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# Putting our best boots forward: US military deployments and host- country crime

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## Abstract

The deployment of military forces abroad has been a major component of the US's grand strategy since the beginning of the Cold War. However, some scholars have argued that the presence of US military personnel abroad creates a series of negative externalities afflicting local communities. We put some of these claims to the test by looking at the effect of US military deployments on crime rates in the host-state. Using cross-national crime statistics from the United Nations and data on US troop deployments, we examine whether US military deployments are associated with higher levels of criminal activity across a large subset of crimes. In aggregate, the mere presence of troops does not increase the criminal activity in a state; however, there is a conditional effect when we account for a difference in culture between the host-state and the US; the presence of foreign deployed troops is associated with higher levels of property-related crimes in a country. Consequently, this paper contributes to a better understanding of the impact that US military deployments, and US foreign policy more broadly, have had on other countries, and also enhances our understanding of the micro-level factors that might affect relationships between alliance partners

## Keywords

Alliances, crime, cross-national criminology, foreign policy externalities, military basing, policy outcome, troop deployments, US foreign policy

“Everything here has been stolen,” goes the standing joke at Subic Bay. “They just haven’t bothered to take it all away yet.” (Deming et al., 1985)

The presence of US military personnel in other countries has prompted public debate and scrutiny of the positive and negative externalities from foreign troops in host-states. This article provides

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one of the first quantitative cross-national studies to inform this policy debate by addressing the following question: is there a systematic relationship between the presence of US military personnel and the prevalence of crime in the host-state? Since the beginning of the Cold War a primary pillar of US grand strategy has been to preserve the state's capability to rapidly deploy its military forces abroad in response to global or regional threats. A fundamental means of achieving this goal has been a reliance on the long-term overseas deployment of tens of thousands of US troops to countries across the globe. However, scholars have suggested that these deployments have largely negative effects on the host-state, and that overseas US military deployments are associated with increased levels of deviant behavior and crime in the host-state. Beyond US military personnel committing crime, such deployments are thought to foster a culture of criminality given issues of benefits, opportunity and accountability.

Scholarly accounts often focus on crimes committed directly by US soldiers. In 1995, three US service personnel kidnapped and raped a 12-year-old Okinawan girl, leading to widespread outrage among Japanese citizens and calls for the removal of US military personnel (Kirk and Okazawa-Rey, 2000). The presence of US soldiers has also generated reports of rises in the sex trade and prostitution. Moon (1997) details how the US military in South Korea provided support for the sex trade from the Korean War in 1950 until a joint US–Korea clean-up campaign in the 1970s. Countries around the globe have reported cases of rapes committed by US military personnel against civilians (Nelson, 1987; Moon, 1997; Baker, 2004). Between 1955 and 1997, 34 reported murders identified US military personnel as the perpetrators (Mercier, 1997).

Although many cases directly involve US service personnel, scholars have also detailed how the mere presence of US military deployments exacerbates criminal activity in the host-state more broadly. Baker (2004: 78) notes that during the Vietnam War drug abuse became rampant among US military personnel, with drug trafficking reaching “crisis proportions in Germany”. Within a three-month period in 1972, a survey found 6.72% of US military personnel in West Germany abusing drugs—roughly 13,500 individuals (Nelson, 1987: 91). The use of drugs and alcohol remained a problem even after the war, forcing the Army to use random drug tests to combat the problem (Hawkins, 2001). Criminal organizations bribed and coerced US military personnel into aiding drug smuggling operations, as the rotation of soldiers out of Vietnam fueled an increasing demand for narcotics. This growth in drug abuse and drug trafficking led to increases in other types of crime, like sexual assault and robbery (Baker, 2004). US troops have also facilitated the growth and expansion of criminal activity by providing civilians with access to goods and supplies from military facilities. Nelson (1987: 113) provides an illustrative example: “police arrested a middleman in Frankfurt who had fenced stereo equipment worth 24,000 marks and gasoline coupons for 500,000 liters”. At other times, the presence of US military forces has led to large demonstrations, riots and brawls (Baker, 2004; Simbulan, 2009). Although US military personnel are not directly involved in such cases, their presence serves as a catalyst.

While several studies emphasize the negative effects of US military deployments, quantitative researchers have not yet explored the issue. Little doubt exists as to whether members of the military have perpetrated criminal acts while deployed abroad. However, whether or not the presence of military personnel is exacerbating social ills in a broad and systematic way is not clear. Particularly egregious cases involving US military personnel often receive tremendous publicity and shape public perceptions regarding the kind of impact a US military presence has on the host-state. However, the cases we have briefly reviewed are not necessarily representative of general trends. Given the highly controversial nature of these deployments, the publication and attribution of crimes traced to a US military presence can be a powerful political tool. This suggests potential selection bias: crimes involving US military personnel may be publicized at a disproportionate rate

relative to crimes not involving US military personnel, thereby creating the impression that US military personnel are having an especially strong effect on crime.

A better understanding of this relationship has important policy implications. With such a large global military footprint, US military personnel and civilian support interact with the local populations more frequently than official delegates from US government agencies. These interactions undoubtedly affect public opinion of the US in the host-state, thereby shaping the incentives of political leaders and their preferences on matters related to US military deployments. This can also affect issues ranging from the specific operational freedoms enjoyed by US military forces on the ground to general issues affecting relations between the US and its allies, such as opening new bases or renewing agreements allowing for the continued presence of US military personnel (Cooley, 2008; Yeo, 2011). As a result, politicians and military commanders may need to enact measures to curb the impact of the US military presence on local criminal activity.

We evaluate these claims using a cross-national dataset on crime from the United Nations Office of Drugs and Crime. While a series of bivariate models indicate a fairly broad and positive relationship between the number of US troops in a country and crime, further refinement suggests more nuanced relationships. We find evidence that the positive association between US troop deployments on host-state crime is conditional upon the host-state's wealth, but only for a limited subset of the crimes for which we have data. We also find little evidence that the effect of US troop deployments on crime varies significantly across regions of the globe. Although our results represent a first attempt to quantitatively assess claims that have been widespread in the qualitative literature, and should be viewed as preliminary, we believe that these findings call into question a relationship that has become conventional wisdom in the study of US foreign policy and its effects.

To begin, we briefly review the existing criminology literature on cross-national studies of crime.<sup>1</sup> Second, given the general lack of specific theoretical mechanisms linking the presence of US military forces to crime increases in the literature, we draw upon literature from political science and criminology to construct a basic theoretical foundation. Finally, we assess these causal mechanisms with quantitative analyses followed by a discussion of the results.

## Cross-national criminology

Research on the cross-national determinants of crime has typically been the domain of sociologists. Such studies tend to focus on structural, or state-level, factors that contribute to shaping the incentives that lead to criminal behavior. Usually these factors include broad economic indicators, like the level of economic development or distribution of wealth, as well as other broad social characteristics of the state, such as the role of women in the labor force. What tends to vary are the theoretical mechanisms by which these characteristics influence the level of crime in a society.

Bennett (1991) discusses two theoretical mechanisms that are associated with variation in criminal activity levels. The first is the opportunity model. Developed by Kick and LaFree (1985), this model links macro-economic conditions like development and economic growth to crime by arguing that these trends create opportunities for crime to take place. Advanced industrial economies are more likely to be diversified across multiple sectors of production; such diversity and larger quantities of goods simply provide more opportunities for individuals to engage in crimes like theft and robbery. Kick and LaFree (1985) argue that urbanization increases the opportunity for crime by increasing the frequency of personal interactions and by lowering the social costs of such behavior. More tightly knit communities may be more likely to apprehend thieves and impose costly social and legal sanctions, while larger urban areas make available a greater variety of targets on whom potential thieves can prey with less fear of recognition and retaliation.

The opportunity model is influenced by factors states have some control over. Using tactics of deterrence, imprisonment and alternative sentencing models can raise or lower the perceived costs and benefits for those considering crime for economic gain (Sieberg, 2005). As the state increases its ability to punish criminals, the incentives for engaging in criminal behavior can be altered. However, given the increased incentives provided by deployments and the shielding of troops from traditional avenues of prosecution, military deployments may corrupt traditional state capacity functions. We return to this in the next section.

