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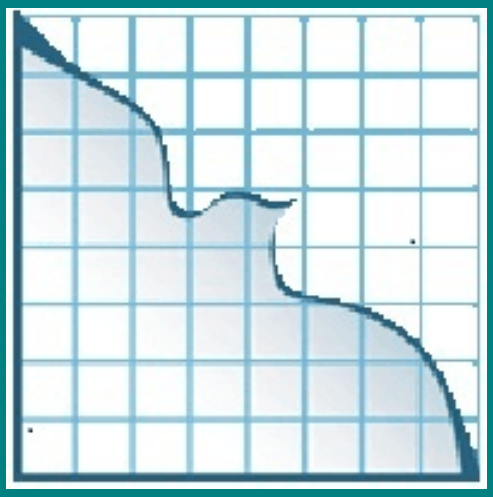

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The effects of terror attacks on happiness: Evidence from Turkey

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Abstract

The economic effects of terror attacks have been extensively examined in the literature. Yet there is a paucity of empirical research investigating their effect on welfare. Existing studies suggest that, in addition to economic costs, terror also imposes social costs. Using both micro-level data at the individual level and macro-level data at the provincial level, this article examines the association of terror and happiness in Turkey. To address income endogeneity, the conditional mixed process estimation method is employed. The results indicate that while a bidirectional relationship between happiness and income level exists, terror negatively affects happiness. Additional factors such as perceived relative income, gender, employment status, and household size have significant effects on individual wellbeing as well. The findings suggest that measuring only the economic costs of terror fails to capture the full extent of the costs imposed on society.

Terror attacks in Europe and the Middle East have created global concern and distress among many nations. Even though acts of terror might be localized, fear of terror affects the lives of everyone anywhere, but especially so in large metropolitan areas. Economic, psychological, and social consequences exist for any country exposed to terror events. Even if not directly affected, spillover effects from neighboring countries and/or to trade partners can matter. Acts of terror may affect economies by a variety of channels such as increased transaction costs, decreased tourism revenues, decreased savings, a decrease in the number of firms and employment, and decreased foreign direct investment. Terror also adversely affects financial markets, although its impact on returns and volatility can be transitory. Counterterrorism may also have a negative effect on economic growth as it entails an increase in security and military expenditure. In addition to its economic effects, terror also carries intangible costs, including victims' pain and despair and the generally increased levels of anxiety and reduced life satisfaction and happiness.[1]

Turkey has been suffering from domestic and international terror attacks for almost fifty years, a large part of which, however, arises from attacks carried out by the Kurdistan Workers' Party (PKK) and is concentrated in the southeastern region of the country. With the intensification of terror in Turkey, claiming the lives of many people and damaging property, the Turkish people have been experiencing major interruptions of daily activities such as delayed commuting in metropolitan areas, closed schools, interrupted education, difficulties in access to health services, and inconsistent work hours. Additionally, there is a significant amount of stress and fear, similar to experiences reported for Israel. Moreover, fear of terror deters people from engaging in ordinary daily activities, such as shopping, so as to avoid crowded places and leading to a decrease in daily economic transactions.[2]

Prior research finds that terror hinders economic growth in Turkey. Yet the impact of terror on the wellbeing of Turkish citizens has not been previously investigated. This article examines this effect by using micro-data at the individual level and macro-data at the provincial level. The micro-data are drawn from the Turkish Statistical Institute's (TurkStat) Life Satisfaction Survey of 2013, where the sampling enables researchers to obtain individual level data. Provincial level data regarding macroeconomic correlates are obtained from TurkStat's regional statistics. Terror events data come from the Global Terrorism Database (GTD) at the University of Maryland. Conditional mixed-process (CMP) models are employed for the empirical analysis. The remainder of the articles is structured as follows: The next section reviews the literature on the effect of terror on individual wellbeing. This is followed by sections covering the data, the analytical framework and estimation method, and the empirical model and estimation results. The final section concludes.[3]

Literature review

In the defense economics literature, terror is often defined as the premeditated use or threat to use violence by individuals or subnational groups against noncombatants to attain political and social objectives by intimidating a large audience beyond

that of immediate victims. Based on ideological, social, and/or political motives, acts of terror are carried out to spread fear so as to compel government officials and politicians to reach an accommodation with the terror perpetrating organization. In Turkey, a number of national and international organizations have been contributing to terror. Until recently, the PKK was the major such organization ,from the 1980s onward, aided by the Islamic Great Eastern Raiders/Front and the Turkish Workers' and Peasants' Liberation Army (TIKKO).[4]

Even though the roots of ethnicity-based terror have been attributed to regional inequalities prevalent in Turkey, unrest in the Middle East, especially in Iraq and Syria, has contributed to the escalation of ethnic-based terror in southeastern Turkey since the early 2000s. In an attempt to solve the conflict peacefully, Turkey's ruling Justice and Development Party initiated a peace process in 2009. Meetings between Turkish government representatives and PKK leaders were held in Oslo in 2012 (which later became known as the Oslo Process). Subsequently, a decline in the number of PKK-initiated terror events was observed between 2012 and 2013. Since then however, terror acts committed by the PKK, and met by Turkish security forces' military operations, have interrupted the Oslo Process.[5]

Research agrees that terror hampers Turkish economic growth. For example, applying nonlinear econometric methods to the 1987–2004 period, Araz-Takay, Arin, and Omay (2009) report a large, statistically significant impact, which is especially pronounced during expansionary periods. Ocal and Yildirim (2010) employ provincial level data and perform a regional effects analysis with a geographically weighted regression approach. They, too, report that terror negatively affects economic growth across Turkey. However, the adverse effects are more accentuated in the southeastern provinces, where most of the terror activity has been concentrated. Bilgel and Karahasan (2013) explore effects on real GDP in terror-stricken eastern and southeastern Turkey, 1975–2001, using the synthetic control method. They find an average real GDP gap of about 7 percent between the actual (with terror) and synthetic control (without terror) of eastern and southeastern Anatolia.

Turkey is a popular tourist destination, attracting more than 25 million foreigners in 2016 and, according to TurkStat data, generating total revenue of USD31.4 billion. The world's 6th-most visited country in 2015, Turkey's tourism sector is a source of foreign exchange reserves, creates employment, and leads to economic growth. However, in addition to a slew of international conflicts in the Middle Eastern region, acts of terror in Turkey adversely affect its tourism revenue. Accordingly, another strand of the literature specifically

Using micro-level data at the individual level and macro-level data at the provincial level, the article examines the association of terror and happiness in Turkey. To address income endogeneity, the conditional mixed process estimation method is employed. Estimation results indicate that terror negatively affects happiness and that a bidirectional relation between happiness and income level exists. Additional factors such as perceived relative income, gender, employment status, and household size have significant effects on individual wellbeing as well. The findings suggest that measuring only the economic costs of terror fails to capture the full extent of the costs imposed on society.

examines the effect of terror on tourism in Turkey. From studies beyond Turkey, it is known that terror events generally have long-lasting negative impacts—even after stability is reestablishment—and a study on Turkey similarly suggests that negative effects can be observed up to 10 months following an attack. Drakos and Kutan (2003) examine the issue for three countries in the Mediterranean region with a high incidence of terrorism: Turkey, Israel, and Greece. Their findings support the results stemming from single country studies in that acts of terror significantly hamper tourist arrivals. Moreover, terror events, in any country, not only affect the domestic economy but also have significant spillover effects on tourism markets of neighboring countries, thus leading to a region-wide loss in economic activity.[6]

Beyond tourism, the effects of terror on other sectors and the overall economy have been assessed by researchers who find that terror leads to increased production and transaction costs, decreased savings, and decreased foreign direct investment, all of which result in significant costs and which, in turn, causes reduced economic growth. However, the total social and political impact of terror is difficult to estimate. Terror and counterterrorism may have psychological consequences for individuals, impairing the daily life of citizens who are not only directly affected by terror but also of those who are indirectly affected. Post-traumatic stress disorder and depression are common among individuals who live in countries exposed to terror attacks. All of these consequences reduce social welfare. And yet, there is a relative lack of empirical research investigating the effect of terror on welfare.[7]

