(0.000732)	(0.000491)	(0.000432)	(0.000180)	(0.000207)***
Log Population	1.433	4.219	-3.296	10.36	2.386	9.129
	(10.92)	(7.775)	(5.655)	(4.911)**	(2.218)	(3.393)***
Veteran Share of Population	1.085	0.738	0.347	-0.331	0.210	-0.358
	(0.825)	(0.560)	(0.548)	(0.427)	(0.296)	(0.185)*
Instruments	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weak Instrument F	11.54	10.60	11.54	11.36	6.358	4.845
Observations	552	543	552	540	403	499

Standard errors in parentheses. All models include state fixed effects and a quadratic trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 5: FEIV heart attack death rates on police militarization

	(1) Heart Attack Death Rate	(2) Heart Attack Death Rate
Militarization	-0.0231	0.213
	(0.0143)	(0.400)
Unemployment Rate	-0.0702	-0.155
	(0.136)	(0.211)
Median Income	-1.842	-1.770
	(1.005)*	(1.006)*
Young Population Share	0.169	0.133
	(0.220)	(0.236)
Total Crime Rate	-0.00256	-0.00230
	(0.00146)*	(0.00139)*
Log Population	23.32	22.50
	(18.29)	(18.32)
Veteran Share of Population	2.978	3.114
	(0.740)****	(0.826)****
Instruments	None	federal defense spending in state
Std. Error	State Cluster	State Cluster
Weak Instrument F		13.46
Observations	561	561

Standard errors in parentheses. All models include state fixed effects and a quadratic trend.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 6: FEIV regression of suicide rates on police militarization w/ alternative instruments and measures of militarization

	(1) Population Suicide Rate	(2) Population Suicide Rate	(3) Population Suicide Rate	(4) Population Suicide Rate	(5) Population Suicide Rate	(6) Population Suicide Rate
Militarization	-0.206	-0.290	-0.229	-0.201	-0.053	-0.218
	(0.103)**	(0.140)**	(0.103)**	(0.101)**	(0.026)**	(0.098)**
Unemployment Rate	0.118	0.148	0.136	0.101	0.0116	0.0202
	(0.107)	(0.139)	(0.113)	(0.104)	(0.0328)	(0.0342)
Median Income	-0.162	-0.187	0.014	-0.172	0.124	0.117
	(0.302)	(0.318)	(0.257)	(0.307)	(0.286)	(0.315)
Young Population Share	0.0351	0.0480	-0.029	0.0456	0.0510	0.0519
	(0.0684)	(0.0673)	(0.102)	(0.0746)	(0.0667)	(0.0664)
Total Crime Rate	0.000228	0.000135	0.00008	0.000155	0.000668	0.000545
	(0.000216)	(0.000242)	(0.0003)	(0.000235)	(0.000236)***	(0.000258)**
Log Population	0.795	1.087	1.053	0.561	4.583	3.887
	(4.596)	(5.097)	(4.646)	(4.556)	(4.015)	(4.380)
Veteran Share of Population	0.242	0.194	0.129	0.276	0.143	0.222
	(0.336)	(0.391)	(0.353)	(0.336)	(0.244)	(0.254)
Militarization Measure	transfer value ($ Millions)	transfer value ($ Millions)	transfer value ($ Millions)	transfer value ($ Millions)	units transferred (1000s)	units transferred (1000s)
DEMIL Codes Included	B-Q	B-Q	B-Q	A-Q	A-Q	B-Q
Instruments	federal defense spending in state	lag of federal defense spending in other states	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state
Std. Error	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster	State Cluster
Weak Instrument F	13.462	10.688	13.462	12.762	12.752	9.975
Observations	561	561	561	561	470	470

Standard errors in parentheses. All models include state fixed effects and a quadratic trend. In column 3, all variables enter the equation with a single lag.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 7: FEIV regression of suicide rates on police militarization w/ differing standard error assumptions

	(1) Population Suicide Rate	(2) Population Suicide Rate	(3) Population Suicide Rate	(4) Population Suicide Rate
Militarization	-0.206*	-0.206***	-0.206****	-0.247**
	(0.123)	(0.0745)	(5.76e-15)	(0.126)
Unemployment Rate	0.118*	0.118	0.118****	0.00394
	(0.0706)	(0.0724)	(1.39e-15)	(0.0384)
Median Income	-0.162	-0.162	-0.162****	-0.184
	(0.329)	(0.238)	(2.76e-15)	(0.297)
Young Population Share	0.0351	0.0351	0.0351****	0.0505
	(0.100)	(0.0693)	(9.61e-16)	(0.0699)
Total Crime Rate	0.000228	0.000228	0.000228****	0.000229
	(0.000369)	(0.000228)	(6.94e-18)	(0.000195)
Log Population	0.795	0.795	0.795****	-1.664
	(4.028)	(3.358)	(5.15e-14)	(3.586)
Veteran Share of Population	0.242	0.242	0.242****	0.522**
	(0.284)	(0.296)	(3.76e-15)	(0.248)
Sample	All States	All States	All States	Excluding Florida
Instruments	federal defense spending in state	federal defense spending in state	federal defense spending in state	federal defense spending in state
Std. Error	Conventional	Huber-White	Region Cluster	State Cluster
Weak Instrument F	16.38	16.85	.	13.62
Observations	561	561	561	550

Standard errors in parentheses. All models include state fixed effects and a quadratic trend.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$