The second approach highlighted by Bennett (1991) is the Durkheimian model,<sup>2</sup> which emphasizes the weakening of social ties through the process of economic and industrial development. Durkheim's (1964) initial argument lies in the idea that, as industrial development outpaces the development of social norms and mores, the asymmetry in growth creates gaps in the range of acceptable behaviors and enforcement mechanisms. The introduction of new modes of economic and social exchange—or the changing pace of existing modes of exchange—will disrupt social norms and standards of behavior. The logics of both the opportunity and Durkheimian models function similarly. Authors have cited urbanization, industrialization and economic development as disrupting traditional modes of production and social interaction by drawing younger workers away from their homes and traditional means of production—agriculture, for instance. Through the migration of large numbers of young people to centralized locations the social forces that otherwise act as a check on individual behavior, such as family ties, evaporate. However, Neumayer's (2003) work exploring cross-national criminology finds that good governance policies can reduce crime, indicating that the state, by providing basic infrastructure and social services, has the capacity to alter the conditions that make crime likely, potentially counteracting such dynamics.

Finally, cultural theories posit that cultural structures drive differences in crime levels between countries and argue “that a lack of social cohesion and shared values reduces social constraints on violent behavior and increases social conflicts” (Neapolitan, 1997b: 360). These cultural traits and lack of social cohesion can include differences in a range of factors, such as a country's racial composition (Avison and Loring, 1986; Neapolitan, 1998), dominant religion (Groves et al., 1985) or colonial history (Neapolitan, 1994, 1997b). As Neumayer (2003) discusses, the opportunity and Durkheimian frameworks typically treat states identically. Alternatively, approaches focusing on cultural variables account for factors that may interact with modernization and development in different ways. Societies that emphasize stronger familial connections may, *ceteris paribus*, counteract the effects of modernization on criminal activity. Alternatively, states with a long colonial past might have difficulties maintaining and implementing mechanisms of social control, as colonizing powers disrupted or destroyed the cultural centers of power that serve as social checks on criminal behavior (Neapolitan, 1997b). The use of decentralized control systems by the British created the ground for ethnic competition to foster tension, violence and ultimately civil war between rival groups (Blanton et al., 2001).

## Linking US military deployments and criminology

As Eric Neumayer argues, sociological models of crime often lack manipulable factors that may influence criminal activity (Neumayer, 2003: 619).<sup>3</sup> One such mechanism is the deployment of foreign military personnel abroad. While a US military base may be a constant, the size of deployments fluctuates over time in response to decisions made by US government officials and the host-state, adding an additional layer of variation. This addresses both the issue of including dynamic factors and that of accounting for the role of international factors in affecting crime. Also, the existing literature provides little theoretical basis for linking troops and crime. Here we attempt to

combine the macro-level insights of the criminology literature with a micro-level theoretical foundation to make predictions on the systematic relationship that may exist between troops and crime.

To construct a theoretical model linking troops and crime we make four initial assumptions regarding the behavior of individual troops. First, we assume that individual US soldiers attempt to maximize personal utility. Second, the degree to which individual soldiers can accomplish this goal within the framework of their jobs as members of the US military varies across individuals. Third, we assume that crime is an option for soldiers to increase personal utility, and that this option carries with it a non-zero probability of being caught and punished. Finally, we assume that the risk-aversion of soldiers with respect to being caught and punished is normally distributed. Thus we should expect a random segment of a US deployment to be unable to maximize their personal utility through military service alone, and to engage in criminal activity to increase their personal gains, while also minimizing the probability of facing punitive measures from the military or the host-state. An additional implication of these assumptions is that, as the size of a deployment grows, so too does the number of individuals who are likely to engage in criminal behavior.

We believe that these assumptions are a reasonable basis on which to proceed: scholars have argued that the military intentionally recruits individuals who will be able to garner higher wages in the military than in civilian life (Johnson, 2004; Nelson, 1987). For some, the military is an alternative to jail since recruits are often young and poor—both demographics that are at risk for criminal activity. This selection process, in conjunction with the regimented organization of the military precluding innovation and entrepreneurial methods of increasing personal gain, suggests that members of the military may be more inclined toward deviant behavior, and more likely to exploit their access to military materials or vulnerable civilian populations in an effort to maximize their personal gains (see Merton, 1949; Humphreys and Weinstein, 2008).

Our model associating US troop with crime is similar to Humphreys and Weinstein's (2006, 2008) and Weinstein's (2007) work on rebel recruitment and the level of abuse directed toward civilians during civil conflicts. The authors argue that abuse toward civilians is more likely when there are "units composed of members with private goals and [that] lack internal mechanisms to discipline behavior" (Humphreys and Weinstein, 2006: 430). Although the authors are theorizing in the context of civil conflict, the theoretical mechanisms regarding the role played by selective incentives in rebel recruitment and the composition of rebel military units may be more generalizable. While the US military does have mechanisms to discipline its soldiers, individual soldiers are prosecuted by the military itself when an individual faces criminal charges (High, 2008). Given this conflict of interest, the offense can receive a lesser penalty or even go unpunished, affording perpetrators more latitude to commit offenses against civilians (Johnson, 2004). If, in an all-volunteer army, a portion of those volunteering are doing so purely to collect wages, then the opportunities for deviant behavior are amplified when the punishment system is inconsistent (Weinstein, 2007).

This proclivity for crime can manifest itself in a number of ways. The presence of a deployment can itself increase the opportunities for crime to take place in the broader environment. Given the extensive supply of material resources that generally accompanies long-term troop deployments, the potential for black market activity is enormous. It is often possible for profit-seeking soldiers to sell military goods ranging from fuel to weaponry to local civilians (Nelson, 1987). In 1985, millions of dollars' worth of equipment were suspected to have been stolen by crew members of the *USS Kitty Hawk* and sold on the black market (Conant and Miller, 1985). Similarly, over 80,000 firearms have gone missing in Iraq (Dickey et al., 2007). Since the military has so much material to account for at any given time, it is not uncommon for items to go missing and never be recovered. This also suggests a lower probability of detection and punishment.

Areas near US military deployments or areas that serve as rest and relaxation centers for US military personnel increase the opportunity for crime to take place by stimulating an increase in local and regional human/sex trafficking, which correlates with high levels of drug abuse and violence (Moon, 1997; Sang-Hun, 2009). The presence of US deployment increases the demand for such goods and services, strengthening existing domestic and international networks of criminal activity, and potentially creating new ones. These networks extend beyond the areas surrounding military facilities. Smugglers sell guns or other materials obtained from such facilities and buyers use them far away from their location of origin. If the presence of US military personnel increases the demand for narcotics, their provision may involve actors from all over the country in the production and trafficking process. Nelson (1987) and Baker (2004) note that the rise in drug abuse among US military personnel was associated with a commensurate increase in drug trafficking within West Germany. The increased demand for sex workers may also lead to increased crime as sex trafficking rings grow to meet this new demand. That the violence and drug abuse frequently associated with these organizations may also increase in a similar fashion suggests another mechanism by which a large troop presence indirectly increases crime (Moon, 1997).

In accordance with the theoretical mechanisms above, we derive the following hypothesis:

**Hypothesis 1.** As the number of US troops increases in a country, the amount of crime in that country should increase.

Economic development might also condition the impact of troops on crime for three reasons. At the micro-level, Humphreys and Weinstein (2006: 431) argue that rebel soldiers will abuse civilian populations less in areas where there are expectations of future gains from extraction and that abuse should be higher in poorer areas where soldiers see lower prospects for future extraction. Assuming troops are seeking to maximize their personal utility, a similar logic should follow. Where opportunities for future extraction are greater (wealthy states), troops should commit less crime in a given period. At the macro-level, the integration of a different economic population into a state can encourage crime as demonstrated by both the opportunity and Durkheimian models. In poorer societies, the presence of a large, developed army introduces wealth, increased economic activity and opportunities for crime. Such economic and social distortions can also exacerbate the breakdown of social ties in the host-state. Societies with higher levels of development may not experience these shocks as pre-existing opportunities for crime may be just as attractive, and the presence of valuable capital-intensive goods is a smaller differential over that already existing. Finally, from a different perspective, higher levels of development can also be indicative of greater state capacity. Wealthier states simply have more resources to direct toward policing and combating crime. They may also have stronger institutions dealing with property protection. Together these factors can alter the benefits and costs for crime. As such, any increases in crime caused by the presence of US troops should be dampened in more developed societies.