Frey, Luechinger, and Stutzer (2009) estimate the cost of terror on life satisfaction in regions of France, the British Isles, and Northern Ireland. They report statistically significant negative effects of terror and positive effects of income on life satisfaction. Similarly, Romanov, Zussman, and Zussman (2012) study the effect of terror on the happiness of Israelis between 2002 and 2004 (during the Intifada), revealing differences in happiness levels of Jewish and Arab Israelis:

While terror fatalities do not significantly affect happiness levels of Jewish Israelis, they do (negatively) affect its Arab citizens. Bryson and MacKerron (2018) find that terror events such as killings, bombings, shootings, incendiary attacks, and assaults adversely affect individuals' *momentary* happiness and anxiety. However, the magnitude of the negative effect depends on the type of the terror act as well as on individuals' time and place proximity to the event. Vorsina, *et al*. (2017) also investigate the social costs of terror. Employing cross-country data from 117 countries covering the time period 2006–2011, they explore the direct relation between individuals' life satisfaction and terror and the indirect effect of terror acts on life satisfaction through its impact on national income. They find that acts of terror directly causes lower life satisfaction and indirectly as well through the channel of reduced national income.

Prior research on the determinants of life satisfaction in Turkey examines the relation between subjective wellbeing and socio-demographic factors. Selim (2008) finds that health, income, and employment significantly affect happiness and life satisfaction, and Ekici and Koydemir (2014) reveal a relation between happiness and various aspects of social capital. Among comparative studies, Dumludag (2013) finds that increases in household consumption levels and income significantly improve life satisfaction. Caner (2014) suggests that in addition to absolute income, favorable income comparisons to others enhance levels of happiness in Turkey, but the effects vary with business cycles. Dumludag, Gokdemir, and Giray (2016) and Caner (2014) point out that the relative standing of income level is a predictor of individuals' life satisfaction. Moreover, household income, being a housewife, being retired, and living in rural areas are positive correlates of happiness. Yet none of the existing studies shed light on the link between terror and happiness in Turkey, which is what this study contributes to the literature.[8]

The data

Consisting of interviews with 196,203 individuals aged 18 and older, and belonging to 125,720 households, the 2013 Life Satisfaction Survey (LSS) conducted by the Turkish Statistical Institute (TurkStat) was the first to reveal data at the province level. Individuals' answer to the question "Thinking about your life as a whole, how happy would you say you are?" constitutes the self-reported happiness variable. Answers were given on a scale of one to five (1 completely happy; 5 completely unhappy). To align with other studies, answers were inverted (1 completely unhappy; 5 completely happy). The monthly household income variable consists of 5 response categories, ranging from income of less than USD1,000 to USD2,950 or more. Additionally, respondents' perception of their own income were captured by asking the question "Imagine a 10-step ladder, on the bottom of which, on the first step, stand the poorest 10% people in Turkey, and on the highest step, the 10th, stand the richest 10% of people in Turkey. On which step of the ladder are you?" Data and sources are presented in Table A1 in the Appendix.

Descriptive statistics are presented in Table A2, where the happiness variable is grouped into three categories: Happy (completely happy + happy), neither happy nor unhappy, and unhappy (completely unhappy + unhappy). The mean happiness score is 3.56 with a standard deviation of 0.86. Some 60.3 percent of the sample report that they are happy; 10.9 percent report unhappiness. Higher proportions of females (62.1%) and married (62.5%) individuals report that they are happy, compared to males (58.0%) and unmarried (53.4%), respectively. The youngest and oldest groups in the sample exhibit higher proportions of happy individuals. University graduates have the highest proportion of happy individuals (62.5%) whereas the illiterate group has the highest proportion of unhappy people (15.8%). The unemployed are more likely to be unhappy as compared to the employed. It appears that money brings happiness since the percentage of happy people rises as the level of income increases.

Data relating to terror events are gathered from the Global Terrorism Database (GTD). Although the GTD data provides information on several items related to terror, it does not cover actions of states and the relationships between activities of separatist groups and reactions of states. Figure 2 presents the number of terror events by attack type. During the time period under consideration, 2000–2013, there were a total of 509 attacks, 54.6% of which were bombings and explosions, followed by armed assaults (21.6%). The number of attacks reached its peak in 2012. The total number of victims were 546 killings and 538 fatalities, that is, averaging slightly more than one dead and one injured person per event. The majority of targets were police and military facilities (35.2%), and 13.2% of targets were on private businesses. Another nearly 15% of targets were on private citizens and properties. Educational and governmental institutions were also among the targets with 6.3% and 13.2% of the attacks, respectively. Approximately 7% of total incidents were directed to transportation facilities.

A terror index is constructed by principal component analysis for the years 2010, 2011, 2012, and 2013. It includes four variables: the number of terror incidences, fatalities, injuries, and the presence of property damage in each province for the given year. The indices are then rescaled to lie between 1 (lowest level) and 10 (highest level). An average index is calculated as the arithmetic average of the indices across the

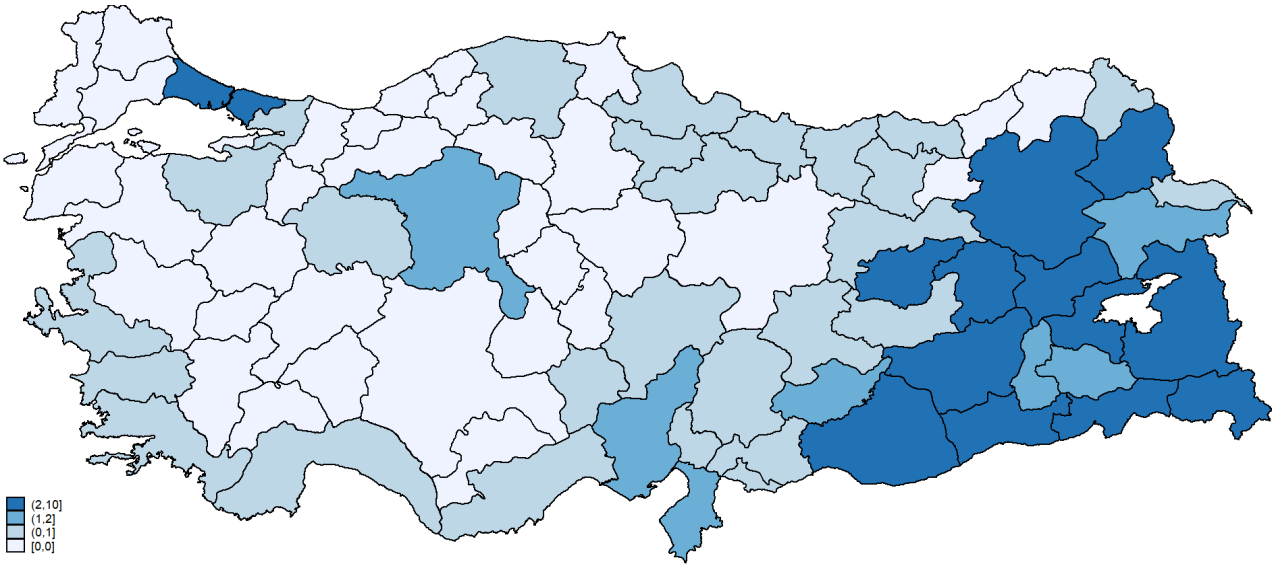

Figure 1: Spatial distribution of average terror index, 2010–2013. *Source*: GTD (2016).