**Hypothesis 2.** Less developed countries will experience more criminal activity from troop deployments than more developed countries.

Accounts in the literature suggest the deployment of US troops and the introduction of competing cultural forces into the host-state also affect crime. For example, in 1970, a spokesman for Libya's Revolutionary Command Council referred to US soldiers as "tens of thousands of foreigners with their wicked habits, including liquor and miniskirts" (Baker, 2004: 72). Such differences may lead to more crime if US troops are unaware of just how different the laws and customs are in

the host-state. Although more specific theoretical mechanisms of cultural factors are sparse in the criminology literature, Fearon and Laitin (1996) provide a useful theoretical framework for conceptualizing these issues. Consistent with our own assumptions they assume that “human transactions and interactions involve the possibility of opportunism—self-interested behavior that has socially harmful consequences” (Fearon and Laitin, 1996: 717). Reputation serves as an important indicator of the probability of being exploited in day-to-day exchanges with other individuals. As the number of individuals in society increases, the costs of obtaining such information also increases. The authors argue that the formation of a dense social network *within* ethnic groups serves as one mechanism for providing this information at a lower cost. Thus, within-group transgressions can be punished with relatively low cost since these dense social networks facilitate identification of offenders. However, since these same social networks do not exist or are much less dense *between* groups, identifying individuals from outside of one’s group becomes more costly and the probability of detection and punishment decreases.

Accordingly, the opportunity to engage in crime—by US troops or host-state residents—should increase where there is a cultural divide between US troops and the host-state’s citizens given that the relative ease of identifying individual perpetrators decreases. As stated by Fearon and Laitin (1996: 730), “[P]eople are better able to distinguish opportunists among coethnics, which facilitates peaceful interaction within and hampers it across ethnic groups”. The nature of US troop deployments—the concentration of troops near military bases and their routine rotation out of the host-state, for example—inhibits the development of the kinds of networks and ties across groups that facilitate identification of offenders. Given relatively short time horizons and weak networks between troops and host-state residents, we should expect individuals to behave more exploitatively as risk of detection decreases.

**Hypothesis 3.** US deployments into states with different cultures as compared with the US should increase the crime in that country.

Having outlined the relevant hypotheses we now turn to the operationalization of our analysis.

## Data and operationalization

Although there is a large body of literature on the study of crime across nations, there remain some important issues to address regarding the accuracy of the data used in these studies. According to LaFree (1999), the two most accurate and comprehensive data sets are those compiled by INTERPOL and the World Health Organization (WHO). Neumayer (2003) states that the WHO data and the United Nations Crime Trends Survey (UNCTS) data are the most reliable sources. The first problem is that the reporting of crime statistics by individual countries is voluntary. Approximately 59% of INTERPOL member countries submitted reported crime statistics in any given year in the period from 1980 to 1984 (Kalish, 1988). The UNCTS survey received returns from approximately 80–90 countries during any given survey cycle. The WHO data face similar difficulties. Generally, the states that submit accurate records on a regular basis are most likely to be advanced industrialized democracies (LaFree, 1999).

These difficulties are further highlighted when we consider that many crimes that directly or indirectly involve US military personnel may go unreported. This problem may be particularly acute when it comes to sexual assault—crimes that are already typically under-reported (Allen, 2007; Diener and Suh, 1997; Williams, 1984). Doubts about the impartiality of investigation and prosecution procedures might lead crime victims to doubt the benefits of reporting a crime. If US military officials are responsible for investigating crimes committed by their own soldiers, then it is clear

why outsiders might doubt the sincerity of efforts to catch and punish those responsible. Although these issues are a clear concern, some of the evidence previously discussed shows that crimes involving US military personnel are in fact often reported to the host-state's law enforcement officials. As we discuss below, it is the distinction between reported and prosecuted that is important.

Our analyses use data from the UNCTS, collected by the United Nations Office on Drugs and Crime (UNODC) (2010). The UNCTS data runs from 1970 through 2008. For the purposes of this study we use data from 1981 to 2008. Some authors note that early iterations of the data collection efforts are highly inaccurate and unreliable. While there is no definitive cutoff point of increasing reliability and accuracy, it may be desirable to exclude data from the early 1970s (Neapolitan, 1997a; Neumayer, 2003). While the data on US troop deployments runs from 1950 to 2011, other independent variables are only available until 2008.

The UNCTS data are desirable for additional reasons. WHO data only provides information on homicide. The UNCTS data provide information on several crimes like homicide, assault and theft. As previous work has linked the presence of US troops to a variety of criminal acts, examining a broader range of crimes is desirable as we can draw more general and robust conclusions concerning the relationship between US troop deployments and crime in the host-state. Using a single outcome variable potentially biases conclusions as null findings or spurious relationships for one particular variable may mask more general trends. For example, some scholars point to the presence of US military personnel leading to an increase in sex-trafficking in some states (Baker, 2004; Moon, 1997). Thus, we might expect to see an increase in other types of violent crimes as well, like assault and rape. Additionally, the UNCTS data consist of *reported crimes*, rather than convictions or prosecutions. This is a desirable characteristic given the serious possibility that US military officials will not prosecute or convict US military personnel accused of a crime. Case studies have indicated that the *reporting* of crimes involving US military personnel to host-state officials is less of an issue than are conviction and punishment. Using reported crime levels helps deal with these issues and allows one way to potentially address the selection bias that may be inherent in statistics provided by the US military. Finally, as we have an interest in the broader impact of US troops on host-state crime levels, the use of crime statistics reported by the host-state itself yields information that crime statistics obtained from US government agencies cannot. While the US military and other government agencies collect data on crime directly involving US military personnel, we cannot evaluate the broader effect of US troops using these figures alone, as they only record crimes where US personnel are directly involved.

We should also note that there are concerns about aggregation bias. Our study looks at a micro-process and assesses the impact at a macro-level, which is problematic as there are potential intervening factors between troop deployments and aggregate crime. We employ these data for a handful of reasons. First, the criminology literature has a history of using these data to analyze crime at a national level. This enables our research to incorporate international relations-based causal processes into an established literature. Second, we are concerned about policy impact on a society: are there noticeable, macro-level effects from US foreign policy? As our arguments are not just about individual actions, but about the networks that result from deployments, a wider view is required. However, we proceed with caution. There are limitations in the reporting, measure and estimation of these data; any relationships we find serve as insight, but not as definitive conclusions.

## Estimation strategy

Previous research tends to use linear models to estimate crime in a given country-year. However, there are clear reporting biases in the crime data; only one-third of the available country-years have reported crime data. Accordingly, we modeled each regression as a pooled cross-sectional

**Table 1.** Crime variable summary statistics

Variable	Mean	Standard deviation	Minimum	Maximum	N
<i>Theft</i>	244,202.517	539,222.037	2	3,911,778	1771
<i>Robbery</i>	19,553.837	61,987.312	1	607,444	1913
<i>Burglary</i>	77,861.735	167,387.614	2	1,369,584	1239
<i>Drug</i>	14,168.902	33,527.178	1	283,708	1820
<i>Assault</i>	31,859.203	73,860.278	1	723,886	1654
<i>Rape</i>	2,160.798	5,965.274	0	54,293	1914
<i>Homicide</i>	2,607.259	7,794.643	1	75,928	2503

time series using a two-step Heckman selection model for important reasons. First, it seems likely that there are systematic reasons why a country chooses not to report crime statistics. As this removes states from the sample, modeling the selection bias in the data is critical. In the first stage of the model, we predict whether or not the country reports the amount of crime for that year. The second stage of the model employs least squares estimation. To account for spatial heterogeneity unaccounted for by the independent variables in our cross-national study we follow Neumayer (2003) and employ binary fixed effect variables in the second stage of the estimation.<sup>4</sup>

## Dependent variables

For our dependent variables we use seven categories of criminal activity obtained from the UNCTS.<sup>5</sup> Below we provide a list of each of these variables along with definitions for each individual category as defined by the United Nations Office of Drugs and Crime (UNODC, 2011). Table 1 provides the summary statistics on each of the seven categories.<sup>6</sup>

The UN definition for the seven categories of crime is as follows.<sup>7</sup> *Theft* – the UN defines theft as depriving a person or organization of property without force with the intent to keep it, and does not include burglaries, armed robberies, or the theft of automobiles. *Robbery* – the UN defines robbery as the theft of property from a person, overcoming resistance by force or threat of force. *Burglary* – the UN defines burglary as gaining unauthorized access to a part of a building/dwelling or other premises, including by use of force, with the intent to steal goods. *Drug Related Crime (DRC)* – the UN defines drug related crimes as all intentional acts that involve the cultivation, production, manufacture, extraction, preparation, offering for sale, distribution, purchase, sale, delivery on any terms whatsoever, brokerage, dispatch, dispatch in transit, transport, importation, exportation, possession or trafficking of internationally controlled drugs.<sup>8</sup> *Assault* – the UN defines an assault as a physical attack against the body of another person resulting in serious bodily injury, excluding indecent/sexual assault, threats and slapping/punching.<sup>9</sup> *Rape* – the UN defines rape as sexual intercourse without valid consent. *Homicide* – this variable captures the total number of reported intentional and unintentional homicides, and the UN defines it as the unlawful death intentionally or unintentionally inflicted on a person by another person. The UNCTS data includes manslaughter in the unintentional homicides category,<sup>10</sup> but excludes traffic accident deaths.<sup>11</sup>

## Independent variables

Our primary variable of interest is the number of *Troops* in a country (Department of Defense, various years; Kane, 2004).<sup>12</sup> The data is appropriate for our purposes as it allows us to capture the ebb

and flow of troops from year to year and discern whether or not the population of troops in a given country relates to increases in crime.<sup>13</sup>

As our hypotheses predict that the impact of troops is potentially conditional upon environmental factors such as opportunity/development, as well as cultural differences, we include additional variables to address these possibilities. To discern the conditional impact that the host-state's level of development has on a US troop presence we include an interaction term (*Troops \* ln GDP*). Our hypotheses also predict that the impact of US troops on crime should be greater in states characterized by greater cultural differences as compared with the US. To approximate such cultural divides we use the Correlates of War (COW) region variable to identify between five global regions. Europe acts as the baseline category, and we include binary variables for Latin America, the Middle East, Asia and Africa.<sup>14</sup> We then interact these regional variables with the *Troops* variable to capture the conditional impact of troops on crime across these divides.<sup>15</sup>

Our control variables represent conditions that have exhibited empirical or theoretical support in the past in relation to crime. We break this down into demographic characteristics and institutional features of the state. Previous studies have used demographic characteristics to determine if a population is more at risk for crime than other populations. As such, we include the log of the total population (*ln Total Population*); the more individuals there are a society, the greater the opportunity for criminal activity. We log this variable as we expect there to be a non-linear relationship between the variables that diminishes at larger values. The percentage of individuals in an urban environment (*% Urban Population*) should increase the opportunities available for crime and generate the anomie that encourages crime (Kick and LaFree, 1985). We generate both total population and the urban population variables from the COW data (Wayman et al., 1983; Singer et al., 1972; Sarkees et al., 2003).

Neumayer (2003) accounts for social integration and anomie by using female work force participation. Higher rates of female participation in the workforce have been alleged to disrupt traditional family structures and gender roles, as well as increase the available opportunities for crimes such as theft and sexual assault (Neumayer, 2003; Gartner, 1990). We include *Women in Labor Force*, as measured by the percentage of women in the labor force, aged 15 or over. Unemployed segments of society represent individuals who are at risk for criminal behavior, and higher levels of unemployment (*Total Unemployment*) should associate with higher crime levels. We obtained these variables from the World Bank's World Development Indicators (World Bank, 2010).

Structural variables affect opportunities for crime. Democratic states reduce opportunities for crime by debasing the need for crime through addressing grievances politically. Polity also appears related to the backlash against troop behavior (Cooley, 2008) and may be indicative of troop behavior in general. We expect a negative association between *Polity*, a 21-point variable that ranges from -10 (autocracy) to 10 (democracy) and criminal activities (Jaggers and Gurr, 1995; Marshall et al., 2003; Neumayer, 2003). As with Neumayer (2003), we use the square of polity (*Polity*<sup>2</sup>) as an additional structural control. Strong democracies and autocracies—regimes where governmental institutions are firmly entrenched and are not likely to deviate from the extremes of the polity scale—should have greater state capacity than their non-consolidated peers. This variable can act as a proxy for the ability to punish, deter and circumvent the conditions that create crime. Such consolidation may deter crime and be negatively associated with each of the seven measures. Further, some consolidated autocracies may see underreporting of crime as a result of corruption and ties between organized crime and government officials. We include *Independent Judiciary* to control for structural corruption (independent judiciaries should decrease crime and corruption) and address this correlation between corruption and authoritarianism (Cingranelli and Richards, 2010).

We also use infant mortality rates (*IMR*) as a proxy for economic development and state capacity (Abouharb and Kimball, 2007; World Bank, 2010). While previous studies have used GDP per capita as this measure, the reliability and completeness of the IMR data are compelling enough to use in its place and enable us to use the natural log of GDP as an additional control variable to measure overall state capacity for commanding resources (*ln GDP*) (World Bank, 2010).

IMR and respect for human rights can both fall under considerations for “good governance” (Neumayer, 2003). Respect for human rights should decrease levels of crime, whereas more repressive governments may foster conditions that lower social inhibitions when it comes to aggressive and violent behavior. Thus we include the CIRI index on human rights as a control (Cingranelli and Richards, 2010).<sup>16</sup> IMR and CIRI demonstrate the present ability of the state to meet the basic demands of its population. High IMR or low CIRI scores represent either an incapability or an unwillingness of the government to improve social welfare.

Finally, there is a possibility that domestic political upheaval might contribute to higher crime. We use the dichotomous variable *Civil Wars* to identify whether or not a state is experiencing a civil war in the observed time period (Sarkees and Wayman, 2010). The presence of a civil war will help us to account for two factors: first, whether or not a state reports their statistics; and second, what the presence of such domestic conflict has on crime in states that do report.

In the selection stage, we have additional variables that we believe are likely to predict whether or not countries report crime statistics. We expect *Polity*, *Human rights*, *GDP*, *Total Population*, *ln Urban Population*, whether the nation reported its statistics previously, and a temporal variable indicating how long studies have been collected by the UN (*time*) all to influence reporting<sup>17</sup>—by affecting either feasibility of reporting or general good governance. Additionally, we include *ln Urban Population* as we expect larger urban populations make policing and reporting manageable. We include a variable for *Time* that counts the year since the start of the study, expecting more nations to opt-in to the study as time goes on. While the selection model is important for adjusting the substantive influence of variables on the number of crimes within a given state, the substantive interpretation of the model is not of primary interest to us. As such, we do not report the selection stage in the following section, but do provide it in the Appendix.<sup>18</sup>

We leave the possibility of directional expectation open for all our variables. While we provide directional hypotheses, we remain skeptical of these connections. Accordingly, we employ two-tailed significance tests for the coefficients in our models.