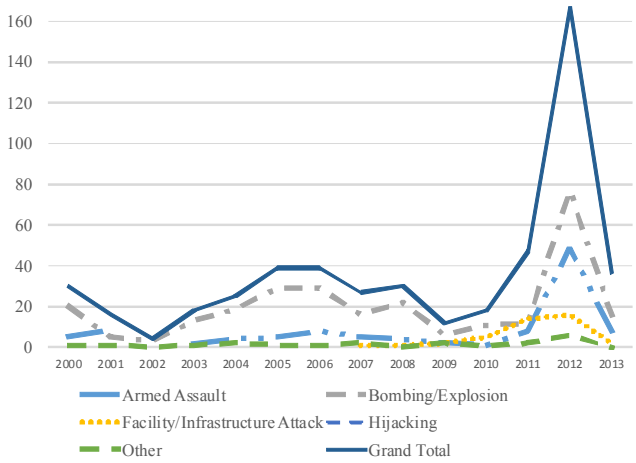

Figure 2: Number of terror incidents by type of attack. *Source*: GTD (2016).

corresponding years and subsequently used in the empirical models. A major characteristic of terror in Turkey is its geographical dimension (see Figure 1).[9] Attacks and fatalities are concentrated in Eastern and Southeastern Turkey and in the major cities.

Analytical framework and estimation method

Depending on the field of specialization, different estimation methods are employed in the literature to investigate the determinants of happiness (Ferrer-i-Carbonell and Frijters, 2004; Kristoffersen, 2010). Generally, psychologists and sociologists prefer to employ Ordinary Least Squares (OLS) regressions, treating happiness by implicit assumption as a cardinal variable. This has been criticized by economists who argue that the subjectivity of happiness hinders an assessment of the realism of the cardinality assumption (Ferrer-i-Carbonell and Frijters, 2004; MacKerron, 2012). Economists, by relaxing the assumption of cardinality, thus generally employ standard ordered probit and logit models (van Praag, 2007), which treat ordinal data as a discrete expression of the continuous latent variable of arbitrary scale (Blanchflower and Oswald, 2004b). Yet results obtained from models that do and models that do not assume cardinality are usually extremely similar (MacKerron, 2012). Some studies employ both methods to demonstrate that their results are not biased by the particular analytical technique (Stevenson and Wolfers, 2009). For instance, models which impose cardinality provide results similar to ordered choice models such as logit and probit (Ferrier-i-Carbonell and Frijters, 2004; Blanchflower and Oswald, 2004b; MacKerron, 2012).

To account for ordinal comparability in rated happiness, our models are intended to be estimated primarily by ordered probit. However, to address the issue of endogeneity of income in the determination of happiness and interdependence leading to unobserved heterogeneities, we employ a conditional mixed process (CMP) model where the correlation between the error terms of happiness and income is estimated as an auxiliary parameter. The CMP estimator is an alternative, more suitable for multiple equations estimations involving different types of dependent and independent variables (Roodman, 2011), and enabling researchers to jointly estimate the system of reduced and structural equations.

We model simultaneously two equations where the model equations can have different dependent variables.

(1) $H_i = X_i\beta + \alpha T_i + \epsilon_{i1}$; $IG_i = X_i\lambda + \epsilon_{i2}$,

where H denotes the ordinal happiness variable, ranging from 1 (completely unhappy) to 5 (completely happy), and IG denotes income group, ranging from 1 (USD0–USD350) to 5 (USD2,950+). The error terms are assumed to be bivariate normally distributed with zero mean, unit variance, and correlation coefficient ρ. X_i=(X_{1i}, X_{2i}, ..., X_{ki}) is a k×1 vector of covariates, T is the average terror variable, and (β_1, β_2, ..., β_k) and (λ_1, λ_2, ..., λ_k) are parameter vectors to be estimated.

The correlation between the two equations' error terms would capture any interdependence of unobserved components in subjective life satisfaction and income. If the error terms of both equations are affected by similar components, they will not be independent, leading to inconsistent parameter estimates in univariate models. The Wald test, and/or Lagrange Multiplier test, provide evidence on the correlation between unobserved explanatory variables that affect both equations. A conditional maximum likelihood estimation approach, which imposes appropriate restrictions on the correlation structure between the errors of the two equations, can be employed to attain consistent and efficient estimates. Roodman's (2009, 2011) novel mixed-process model deals with the endogeneity problem and obtains efficient estimates. Roodman (2011) proposes a general tool implemented on Stata software and using the CMP algorithm to estimate a limited information maximum likelihood.

Estimation results

As self-ratings of individual's overall happiness are measured by an ordered categorical variable, existing studies have generally employed single equation standard ordered response models to analyze its determinants. Since these studies consider various forms of subjective wellbeing and income level as independent achievements, or choices, they fail to account for any interdependency there might exist between these variables. But extensive evidence from by cross-sectional and panel survey data studies suggests that higher income is in fact associated with elevated levels of happiness and life satisfaction (Blanchflower and Oswald, 2004a; Diener, Diener, and Diener, 1995; Ferrer-i-Carbonell and Frijters, 2004; Frijters, Haisken-Denew, and Shields, 2004; Kahneman, *et al*., 2006; Stevenson and Wolfers, 2009). And yet, Easterlin's pioneering work shows that a rising individual *absolute* income level alone does not uniformly increase happiness. This Easterlin Paradox effect occurs because an individual`s income in comparison to others' income—the *relative* income level—may not have not changed (Easterlin, 1974; 1995; 2001). Subsequent empirical research agrees that absolute income enhances happiness but that its marginal impact on self-reported happiness decreases when individual income increases. In addition to this direct effect, income also affects happiness through social comparisons with certain reference groups (Clark, Frijters, and Shields, 2008; Dumludag, Gokdemir, and Giray, 2016; Wolbring, Keuschnigg, and Negele, 2013). Thus, only relative income is significant for life satisfaction (Mentzakis and Moro, 2009). These mixed findings pave the way for further research on the income-happiness relation.[10]

Random disturbances affecting subjective wellbeing such as happiness, life satisfaction, and income level may be correlated and, thus, interdependent. Neglecting unobserved heterogeneity will result in personality bias in empirical estimates.[11] Earlier studies reveal that happy people are more likely to have a number of positive psychological traits. Thus, they are more productive and successful in their professions leading to improved workplace outcomes and higher satisfaction levels with their jobs as compared to unhappy people (Boehm and Lyubomirsky, 2008; Judge and Ilies, 2004; Mignonac and Herrbach, 2005). Moreover, individual characteristics have an impact on both happiness and income determination. Individuals who are extravert and resilient are more likely to be happy and to earn more (Boehm and Lyubomirsky, 2008; Lyubomirsky, King, and Diener, 2005). Furthermore, some individuals may need to work in unfavorable conditions, long hours, and spend time away from their homes and loved ones, all of which negatively affect their happiness. In such cases, estimating standard ordered response models would inappropriately constrain the correlation between random disturbances to be equal to zero, implying that any randomness affecting happiness is unrelated to income level. This constraint can be relaxed, however, by jointly estimating equations in the form of a bivariate ordered response model, which contains an extra parameter to account for any correlation across equations similar to a seemingly unrelated regression model.

The empirical results of our conditional mixed process estimation for the two-equation system in equation (1) are presented in Table A3. While the dependent variables are self-rated happiness and income groups, the main explanatory variable is the average terror index of each province for the 2010–2013 period.[12] Additionally, a provincial level variable—to control for ethnicity (the percentage of the Kurdish population in each province)—is included in the empirical models. The remaining explanatory variables come from the Life Satisfaction Survey (LSS) and include gender, age, education level, household income, perceived relative income, marital status, health indicator, work status, and immigration

status. The CMP estimation results reveal that the correlation coefficient between disturbances of the two equations (atanhrho) is statistically significant. This indicates that single equation ordered probit estimates fail to capture the association of happiness and income level. The statistically significant positive value of the correlation coefficient (atanhrho) that we find instead suggests that there are some unobserved factors that positively impact happiness and income variables.

The estimation results for the happiness equation, presented in Table A3, indicate that terror has a statistically significant negative impact on happiness, and thus supports earlier evidence (Frey, Luechinger, and Stutzer, 2007; Vorsina, *et al*., 2017). The existing literature suggests that ethnic diversity is associated with life satisfaction (Barger, Donoho, and Wayment, 2009; Algan, Hemet, and Laitin, 2016). Since terror in Turkey is generally concentrated in its southeastern provinces where a high number of Kurds live, the explanatory variables also include an ethnicity variable which is the percentage of Kurdish population in each province as calculated by Mutlu (1996). The results reveal that having a high percentage of Kurdish population in a province hinders happiness. However, its negative impact vanishes when regional fixed effects are introduced into the model (Model II). The great majority of existing research reports a positive association between *absolute* income and happiness or subjective wellbeing, although its impact is smaller as when compared to variables such as marriage and unemployment (Ferrer-i-Carbonell, 2013). Yet, the evidence regarding the relation between happiness and *relative* income is mixed (Verme, 2018). A strand of the literature using panel data for developed countries reports a negative relation between self-reported happiness and income of a reference group (Helliwell, 2003; Stutzer, 2004; Vendrik and Woltjer, 2007). However, a positive association between perceived income and happiness is reported for a number of less developed countries (Dumludag, Gokdemir, and Giray, 2016; Knight, Song, and Gunatilaka, 2007; Knight, Shi, and Song, 2006; Stutzer, 2004). Results presented in Table A3 reveal that the level of perceived relative income has a positive impact on self-reported happiness in Turkey. Individuals are happier when their income is higher than the income of the reference group. This result is consistent with previous findings for Turkey reported by Dumludag, Gokdemir, and Giray (2016).

According to the findings presented in Table A3, the education variables are not statistically significant for happiness levels. Previous studies report mixed results for the relationship between education level and life satisfaction. While Dumludag (2013) and Kangal (2013) find a positive education effect, Selim (2008) reports that education is not a statistically significant determinant of happiness. Recently, Dumludag, Gokdemir, and Giray (2016) provide empirical evidence for a U-shaped relationship between education and happiness. The endogeneity of income could be one of the reasons for the mixed empirical findings with respect to the education variable. Even though education may have an impact on happiness, it is not a direct effect. Rather, education fosters happiness indirectly through its positive effects on income levels. Estimation results from the CMP model of the income equation, given in Table A3, suggest that higher levels of education lead to an increase in income which then enhances individuals' happiness. This could be due to the fact that enhancing human capital enables people to get better jobs with higher earnings.

With respect to socio-demographic variables, our results indicate that self-reported happiness follows a U-shape over age, which is consistent with the literature (Blanchflower and Oswald, 2008; Cheng, Powdthavee, and Oswald, 2015; van Landeghem, 2012; Stone, *et al*., 2010). For Turkey, the literature either reports a negative (Ekici and Koydemir, 2014; Selim, 2008) or a U-shaped (Caner, 2014; Dumludag, Gokdemir, and Giray, 2016) impact of age on happiness. Our study here indicates that happiness falls with age and reaches a minimum at age 51, which is very close to the age 55 marker reported in the literature and then rises again (Blanchflower and Oswald, 2004a; Frijters and Beatton, 2012). Gender differences in happiness exist and our estimation results imply that females are happier than males, also consistent with previous evidence (Caner, 2014; Cordero, Salinas-Jimenez, and Salinas-Jimenez, 2017; Ekici and Koydemir, 2014; Selim, 2008). Being married enhances the likelihood of being happy, again supporting earlier findings (Dumludag, Gokdemir, and Giray, 2016; Ekici and Koydemir, 2014; Mentzakis and Moro, 2009; Stutzer and Frey, 2006). Stack and Eshleman (1998) suggest three intermediating processes by which marriage or cohabitation may positively impact happiness: Marriage may enhance financial resources, stimulate better physical health, and/or yield greater emotional support. Stutzer and Frey (2006), however, claim that happier people are more likely to get married, and hence there is a bidirectional relationship between them.

Considering work status, being unemployed or disabled hinders happiness. However, retirees and homemakers are more likely to be happy when compared to employed individuals. Employment is generally considered to provide basic financial resources, social contacts, social status, and identity within society's institutions and networks (van der Meer, 2014; Warr, 1982). Thus, unemployment is expected to be negatively related to happiness, a proposition empirically

supported by longitudinal and by cross-sectional research (Ferrer-i-Carbonell and Frijters, 2004; Stam, *et al*., 2016; Winkelmann and Winkelmann, 1998).When unemployed, an individual loses both financial and social benefits. Winkelmann and Winkelmann (1998) argue that the nonpecuniary costs of unemployment are greater than the pecuniary costs resulting from loss of income. On the one hand, loss of financial resources during unemployment may inhibit people from planning their future and fulfilling various psychological needs, and it may even lead to poverty (Shields and Price, 2005). Loss of nonpecuniary benefits, on the other hand, may lead to social exclusion. Losing nonpecuniary benefits may help explain happiness levels of individuals who belong to another work or employment status, such as retirees, homemakers, disabled people, and students (Stam, *et al*., 2016).[13] An ability to compensate for a lack of nonpecuniary benefits determines the degree of wellbeing. Hence, homemakers are expected to follow employed people in terms of happiness as the family sphere may sufficiently compensate them for any lost work-related nonpecuniary benefits. Students also have a social environment and they can create identities and activities that compensate for any lost benefits (Calvo, Mair, and Sarkisian, 2015). Similarly, retirees may likewise benefit from a family environment, which makes up for any lost benefits of employment. In contrast, the unemployed and disabled may have some difficulties compensating for the lack of nonpecuniary benefits through other activities (Stam, *et al*., 2016; Strandh, *et al*., 2013). Our own results provided in Table A3 are in line with the existing literature, except for students. Being a student hinders happiness in Turkey.

Accumulated evidence indicates that the better an individual's physical and psychological health, the happier (s)he is on average.[14] Bloom and Canning (2000) argue for a two-way causality between health and income. Healthy people tend to be more productive and are more likely to invest in human capital, hence they are more educated. Additionally, since they are more likely to live longer they also tend to invest in physical capital. Accordingly, any improvement in health status elevates happiness. Moreover, chronic diseases and specific conditions such as heart attacks and strokes reduce life satisfaction (Dolan, Peasgood, and White, 2008). Our findings are in line with this literature that being unhealthy in the previous year reduces happiness (Dumludag, 2013; Peiro, 2006; Selim, 2008; Stam, *et al*., 2016).

Considering only economic motives for migration, one may assume that life in wealthier countries/provinces bring more happiness and prosperity. However, the bulk of the evidence in the literature generally suggests that migrants are less happy than natives in destination countries even though there has been a rise in their income levels (Baltatescu, 2007; Bartram, 2013; Knight and Gunatilaka, 2010; Koczan, 2016). Several factors may contribute to this finding such as underemployment, discrimination, extended separation from close family, and isolation (Bartram, 2013). Similar findings are reported in research on internal migration for China (Knight and Gunatilaka, 2010), Thailand (Jong, Chamratrithirong, and Tran, 2002), for Germany (Nakazato, Schimmack, and Oishi, 2011), and Britain (Nowok, *et al*., 2013). Our findings suggest that immigrants are *more likely* to be happy as compared to natives in Turkey, supporting the findings of Melzer (2011) and Switek (2016), even though there appears to be decline in their household income. Melzer (2011) finds that migrants, moving from East to West Germany, have improved their life satisfaction levels. Switek (2016), however, states that migration's impact on happiness depends on the reason for moving, and only individuals who migrated for better employment opportunities experience an increase in life satisfaction which lasts 6 to 10 years after their move. The latter two papers study fairly homogenous groups of migrants, and that could be the case for Turkey as well. Our finding that internal migration reduces income is in line with Tunali (2000) who reports that nearly 75 percent of migrants in Turkey realize net negative monetary returns over the period 1963–1973.