## Results

For comparative purposes, Table 2 presents basic bivariate estimations of the relationship between US troops and crime. If we assumed this to reflect the true data generating process then conventional wisdom regarding the relationship between troops and crime would appear to be accurate: troops are systematically and positively correlated with crime across most categories. In six of the seven models, troops increase the level of crime in a state; only homicides decrease with the presence of troops. However, given that there is much variation being captured by the error term, *Troops* variable and the constant, we turn to our more complicated models that try to tease out the spatial and temporal variation exhibited by the US and prospective host-countries.

Table 3 presents the initial set of full regressions where we look at the independent effect of troops. The *Troops* variable is negative and significant in four of the seven models, and presents mixed results in the three remaining models. Overall, the preliminary findings in this second table suggest that, once we control for additional explanatory factors, troops have a negative impact—no statistically significant impact—on a range of crimes, contrary to established expectations.

**Table 2.** Total reported crime using a standard linear model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<i>Theft</i>	<i>Robbery</i>	<i>Burglary</i>	<i>Drug</i>	<i>Assaults</i>	<i>Rape</i>	<i>Homicides</i>
<i>Troops</i>	14.45*** (1.279)	0.0957*** (0.0230)	2.137*** (0.602)	0.475*** (0.0834)	1.019*** (0.119)	0.0213*** (0.00245)	-0.00556** (0.00249)
Constant	196,616.2*** (9974.0)	20,212.2*** (1503.2)	75,866.2*** (4580.6)	12,878.0*** (699.0)	29,129.5*** (1730.6)	2189.7*** (147.5)	2800.9*** (171.3)
Observations	1678	1813	1161	1729	1561	1813	2336

Robust standard errors in parentheses. Two-tailed significance tests used.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

As for the control variables, *Unemployment* and *ln GDP* are both positively correlated with the occurrence of theft. Democratic states correlate with higher levels of robbery while greater respect for human rights correlates with less robbery. The percentage of women in the work force is negatively correlated with the amount of burglary in a society. *ln Total Population*, *ln GDP* and *Polity*<sup>2</sup> indicate more drug-related crimes. The percentage of women in the work force and *ln GDP* correlate with increased number of reported assaults, while large populations have fewer reported assaults. For rape, civil war and greater respect for human rights increase the reported number of rapes in a society. The percentage of women in the workforce is also associated with a higher level of reported rape in a society. It is our expectation that this probably due to the economic empowerment of women facilitating the reporting of the crime, as well as the potential increased opportunity for such events to occur. Finally, *ln GDP*, *Polity* and high *IMR* correlate positively with reported homicides, while an independent judiciary and consolidated political institutions decrease the number of reported homicides in a country.

The control variables vary in their significance across the seven crime categories. The two variables that tend to be most consistently significantly correlated with crime—*Percentage of Women in the Workforce* and *ln GDP*—only prove to be significant in three of the seven estimations. Mixed indications are to be expected given that the crimes have different causal processes and the variables will capture separate elements of it. For example, the first four categories represent different forms of property-related crime. Within those four categories, *Robbery*, and to a lesser extent *Burglary*, involve the taking of property with the explicit use or threat of force and are typically treated as a more severe crime than regular theft. Alternatively, *DRC* represents 18 different classes of drug-related crimes. Accordingly, the causal pathways between our variables and the aggregated dependent variables are likely to be mixed. Thus, the three variables that increase the amount of DRC in a society, *Polity*<sup>2</sup>, *Population* and *ln GDP*, all relate to the demand side (or opportunity cost of demanding drugs), which would increase the supply chain of drugs as a whole.

However, as per our hypotheses, these initial findings could be the result of omitted variable bias. If the impact of troops on crime is conditional upon certain cultural or economic factors that contribute to providing greater or fewer opportunities for crime to take place, or that increase the kinds of social anomie that the theoretical literature associates with crime, these initial results are potentially biased as they simply capture average effects across countries and time.

Table 4 shows the results from the second set of models that include the interactions between troops and the regional variables and with *ln GDP*. Given the interactions, interpreting the coefficient on the troops variable alone requires caution. The range of *ln GDP* is from 4.87 to 15.48, and

**Table 3.** Total reported crime rates using a Heckman selection model with fixed effects in the prediction stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Theft	Robbery	Burglary	Drug	Assaults	Rape	Homicides
Troops	-3.107 (3.464)	1.023 (0.715)	-4.960*** (1.558)	-0.972*** (0.305)	-1.796*** (0.689)	-0.110*** (0.0361)	0.0367 (0.0254)
Polity	2358 (6246)	2173** (868.2)	139.5 (2358)	-213.0 (340.1)	-326.1 (867.0)	-52.22 (43.68)	165.6*** (35.29)
Polity <sup>2</sup>	432.9 (848.0)	-66.36 (155.3)	-202.7 (308.0)	287.5*** (59.42)	-144.2 (156.1)	5.200 (7.553)	-14.34** (6.024)
In Population	-156.871 (128,230)	26,399 (26,284)	81,770 (53,650)	19,143* (10,968)	-53,403*** (26,254)	392.2 (1323)	-857.8 (1181)
% Urban Population	-67.44 (115.2)	10.73 (24.84)	5.330 (36.24)	1.659 (9.497)	10.69 (21.84)	-0.218 (1.231)	1.378 (4.154)
Unemployment	9738*** (3071)	326.1 (602.5)	1669 (1187)	145.9 (239.1)	267.9 (589.4)	43.32 (31.04)	29.04 (25.97)
% Women in Workforce	-2425 (3095)	649.4 (615.8)	-2719** (1257)	-380.6 (251.1)	2265*** (592.3)	82.31*** (31.00)	-5.417 (26.91)
In GDP	75,395*** (23,574)	7337 (4802)	-13,019 (11,335)	5397*** (1873)	15,221*** (4634)	363.2 (238.9)	454.7** (214.6)
Infant Mortality Rate	531.8 (1364)	167.4 (281.4)	747.1 (727.2)	-102.5 (115.3)	-9.423 (274.3)	-13.32 (14.30)	27.98** (12.06)
CIRI	-2285 (8113)	-4725*** (1646)	-3231 (3277)	-222.3 (664.2)	1155 (1574)	146.5* (82.58)	-81.78 (69.44)
Independent Judiciary	-39,328 (26,196)	-2895 (5136)	-9579 (10,643)	-1732 (2068)	-824.9 (5009)	4.662 (258.1)	-780.7*** (233.1)
Civil War	-9499 (43,743)	-439.4 (8396)	-12,034 (21,270)	4680 (3786)	7468 (9624)	808.3* (423.9)	378.6 (352.5)
Constant	1.787 × 10 <sup>6</sup> * (1.806 × 10 <sup>6</sup> )	-493,754 (371,258)	-823,463 (747,968)	-327,030** (155,714)	550,924 (369,825)	-15,050 (18,673)	9914 (16,681)
N	3288	3221	3390	3233	3293	3219	3232
N (Censored)	2368	2221	2714	2285	2415	2233	2402
ρ	0.0873	0.0566	0.0775	-0.0441	-0.0161	0.00621	0.00301
σ	205.485	44.373	63.977	16.937	38.840	2195	1668
λ	17946	2511	4961	-747.3	-627.1	13.63	5.029

Standard errors in parentheses. Two-tailed significance tests used.  
\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01.

so it is always non-zero and both the constituent terms and the interaction should be included for interpretation. As such, troops have positive effects on *Theft*, *Robbery* and *Burglary* for the majority of values for *ln GDP* in European countries. Broadly speaking, as values for *ln GDP* increase, the effect of troops on aggregate crime becomes smaller, eventually becoming statistically indistinguishable from zero at the highest values of *ln GDP* for *Theft*.<sup>19</sup> The *Troops* variable is negative and significant for *DRC*, *Assaults* and *Rapes*. The effect of *Troops* is not statistically significant for *Homicides*. The interaction between *Troops* and *Assaults* is the only interaction with *ln GDP* that is statistically significant beyond thefts, robberies, and burglaries. This indicates that the negative correlation between *Troops* and *Assaults* becomes smaller in magnitude as *ln GDP* increases.