Regarding the remaining variables for the income level equations in models I and II, it appears that as the percentages of females, disabled, and students in the household increases, there is a decline in household income level. However, an increase in the percentage of income earners leads to a rise in household income level. Additionally, Model II in Table A3 controls for regional differences in social and economic conditions not accounted for by the other variables. There are 12 NUTS (the Nomenclature of Territorial Units for Statistics) Level-1 regions in Turkey. The reference region in the analysis is Istanbul. Although coefficients are not reported to conserve space, all regional dummy variables are negative and statistically significant indicating that compared to Istanbul all other regions have lower self-reported happiness levels. The coefficients of all control variables which are significant in Model I remain statistically significant and are quite similar is size. However, the size of the negative impact of terror on self-reported happiness is elevated when regional differences are taken into account.

Conclusion

Terrorism can be classified as a subset of human-caused disasters (Colletta, 2004; Goldfrank, Panzer, and Butler, 2003) which in addition to causing material damage, can have a

particularly devastating impact on psychological functioning. Goldfrank, Panzer, and Butler (2003) state that terrorism may have a greater impact than other disasters on distress responses, behavioral change, and psychiatric illness due to the unique characteristics of terror events. Previous evidence suggests that terror has economic, psychological, and social consequences. While the economic costs have been well-documented, evidence for any welfare costs is limited. This study thus investigates the impact of terror on self-reported happiness for Turkey, which has been suffering from terror acts for almost fifty years. Acts of terror have been localized in the southeastern provinces and in major cities, and their frequency and severity has been increasing in recent years. In addition to claiming many lives and damaging property, terror spreads fear, uncertainty, anxiety, and anger, which collectively lead to changes in daily activities of people because of the unpredictability of such attacks. Understanding the extend of any welfare impacts of terror is important and may shape efforts to develop intervention strategies to lessen adverse psychological effects.

Employing a unique dataset which combines micro-data at the individual level and macro-data at the provincial level for Turkey, this article makes two major contributions to the literature. First, it analyzes the impact of terror on self-reported happiness in Turkey. Second, it contributes to the happiness literature by jointly estimating self-reported happiness and income group variables, taking the potential endogeneity of the latter into account. For this purpose, it employs a conditional mixed process (CMP) estimation method, where the correlation between the error terms of happiness and income models is estimated as an auxiliary parameter.

The empirical findings suggest that terror diminishes self-reported happiness, supporting earlier findings (Frey, Luechinger, and Stutzer, 2007; Romanov, Zussman, and Zussman, 2012; Vorsina, *et al*., 2017; Bryson and MacKerron, 2018). Furthermore, our results indicate that there is a U-shaped relationship between age and self-reported happiness, while marriage, being female, and being healthy enhance self-reported happiness levels. The estimation also suggests that education fosters happiness indirectly through its positive effects on income level. Regarding work status, unemployed or disabled people are less happy compared to employed respondents. While migration elevates happiness, this is achieved at the expense of a lower household income. Regarding the determinants of income level, it appears that as the percentage of females, disabled, and students in the household increases, there is a decline in household income level. However, an increase in the percentage of income earners in the household leads to a rise in income level. Finally, the results reveal that the correlation coefficient between disturbances of the income and happiness equations is statistically significant and positive. This implies that any increase in income level enhances self-reported happiness. In addition to absolute levels of household income, perceived relative income level is also positively related to self-reported happiness of Turkish individuals. Individuals who live in provinces which a higher percentage of Kurds report lower happiness. Moreover, the negative impact of terror on happiness is accentuated when regional variation is considered by including regional indicators.

In all, this study reveals that the cost of terror includes both tangible and intangible costs. The findings suggest that the welfare cost to society is underestimated if one considers only the traditional economic cost of terror.

Notes

1. Transaction costs: Frey, Luechinger, and Stutzer, 2007. Decrease in tourism revenues: Blunk, Clark, and McGibany, 2006; Brian, 2003; Sloboda, 2003; Drakos and Kutan, 2003; Enders, Sandler, and Parise, 1992; Yechiam, *et al*., 2005. Decrease in savings: Fielding (2003). Decrease in the number of firms and employment: Greenbaum, Dugan, and Lafree, 2006. Decrease in foreign direct investment: Fielding, 2004. Financial markets: Chesney, Reshetar, and Karaman, 2011; Aslam, *et al*., 2018; Kollias, Papadamou, and Arvanitis, 2013). Transitory returns and volatility: Kollias, Papadamou, and Stagiannis, 2011. Counter-terrorism activities: Eckstein and Tsiddon, 2004. Intangible costs: Schuster, *et al*., 2001; Vorsina, *et al*., 2017.

2. Southeastern region: Ocal and Yildirim (2010); Yildirim and Ocal (2013). Israel: Shalev, *et al*. (2006).

3. Prior research: Araz-Takay, Arin, and Omay (2009); Ocal and Yildirim (2010). Life Satisfaction Survey: TurkStat (2013). Events data: Global Terrorism Database (2016).

4. Definition of terror: Enders and Sandler (1993, 2000). Until recently: Drakos and Kutan (2003).

5. Root causes: Feridun and Sezgin (2008); Yildirim and Ocal (2013). Peace process: Unal (2016).

6. 6th-most visited: UNWTO (2018). Adverse effect on tourist revenue: Tosun, Timothy, and Ozturk (2003). Another strand: Drakos and Kutan (2003); Yaya (2009). Long-lasting: Enders and Sandler (1991); Enders, Sandler, and Parise (1992). 10 months later: Yaya (2009).

7. PTSD and depression common: Schuster, *et al*. (2001); Schiff (2006); Canetti-Nisim, *et al*. (2009). Social welfare reduction: Frey, Luechinger, and Stutzer (2007).

8. Determinants of life satisfaction in Turkey: Dumludag (2013); Dumludag, Gokdemir, and Giray (2016); Ekici and Koydemir (2014); Selim (2008).

9. For brief accounts of terror events in Turkey, see Ocal and Yildirim (2010); Yildirim and Ocal (2013).

10. See Frey and Stutzer (2002) for a detailed literature review on economics and happiness research.

11. See Powdthavee (2010) for a review.

12. Alternative specifications for the terror variable have been considered. We estimated models using the terror index for 2013 to examine the current effects. Models using of terror index only for 2013 provide similar results. The estimation results also remained robust when alternative definitions of the terror variable were used.

13. See Stam, *et al*. (2016) and Calvo, Mair, and Sarkisian (2015) for a detailed discussion on how work status and subjective wellbeing are associated.

14. See Frey and Stutzer (2002) for a review.

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Table A1: Data, data sources, and variable definitions

Variable	*Definition*
	Provincial level variables
Terror index	Yearly terror indices (2010–2013) are constructed by principal component analysis and include four variables: Number of terror events, number of fatalities, number of injuries, and presence of property damage, by province by year. The empirical model uses the 4-year arithmetic average of the indices.
Number of events	Total number of terror events by province by year.
Number of fatalities	Total number of confirmed victims and attackers who died as a direct result of the event.
Number of injuries	Total number of confirmed nonfatal injuries to both perpetrators and victims.
Property damage	1 = The event led to property damage. 0 = The event did not lead to property damage.
Ethnicity	Percentage of Kurdish population by province.
	Individual level variables
Happiness	Self-reported. Ranges from 1="completely happy" to 5="completely unhappy".
Female	1=Female; 0=Male.
Age	Age of the individual.
Education	No schooling (reference group) Primary education High school University and other higher education
Work status	Working or temporarily laid-off (Reference group=employed) Unemployed Retired Permanently disabled Keeping house Student
Married	1=Married; 0=Otherwise.
Immigrant	1=Person migrated in the previous year; 0=Otherwise.
Health	1=Serious health problem in the previous year; 0=Otherwise.
Perceived income	Income ladder with ranges from 1 (lowest) to 10 (highest).
	Household level variables
Income group	Group 1: < USD1,000 Group 2: USD1,001 – USD1,435 Group 3: USD1,436 – USD2,000 Group 4: USD2,001 – USD2,950 Group 5: > USD2,950
Percentage of females in the household	
Percentage of disabled persons in the household	
Percentage of students in the household	
Percentage of income earners in the household	

Sources: GTD (2016); TurkStat (2013).

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Table A2: Frequency distributions for levels of happiness (in percent)

Variable	*Happy*	*Neither happy nor unhappy*	*Unhappy*
Total	60.3	28.8	10.9
Male	58.0	30.4	11.6
Female	62.1	27.6	10.3
Age group			
18–24	55.1	26.8	8.1
25–34	60.3	30.9	8.8
35–44	55.1	33.8	11.1
45–54	54.5	33.2	12.4
55–64	58.2	28.9	13.0
65+	63.4	23.6	13.0
Education			
< primary school	59.8	24.4	15.8
Primary school	57.3	31.9	10.9
Secondary school	58.4	31.9	9.7
High school or equivalent	59.8	31.2	9.0
University or higher	62.5	29.5	8.0
Marital status			
Married	62.5	28.0	9.5
Not married	53.5	31.3	15.3
Working status			
Working or temporary lay-off	59.5	31.2	9.4
Unemployed	42.1	33.8	24.1
Retired	61.7	28.1	10.2
Permanently disabled	44.2	28.8	27.0
Keeping house	64.0	26.5	9.5
Student	51.0	33.2	15.7
Unhealthy	51.3	29.9	18.8
Income groups			
< USD1,000	56.4	28.8	14.8
USD1,001 – USD1,435	61.4	29.4	9.2
USD1,436 – USD2,000	61.7	30.0	8.3
USD2,001 – USD2,950	64.6	28.9	6.5
> USD2, 950	69.7	25.3	5.0

Source: TurkStat (2013).

Table A3: CMP estimation results

Variable	*Model I*		*Model II*	
	Happiness	*Income level*	*Happiness*	*Income level*
Terror index	–0.00847***		–0.0093***	
	(0.0028)		(0.0045)	
Female	0.0483***		0.0481***	
	(0.0102)		(0.0103)	
Age	–0.0466***	0.0056***	–0.0466***	0.0449***
	(0.0011)	(0.0098)	(0.0011)	(0.00098)
Age squared	0.00045***	–0.00049***	0.00045***	–0.00049***
	(0.00001)	(0.0001)	(0.00001)	(0.00001)
Primary school	–0.0031	0.424***	–0.0065	0.382***
	(0.0083)	(0.0079)	(0.0084)	(0.0081)
High school	–0.0112	1.122***	–0.014	1.082***
	(0.0118)	(0.0095)	(0.0118)	(0.0096)
University/higher ed.	–0.00052	1.973***	–0.0054	1.934***
	(0.0198)	(0.0126)	(0.0198)	(0.0128)
Perceived relative income	0.128***		0.128***	
	(0.0018)		(0.0018)	
Married	0.320***		0.322***	
	(0.0085)		(0.0085)	
Unhealthy	–0.237***		–0.237***	
	(0.0078)		(0.0078)	
Unemployed	–0.336***		–0.337***	
	(0.0154)		(0.0015)	
Retired	0.0401***		0.0369***	
	(0.0133)		(0.0133)	
Student	–0.0809***		–0.0836***	
	(0.0227)		(0.0228)	
House keeping	0.055***		0.0545***	
	(0.0122)		(0.0122)	
Disabled	–0.229***		–0.226***	
	(0.0209)		(0.0209)	
Ethnicity	–0.00054***		0.00022	
	(0.00016)		(0.00025)	
Immigrant	0.0889***	–0.0397***	0.0854***	–0.033**
	(0.0206)	(0.0153)	(0.0206)	(0.0156)
% disabled in household		–1.408**		–1.380***
		(0.0397)		(0.0398)
% students in household		–0.173***		–0.174***
		(0.0281)		(0.0281)
% income earners in household		1.365***		1.316***
		(0.0125)		(0.00127)
% females in household		–0.744***		–0.766***
		(0.0152)		(0.0153)
atanhrho	0.0779***		0.0753***	
	(0.0036)		(0.0036)	
Regional dummies	No		Yes	
Wald chi-squared	$\chi2(17)=11,873.24$***		$\chi2(27)=12,303.59$***	
Obs N	196,203		196,203	

Notes: Robust standard errors in parentheses; ***p<0.01; **p<0.05; *p<0.10.
Sources: TurkStat (2013); GTD (2016).

Conflict determinants in Africa

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Abstract

This article considers the determinants of conflict in Africa. It revisits the greed versus grievance debate to consider the specific regional context and changing nature of conflict in Africa. This is a literature that has grown rapidly in economics and political science, but some recent developments in modeling and conceptualization are providing important new contributions. The article uses the zero-inflated ordered probit technique that deals with the problem of excess zeros in datasets, revisits the definition of conflict, and improves upon some proxy measures. It also considers the substantive as well as statistical significance of the variables. Changes in the technique used provide more support for the influence of grievance terms than given credit for with the usual probit model approach. Both greed and grievance determine conflict in Africa.

Civil war has been commonplace for the past 60 years, but until fairly recently it received little attention from economists. Civil war is not just common; it is also persistent and lasting longer, decade after decade (Fearon, Kasara, and Laitin, 2007). Blattman and Miguel (2010) estimated that, since 1960, some 20 percent of all countries have experienced at least 10 years of civil conflict, often devastating them culturally, politically, and economically. Collier, *et al*. (2003) suggested that the destructive forces could be large enough to explain the income gap between the poorest and richest nations. One could almost see civil war as reversing development, diverting resources from productive activities to destruction and having both, devastating direct costs and opportunity costs from the loss of productive resources (Collier, *et al*., 2003). The actual and potential costs make it important to understand why conflicts start, and the contribution by Collier and Hoeffler (2004), which sought to test two competing theoretical hypotheses concerning the determinants of intrastate armed conflict—opportunity, or "greed", versus grievance—has led to a large empirical literature. Their finding of overwhelming support in favor of the view that rebellion is motivated by opportunity is generally accepted but has become rather more nuanced (Blattman and Miguel, 2010).

As researchers started to accept the general framework, they also examined other potential determinants that had not already been considered. Nowhere is this move beyond greed or grievance more evident than in quantitative studies of conflict prevalence in Africa where the imposition of artificial state borders, living in "bad neighborhoods", and warmer temperatures (increasingly so, in the face of climate change) have come to take center stage as explanatory variables of interest in the econometric models employed in these studies (Hendrix and Glaser, 2007; Burke, *et al*. 2009).

A number of developments have led to a point where there is some value to be gained from revisiting the debate. First, there are obviously more years of data available, more economic shocks, and more conflicts. Second, there have been significant improvements in the operationalization of difficult-to-measure indicators of greed and of grievance (e.g., income inequality, ethnic divisions). And third, there have been developments in the estimation methods available for analysis, in particular the recognition that simple probit, or logit, models do not perform well in situations with a large number of zeroes in the dependent variable, a likely case for civil conflict as, fortunately, many country-year observations are zero (i.e., peace; see Dunne and Tian, 2017).

A brief review of the determinants of civil war literature and the greed–grievance debate is provided in the next section, followed by a discussion and outline of the estimation procedure used, in particular the zero-inflated ordered probit (ZiOP) model. The section thereafter presents variable construction, the data used, and some descriptive statistics, followed by empirical estimates of a greed–grievance model using the usual methods as against the ZiOP model, and with robustness checks. The final section offers conclusions and discussion of policy implications.

Causes of civil conflict

A range of theoretical perspectives inform the analysis of civil wars. These reflect the interdisciplinary nature of the research

and the relatively late involvement of economists. Political scientists focused upon grievance-related determinants of conflict, with theories emphasizing how modernization could lead to disruption of social order, with social and economic change causing the breakdown of social cohesion and alteration of perceptions. A formalization of this perspective was provided by political rational choice theories. These focused on the role of political repression, failing institutions, political transitions, and informational problems, which together with a failure to redress grievances—economic or political—can lead to conflict.