To better assess the predicted substantive impact of troops on crime it is informative to more closely examine the marginal effect of troops. Figure 1 plots the marginal effect of an increase of 500 troops on thefts and assaults.<sup>20</sup> We use 500 as our change value because it is approximately equal to the size of one battalion and we believe it is more substantively meaningful to talk in terms of military units than in terms of individual soldiers. To better enable us to compare effects across models, we also hold the *y*-axis constant in both panels.

Continuing to focus on Europe as our base category, Figure 1a shows that at the lowest observed levels of *ln GDP* an increase of 500 troops increases the number of reported thefts by approximately 27,000. However, as *ln GDP* increases, the positive effect of adding one battalion-sized unit on levels of reported thefts steadily decreases in magnitude, and becomes statistically insignificant when *ln GDP* is approximately equal to 12.6. Figure 1b illustrates that a similar increase of 500 troops has a negative and statistically significant effect on assaults. While the substantive size of this negative effect does diminish at higher levels of *ln GDP*, it remains statistically significantly different from zero over the entire observed range of *ln GDP*. In terms of absolute effects, the results suggest that the conditioning effect of *ln GDP* is substantively larger for *Theft* than for *Assault*.

Beyond *ln GDP*, the interaction variables between the *Troops* variable and the regional variables generally yield results that are either insignificant or positive. Of the interaction terms displayed in Table 4, only the theft equation yields negative interactions between the troops variable and the Asia and Latin America dummy variables. However, as with *ln GDP*, we need to be cautious when interpreting these results. Figure 2 shows the predicted marginal effects for an increase of 500 troops for the seven categories of crime modeled in Table 4. We should note that we exclude Africa from these figures because the model estimates yield effects that are so far out of line relative to the other regions that it makes visual interpretation of the remaining regions difficult.

The results shown in Figure 2 illustrate that the predicted effect of troops—as conditioned by region of deployment—is statistically insignificant in most cases. When we look at Europe, the marginal effect of an increase of 500 troops is negative and statistically significantly different from zero for *DRC*, *Assault* and *Rape*. The only marginal effect of troops that is positive and significant when looking at Europe is for *Theft*. Alternatively, Asia is the only other region besides Europe that yields statistically significant marginal effects for a similar battalion-sized increase. The marginal effect of troops is significant and negative in Asia for *Theft* only, but the marginal effect is positive and significant for *DRC*, *Assault* and *Homicide*. These results indicate that relationship between US troops and crime does not appear to vary greatly across regions. Where it does, it appears that troops are most likely to exacerbate crime in Asia. This effect also appears confined to *DRC*, assaults and homicides. In Latin America and the Middle East the marginal effect of troops is statistically indistinguishable from zero across all seven of the crime models. Finally, although we do not graph the marginal effects of troops in Africa for presentation reasons, it should be noted that these effects are not statistically significant in any of the seven crime models presented in Table 4.

**Table 4.** Total reported crime rates using a Heckman selection model with fixed effects in the prediction stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Theft</i>							
<i>Troops</i>	110.6*** (21.50)	10.07*** (3.878)	44.77*** (11.66)	-2.442* (1.477)	-19.71*** (4.283)	-0.332* (0.195)	-0.0561 (0.164)
<i>Africa</i>	-282.751* (168,488)	62,890* (35,055)	-85,136 (65,572)	38,865*** (14,337)	33,660 (33,066)	3782*** (1757)	-433.9 (1590)
<i>Asia</i>	-68,657 (84,620)	13,684 (17,646)	-26,455 (27,738)	23,684*** (6504)	28,502* (16,452)	1230 (877.7)	-32.26 (733.4)
<i>Middle East</i>	-487,398 (316,640)	52,949 (49,001)	9538 (120,771)	-10,281 (18,619)	12,014 (42,433)	951.9 (2434)	1567 (2791)
<i>Latin America</i>	-109,955 (92,411)	44,729** (17,920)	-125,118*** (35,255)	14,058** (7038)	30,842* (16,039)	2147*** (1006)	252.7 (824.9)
<i>Troops * Africa</i>	-5651 (13,194)	3128 (1929)	2299 (2938)	-404.1 (727.1)	4475* (2474)	3.012 (95.63)	-35.31 (73.12)
<i>Troops * Asia</i>	-36.24*** (8,278)	-1.538 (1,665)	5,668* (3,252)	3,520*** (0,726)	10,92*** (1,658)	0.134 (0,0843)	0.0819 (0,0699)
<i>Troops * Middle East</i>	-23.45 (24,40)	-5,923 (5,975)	-1,825 (9,853)	4,543* (2,413)	-1,942 (5,004)	0.0601 (0,269)	-0.0520 (0,0701)
<i>Troops * Latin America</i>	-36.88** (16,49)	-2,975 (3,489)	-210.4 (181.6)	3,168** (1,324)	10,73** (4,766)	0.313* (0,174)	-0.0378 (0,142)
<i>Troops * In GDP</i>	-8.014*** (1,551)	-0.675** (0,277)	-3.919*** (0,863)	0.0199 (0,105)	1.136*** (0,310)	0.0126 (0,0139)	0.00728 (0,0119)
<i>Polity</i>	5660 (6192)	2954*** (906.1)	1349 (2508)	-228.3 (350.0)	-316.6 (845.9)	-59.18 (45.76)	160.5*** (36.85)
<i>Polity<sup>2</sup></i>	243.6 (843.2)	-136.3 (160.7)	-233.2 (321.7)	294.6*** (60.70)	-115.4 (152.4)	6.664 (7.822)	-13.22** (6.228)
<i>In Population</i>	-294,079** (129,467)	15,772 (26,963)	65,998 (53,053)	24,364** (11,062)	-25,844 (26,032)	953.7 (1354)	-600.5 (1221)
<i>% Urban Population</i>	-89.03 (113.4)	8,398 (24.76)	1,118 (35.54)	1,112 (9,348)	11.88 (21.25)	-0.176 (1.230)	1.375 (4.147)

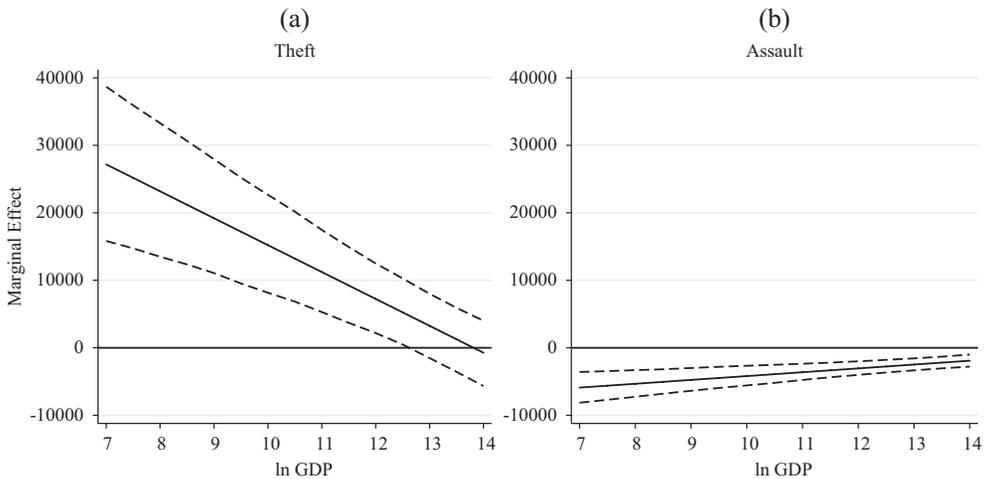
(continued)

Table 4. (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Theft	Robbery	Burglary	Drug	Assaults	Rape	Homicides
Unemployment	10,486 <sup>***</sup> (3047)	392.3 (602.7)	2217* (1172)	224.5 (236.6)	375.4 (577.2)	43.89 (31.15)	32.51 (26.02)
% Women in Workforce	319.8 (3143)	821.3 (646.1)	-2444* (1258)	-578.8 <sup>**</sup> (259.9)	1836 <sup>***</sup> (595.8)	76.62 <sup>**</sup> (32.13)	-5.309 (27.51)
In GDP	110,117 <sup>***</sup> (24,330)	11,369 <sup>**</sup> (5049)	-2730 (11,363)	5866 <sup>***</sup> (1950)	11,855 <sup>**</sup> (4684)	275.6 (250.1)	437.3* (231.8)
Infant Mortality Rate	554.7 (1369)	246.1 (286.5)	876.3 (715.5)	-111.5 (116.4)	129.9 (272.3)	-12.19 (14.53)	29.41 <sup>**</sup> (12.09)
CIRI	-546.5 (8022)	-4869 <sup>***</sup> (1648)	-2751 (3222)	-184.4 (657.8)	1076 (1533)	157.4* (82.95)	-81.77 (69.70)
Independent Judiciary	-38,217 (25,915)	-1316 (5167)	-112.1 (10,675)	-1766 (2061)	116.9 (4884)	-52.57 (260.1)	-823.4 <sup>***</sup> (234.0)
Civil War	4630 (43,199)	-381.6 (8431)	-12,582 (20,878)	3645 (3736)	3780 (9419)	752.3* (426.8)	297.7 (357.6)
Constant	3.360 × 10 <sup>6*</sup> (1.844 × 10 <sup>6*</sup> )	-404,575 (384,237)	-722,367 (748,714)	-424,816 <sup>***</sup> (158,487)	159,987 (371,270)	-23,561 (19,315)	6224 (17,391)
N	3288	3221	3390	3233	3293	3219	3232
N (Censored)	2368	2221	2714	2285	2415	2233	2402
$\rho$	0.0926	0.0614	0.0448	-0.0450	0.0182	0.0145	0.00950
$\sigma$	202,147	44,185	62,662	16,656	37,783	2190	1664
$\lambda$	18719	2714	2805	-750.2	688.0	31.69	15.80

Standard errors in parentheses. Country fixed effects not shown. Two-tailed significance tests used.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

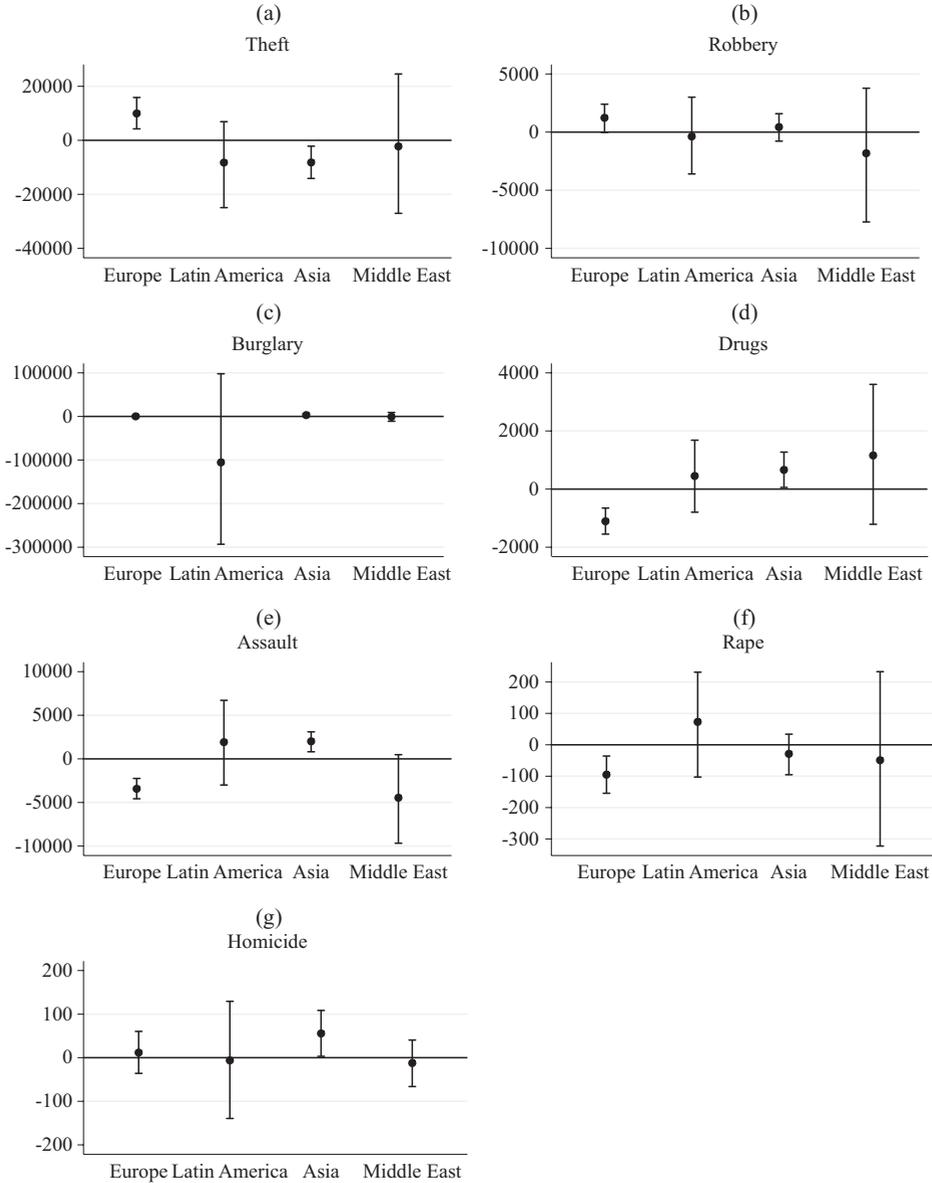


**Figure 1.** Marginal effect of an increase of 500 troops across GDP based on models 1 and 5 in Table 4; 95% confidence intervals shown. Five hundred troops represent an increase in the US troop presence that is approximately equal to one battalion. Europe is used as the geographic area in calculating marginal effects. Values of independent variables held to their respective means, medians or modal values when generating predictions.

For every category of crime the estimates for the effect of troops on crime in Africa, and the corresponding standard errors, are orders of magnitude larger than for any of the other regions.

To the extent that these regions serve as proxies for cultural differences, these results provide little support for the hypothesis that such divides interact with the presence of US troops to increase the occurrence of crime in the host-state. Of the five regions we examine, only Asia exhibits a positive and statistically significant marginal effect for troops across multiple equations (three of seven). Europe, alternatively, is the only region to yield a positive and significant marginal effect in the *Theft* category, while the predicted effect for Asia is negative. As we discuss above, we assume that Europe should act as a baseline for comparison regarding cultural similarities. Although the marginal effect of troops is negative and significant in three categories for Europe, this is relative to null findings for most other regions and crimes. A negative effect for some categories in Europe and a null finding for the remaining regions is not equivalent to finding a significant and positive effect for troops across these other regions. Rather, our findings suggest no discernible link between troops and crime in the majority of region–crime cases. For hypotheses rooted in cultural arguments to be validated, however, we should see a *positive effect* in these remaining regions. As such, our ability to draw any generalizable inferences regarding the idea that the effect of troops on crime should be higher in regions that we might label as being culturally different from the US is limited. At most, there is some weak evidence indicating that troops are associated with increased crime in Asia specifically, and that troops are associated with the decreased occurrence of some crimes in Europe.

Finally, a brief word on the control variables in Table 4. As before, the control variables exhibit significant variation in their association with the seven categories of crime. Still, it is worth noting that we again find that a higher percentage of women participating in the workforce, as well as *ln GDP*, are the most consistently significant control variables. More women participating in the workforce negatively correlates with burglaries and DRC, but positively



**Figure 2.** Marginal effect of an increase of 500 troops on assault across regions based on model 5 in Table 4; 95% confidence intervals shown. Five hundred troops represent an increase in the US troop presence that is approximately equal to one battalion. Africa omitted for scaling and presentational reasons. Values of independent variables held to their respective means, medians or modal values when generating predictions.

correlates with reported assaults and rapes. The *ln GDP* is consistently positive and significant for *Theft*, *Robbery*, *DRC*, *Assault* and *Homicide*. However, given the hypothesized conditional

relationship between troop deployments and *ln GDP*, interpreting these results requires caution as we discuss above.