An alternative was provided by constructivist theories, which focused on the social construction of identity, rather than accepting identity as some fixed attribute. Here, political mobilization leads to civil violence, with leaders constructing ethnic and social identity in ways that benefit themselves (Sambanis, 2002).

In contrast, the focus of economists was on "greed", or opportunity-based, determinants of conflict. Grossman (1991) modeled rebellion as an industry, while Hirschleifer (1995) suggested it was possible for rational agents to misperceive opportunities and grievances because of asymmetric information. This perspective suggests that civil conflict onset is linked to the possibility (and ability) of insurgents to make a profit—the greed hypothesis—rather than the result of grievances (Dunne and Coulomb, 2008; Skaperdas, 2008).

Collier and Hoeffler (2004) provided an empirical analysis of these competing hypotheses, suggesting that while political grievances are universal, economic incentives are not, and so are often decisive in the start of conflict. The probability of rebel victory depends on the ability of the incumbent to defend, which is determined by technology (the technology may also be available to the rebels, but is limited) and by military expenditure, to which the rebels do not have access.[1] Factors that influence opportunity (such as finance, cost of rebellion, and military advantage) were statistically significant in determining civil war, while most proxies of grievance (ethnic fractionalization, inequality, and democracy) were insignificant, although population size had an effect and time seemed to heal the damaging effects of conflict. This finding—that opportunity explained conflict risk— supported the economic interpretation of rebellion as motivated by greed (Collier and Hoeffler 2004; 2007).

Around the same time, Fearon and Laitin (2003) developed a different model, a game of insurgency where the size of a rebellion is influenced by government effort and the scale of the initial rebellion. They, too, found that political grievance had little explanatory power, but that state institutional capacity was significant, suggesting that wars are caused by countries having weak institutions. Yet they differed from Collier and Hoeffler (2004) in the interpretation of GDP per capita (reflecting state capacity rather than as an opportunity cost), how civil wars were coded, and using annual data rather than five-year data averages. The two papers (F&L and C&H, in the tables below) had a major bearing on research and debate and led to a large literature that has advanced our understanding on civil conflict telling us what we do know, as well as what we do not (Blattman and Miguel, 2010).

This article revisits the greed versus grievance debate to consider the specific regional context and the changing nature of conflict in Africa as recent developments in statistical modeling and conceptualization are providing important new contributions. In particular, the article use a modeling technique (zero-inflated ordered probit) that deals with the statistical problem of excess zeros in the dataset, revisits the definition of conflict, and improves upon some proxy measures. It also considers the substantive as well as statistical significance of the variables. The results provide more support for the influence of grievance terms than ordinarily found with the usual ordered probit model.

While the general consensus in the literature—that the motivations of greed outweigh those of grievance in explaining civil war onset—remained, the literature continued to develop and improve in a number of areas. First, political scientists questioned the apparent lack of significance of variables that are proxies for objective grievance.[2] This led to efforts being made to improve measurement and to obtain better proxies. This included improvement of natural resource data, better measurement of grievances, such as measures of inequality and the consideration of horizontal and vertical inequality, and better measures of weak institutions (Lujala, Gleditsch, and Gilmore, 2005; Wucherpfennig, *et al*., 2011). Second, some attempts have been made at improving causal identification. The potential endogeneity of GDP to conflict led to the use of rainfall as an instrument, given that it may affect agrarian economies' output, but not conflict. Other attempts have used price shocks and trade shocks in a similar manner. The identification problem remains an issue, mostly due to difficulties in finding appropriate instruments (Blattman and Miguel, 2010; Miguel, Satyanath, and Sergenti, 2004). Third, some attempts have been made to consider possible spillover effects of conflicts, creating conflicts in other countries, with the feedback of refugees keeping conflicts going (Salehyan and Gleditsch, 2006; Dunne and Tian, 2014). Fourth, questions have been raised about measures of conflict and violence. In the past, war tended to be defined as an event in which there were more than 1,000 battle-related deaths (and peace defined as less than this). Initially, this definition was developed for interstate conflicts and then continued in use, even after the

focus shifted to civil conflicts in the post-cold war world. Eventually, this was deemed unsuitable, and an added definition of conflict (more than 25 battle-related deaths), was created.[3]

This article engages with a fifth concern, the estimation method used. An ordered probit model with a zero–one dependent variable for conflict has generally been used, but this includes a lot of zeros (peace years) in the dataset, and these zeroes are unlikely to all stem from the same data generation process. An observation of a year of peace for a country that is in and out of war surely is different to one for a country that is generally at peace. A zero value in a particular year for Botswana, for example, is rather different to one for the DR Congo (Bagozzi, *et al*., 2015).

Data and units of measurement

To operationalize a greed–grievance empirical model, data for a range of variables were collected, following developments in the literature. Two sets of income variables (real GDP in purchasing power parity terms and its per capita growth rate) were taken from the World Bank and Penn World Tables 8.0, as well as the degree of urbanization (the proportion of a country's population living in an urban environment), and life expectancy (in years).[4] The percentage of mountainous terrain in a given country was also considered, as an indicator of military accessibility or safe havens for rebels.[5]

Natural resource dependence was proxied by the percent share of primary commodity exports in GDP[6] but, given the ongoing debates on the measure of natural resource dependence and the type of commodities used, three additional measures were considered. First and second, annual oil production in metric tons and oil exports greater than one-third of total exports were collected as proxies for oil abundance and dependence, respectively.[7] And, third, to distinguish fuel and nonfuel minerals from other primary commodities, a mineral dependence variable was created. A country was considered mineral dependent if its mineral exports constituted 25 percent or more of total tangible exports. Collectively, these variables are our opportunity variables (see Table 1).

Our grievance variables are, for the most part, common to those identified by Fearon and Latin (2003) and Collier and Hoeffler (2004). They fall into three groups: (1) ethnic and/or religious hatred, (2) political repression, or freedom, and (3) horizontal income inequality. As to the first, the most commonly chosen indicator to test for any link between ethnicity and civil conflict is ethnic fractionalization. Measurement of this, in Table 1, is taken from Collier and Hoeffler (2004), with ethnic dominance measured as a binary variable, taking on the value of 1 if the largest ethnic group in a country amounts to 45–90 percent of the population, and religious fractionalization similarly measured.[8] Regarding the second group, data from the Polity IV database was used to measure the degree of political rights, with the variable ranging from –10 (high autocracy) to +10 (high democracy). In the regressions to follow, we include a squared term to allow for nonlinear effects (Hegre, *et al*., 2001). And third, Buhaug, Cederman, and Gleditsch (2014) found that certain indices of horizontal income inequality and political discrimination (LDG, NHI, and PHI in Table 1) performed much better than conventional indicators and these are used in robustness checks for ethnopolitical and economic grievance.[9]

Table 1: Descriptive statistics, means

	Full sample	*Always 0*	*Not always 0*	*Civil war*	*No civil war*
	(See text for units of measurement.)				
OPPORTUNITY VARIABLES					
GDP/capita	7,931	14,069	3,311	3,172	8,699
GDP/capita growth	1.8	2.2	1.6	1.0	1.9
Urbanization rate	46.9	56.0	39.7	40.6	47.9
Life expectancy	61.6	66.2	58.0	59.4	62.0
Mountains	16.38	14.93	18.11	23.16	15.33
Primary commodity exports/GDP	15.6	17.8	13.9	10.9	16.4
Oil production	17,000	13,7000	19,300	19,100	16,700
Oil exports	18.7	15.5	20.8	16.8	18.9
Mineral dependence	49.3	41.5	54.5	55.5	48.4
GRIEVANCE VARIABLES					
Ethnic fract.	63.0	52.1	69.9	77.5	60.1
Ethinic dominance	47.0	48.3	46.7	54.9	45.7
Religious frac.	36.5	36.1	36.6	0.36	0.37
Polity IV	1.13	3.84	–0.73	0.97	1.30
LDG (see Notes)	0.056	0.024	0.081	0.142	0.042
NHI (see Notes)	1.189	1.064	1.278	1.398	1.155
PHI (see Notes)	1.201	1.086	1.287	1.224	1.197

Notes: LDG = largest discriminated (against) ethnic group; NHI = negative horizontal inequality (relative gap between mean national income and income level of the poorest group); PHI = positive horizontal inequality (relative gap between mean national income and income level of the richest group).