## Conclusion

Overall, we find mixed evidence that US military deployments are systematically correlated with increased crime in host-states. Although the prior expectation and basic results from the bivariate regression in Table 2 suggest a positive correlation between troops and six of the seven categories of crime that we examine, the results from Table 3 indicate that, once we control for other explanatory factors, the relationship between troops and crime becomes negative or is not statistically significant. Further, the results from Table 4 indicate that the conditional effect of troops on crime across regions is generally statistically insignificant, although positive relationships do appear in some cases—primarily in Asia for DRC, assaults and homicides, and for thefts in Europe.

We do, however, find some evidence that the effect troops have on certain categories of crime is conditional on the host-state's GDP. In particular, less wealthy states appear to experience a larger increase in theft, robbery and burglary resulting from an increase in the size of a US troop deployment than do wealthier states. As the host-state becomes wealthier, this relationship becomes increasingly smaller to the point of statistical insignificance. This finding matches micro-level expectations derived from Humphreys and Weinstein's (2006) work and the macro-level expectations derived from the Durkheimian framework. Based on these approaches, we should expect the incentive for restraint to be lower among troops looking to exploit the local population (micro-level), and we should expect the presence of a large troop deployment to create greater social and economic dislocations where the gap between the US soldiers and the host-state is greater. Alternatively, as indicated in Figure 1b, we find that the interaction between troops and assaults is consistently negative across the displayed range of GDP. Therefore, while troops appear to increase *some types of crime* in poorer states, it also appears that this adverse impact is not constant across all categories of crime.

In sum, we believe that the evidence presented here calls into question the conventional wisdom regarding the negative impact that US troop deployments have on host-states. Given the lack of a clear and systematic relationship between US troop deployments and higher levels of crime within the host-state, it appears as though the accounts that are typically highlighted in the literature are chosen because of their egregious nature, rather than simply as randomly selected examples of a broader population of cases. However, as we have said previously, we do not view these findings as conclusive. Rather, our findings indicate the need to further develop our understanding of the systematic links between foreign troop deployments and criminal activity in the host-state.

We highlight three important points to consider in moving forward. First, aggregation bias is a serious concern. Our study uses national-level data on reported incidents of crime, which can mask sub-national variation in crime as a result of exposure to US troop deployments. Given that troop deployments are often focused around particular geographic areas (i.e. bases), it is possible that greater incidences of crime in areas immediately surrounding troop deployments are washed out in the aggregate if other regions within the host-state exhibit lower incidents of crime. Second, this also suggests the need for country-level analyses. Although our goal was to evaluate broader cross-national trends, country-level studies could utilize more refined data (where available) and modeling techniques. Even in states that host large US troop deployments, like Germany, considerable variation exists with respect to how many troops are deployed, where they are deployed and how those deployments relate to other domestic factors like regional population size, urbanization and unemployment. By taking advantage of better and more refined data, country-level studies of this sort could provide important insights that are simply unavailable through cross-national

approaches. Finally, troop deployments can also have positive externalities, including increased economic activity and employment for citizens in host-states. These positive externalities can mediate the overall negative social impact of deployments, and should be empirically evaluated.

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### Notes

1. We should note that, while the term “crime” can encompass a broad range of activities, we conceptualize crime herein as those acts specifically prohibited by the statutory law of the state, the violation of which can lead to prosecution by the state. Although specific definitions and statutes may vary across states, we focus on those acts that are commonly recognized as criminal across a range of countries, such as assault, theft and homicide.
2. While we will stick with the “Durkheimian” label, other possible labels include Modernization Theory and Institutional–Anomie Theory. All of these labels arise from the same basic theoretical approach of Durkheim (1964).
3. Some studies have begun to incorporate more dynamic variables into their models (e.g. Messner and Rosenfeld, 1997; Neumayer, 2003).
4. Given our use of country fixed effects we shied away from the Full Information Maximum Likelihood version of the model and opted for the two-step model that employs Limited Information Maximum Likelihood. For a detailed discussion of the problems with fixed effects estimation in full information models, see Nijman and Verbeek (1992) and Wooldridge (1995).
5. There are a variety of activities that we could consider crimes associated with a US military presence that are not captured in this research. Various human rights abuses, violations of international law, increased prostitution and environmental crimes committed by or associated with US military forces are not included in these data. Crimes committed on bases are also likely to not be included.
6. For some country–years there may be two separate reported statistics for some crimes. Although there are few such cases, we use the highest reported value in each category for each year. We believe this to be the appropriate step given that errors or manipulation in reporting usually tend to be biased against reporting higher figures.
7. For estimation purposes, we use the aggregate amount of crime. While it is more common to use crime per capita as a measure in the criminology literature, there are estimation problems in doing so. First, we are interested in how population affects crime rates and include both urban and total population as independent variables; estimating these variables on the per capita rates would bias the estimates in a predictable way: there would be a negative relationship between them and the outcome variable since we would be regressing  $y/x = \beta x$ . Second, constraining population to be the denominator in the dependent variable enforces a 1:1 relationship between crime and population and the remaining independent variables. By having population on the left-hand side of the regression as an independent variable, we allow this relationship to be parameterized.
8. This variable is unique from the seven others in that it encompasses several different kinds of crime into one category: cultivation and distribution of drugs exist for different reasons. As such, there are multiple

pathways to causation that make interpretation of this variable difficult. However, we still incorporate it in our models as it is an indicator for criminal activity increasing in the aggregate. If the presence of troops increases the demand for illicit drugs in a society, then there ought to be an increase in all levels of drug-related activity, although this may not be a symmetric increase. Consequently, interpretation of this variable requires more care than the other six categories.

9. Our analyses focus only on total reported thefts and assaults since the criteria surrounding both “major” categories in the UNCTS survey are highly ambiguous. Criteria for inclusion in these categories depend on the legal differentiation between categories of theft and assault within each individual country.
10. Earlier iterations of the UNCTS data provide information on total homicides. In later surveys, where this variable is unavailable, we generate a total by summing intentional and unintentional homicides.
11. This exclusion may appear beneficial to our analysis, but actually does limit some interpretations about the links between troops and crime. Traffic collisions involving US troops for civilians living near military bases do occur, but our research does not allow us to account for those incidents.
12. This data may underrepresent the presence of US military forces in some countries. Deployments related to covert missions are probably not captured by this data.
13. The presence of US troops in a particular state may have consequences for crime in neighboring states. The presence of a large deployment of US troops in South Korea and Japan, for example, has had a large impact on the level of human/sex trafficking in the broader context of the Asia-Pacific region (Moon, 1997).
14. We include Canada in the Europe category given that this is to act as the baseline for cultural similarity. Canada’s participation in NATO also makes it a better fit in the Europe category of culture than in the Americas.
15. Following Brambor et al. (2006) we include all constituent terms in the models.
16. Neumayer (2003) uses the Political Terror Scale to capture repression (Gibney et al., 2010). We employ the CIRI measure as it only captures government action. For our purposes we believe it to be a better proxy for quality of governance than the Political Terror Scale, which picks up violence by non-state actors.
17. The system of equations is over-identified by both rank and order conditions for identification (Bollen, 1989; Greene, 2002). The inclusion of the inverse-Mills ratio, fixed effects, *Troops* variable,  $Polity^2$ , *Percentage of Urban Population*, *Independent Judiciary*, *Total Unemployment*, *Female Labor Participation* and *IMR* is exclusive to the prediction stage. The selection stage exclusively contains variables for previous crime reporting, *ln Urban Population* and the *Time* variable. The estimates produced in the selection stage are identical for both sets of models in Tables 3 and 4.
18. The Appendix is available at <http://www.m-flynn.com/> and <http://www.ma-allen.com/>
19. *Troops* contributions to *Theft* become negative for *ln GDP* values above 13.8, *Robbery* at 14.9 and *Burglary* at 11.4.
20. Marginal effect values were generated using CLARIFY (Tomz et al., 2001a, b).

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