The control variables included in our regressions are the standard ones found in the literature (e.g., population and the cold war period; not shown in Table 1 but shown in Tables 2 and 3). The dependent variable, conflict prevalence, takes on three values, namely 0 for all peace year observations, 1 for "minor" conflict years with combat deaths ranging between 25–999 people, and 2 for "major" civil wars with annual battle deaths with 1,000 or more people.

Table 1 shows that the always zero or "complete peace" group has higher GDP per capita (level and growth), greater urbanization, better life expectancy, and more political freedom than the "not always zero" group and also exhibits lower levels of ethnic and religious fractionalization and income inequality. Correlations suggested some association between the income and inequality variables and the likelihood of a country being completely peaceful versus having some experience of conflict. In episodes of civil conflict, GDP per capita (level and growth), urbanization, life expectancy, and political freedom all are lower than in times of peace, while ethnic divisions, income inequality, and substantial amounts of rough terrain are higher for civil war episodes. Interestingly, primary commodity exports (share of GDP) is on average lower for civil war years.

Greed versus grievance revisited

Estimating the probability of civil conflict using an ordered probit gave the results in Table 2. Column (1) gives the results with the ethnolinguistic fractionalization variable used by Fearon and Laitin (2003) and column (2) when this is replaced by the Collier and Hoeffler (2004) measure. Column (3) gives the results when, following Buhaug, Cederman, and Gleditsch (2014), other ethnic discrimination and income inequality measures are introduced instead. (These are denoted as F&L, C&H, and BC&G, respectively.) Taking the opportunity variables first, all six signs for GDP and per capita GDP growth are negative, suggesting that higher income moderates the likelihood of civil war. Primary commodity exports as a share of GDP exert a nonlinear effect on conflict prevalence, first decreasing and then increasing,[10] and mountainous terrain increases civil war risk. The magnitudes, signs, and statistical significance for all these variables are very similar across the three specifications.

As for the grievance variables, political freedom as captured by the Polity IV index is statistically insignificant, and the results for ethnicity vary, conditional on the choice of the ethnic fractionalization variable. When the Fearon and Laitin (2003) variable is used it is significant, but insignificant for the Collier and Hoeffler (2004) variable. Ethnic dominance is significant and positive, but religious fractionalization, while positive, is only significant for model (1). Column (3) shows LDG, the largest discriminated against ethnic group, as a proxy for ethnic and political inequality, to be positive and highly significant. NHI, the measure of negative horizontal inequality, is positive and statistically significant at the 1 percent level as well (suggesting that African countries with one or more ethnic groups that are radically poorer than the national average have a higher risk of conflict onset), and PHI, the positive horizontal inequality, to be negative and significant.[11] The log of population control variable is positive and significant for all specifications, but the cold war dummy variable is negative and significant only for specification (3).

In most analyses of the determinants of civil conflict, an ordered dependent variable is used, in which a given country-year is assigned a value of 0 for peace and a value of 1 when violence between the state and another side reaches a given threshold, thereby classifying it as a civil war. Since peaceful years dominate conflict years, a very large number of zero observations are in the dataset. These can reflect rather different states of peace, however, namely, one where structural and societal forces ensure a zero probability of civil conflict regardless of greed or grievance incentives or another that captures a mere interlude in fighting and a high probability of returning to conflict.

In the first group will often be states such as Botswana, which can be labeled as "complete-peace." The second group contains states in regions such as Central, West, or East Africa and can be labeled as "incomplete-peace." (Boulding, 1978, might call the groups "stable peace" and "unstable peace"). The main difference between the first and second case of zeroes is that while the probability of transition into war for the first type is zero, the probability for the second group is not zero. In the latter case, incentives resulting from opportunity and/or grievance can induce violent conflict.

Given the high proportion of heterogeneous zeroes in the analysis, using conventional probit, or logit, models may not be appropriate tools for statistical inference and can potentially give biased estimates (Bagozzi, *et al*., 2015). In such cases, a more satisfactory estimation method is the split-population or two-part model proposed by Harris and Zhao (2007) and Vance and Ritter (2014). This is typically done in the form of zero-inflated models or, in our case, a zero-inflated ordered probit (ZiOP) model, where estimations follow two stages. The first is a selection or *inflation equation*, which splits the observations into two processes, each potentially having different sets of explanatory variables. In the context of civil war prevalence, zero observations in process 0 (w_i=0) include inflated zeroes, consistent with countries that never experience civil conflict (e.g., Botswana), while zero observations in process 1 (w_i=1) includes cases for which the probability of

transitioning into a civil conflict is not zero, even if civil war casualties have not reached the lower bound (or limit) of 1,000 battle-related deaths. The binary variable, w, thus indicates the split between process 0 (with w_i = 0 for no war) and process 1 (with w_i = 1 for war). A second stage estimates the ordered probit *outcome equation*, conditioned on the first stage. A fuller exposition of the model is provided in the Appendix.[12]

Compared to standard probit or logit models, the ZiOP model allows more accurate estimates to be obtained but it should be noted that the usefulness of the model (i.e., unbiased estimates) declines when the size of the split in the sample population becomes very big or very small, leading to biased results.[13] Bagozzi, *et al*. (2015) suggest that this becomes an issue when there are less than 10 percent or greater than 90 percent of zero observations. In our case, the zero observations comprise about 76 percent of the dataset.

For the Fearon and Laitin (2003) measure of ethnic division, the results of two specifications of the ZiOP model are given in Table 3. In the first, the inflation equation is limited to GDP (level and growth), political freedom (Polity IV), and ethnic fractionalization as these factors promote interest compatibility between the state and its citizens, which in turn influences the probability that a country is in the always zero group and always experiences peace. That said, to ensure that the ZiOP estimates are not driven by choice of variables, a second specification includes all covariates in the outcome equation in the inflation equation as well. This second specification is used to check that the results do not change markedly when the specification of the inflation/selection equation changes. This is to show that the researchers have not simply searched for a specification that "works".[14]

Table 2: Ordered probit of civil war prevalence, 1960–2013

	(1) [F&L]	(2) [C&H]	(3) [BC&G]
OPPORTUNITY VARIABLES			
log, real GDP	–0.024 (0.052)	–0.137** (0.052)	–0.202** (0.050)
Real GDP per capita growth	–2.496** (0.523)	–2.455** (0.520)	–2.492** (0.527)
Prim. exp./GDP	–5.329** (0.966)	–5.601** (0.978)	–4.091** (0.963)
Prim. exp./GDP squared	7.801** (1.585)	9.045** (1.595)	5.908** (1.597)
log, mountains	0.054* (0.028)	0.118** (0.030)	0.062* (0.028)
GRIEVANCE VARIABLES			
Polity IV	–0.015 (0.032)	–0.018 (0.032)	0.001 (0.031)
Polity IV squared	0.001 (0.005)	0.003 (0.005)	–0.005 (0.004)
Ethno frac. (F&L)	6.022** (0.998)		
Ethno frac. squared (F&L)	–5.529** (0.934)		
Ethno frac. (C&H)		0.011 (0.008)	
Ethno frac. squared (C&H)		–0.001 (0.001)	
Ethnic dominance	0.210* (0.086)	0.292* (0.119)	
Religious frac.	0.967** (0.301)	0.218 (0.275)	
LDG (see Notes)			1.264** (0.168)
NHI (see Notes)			0.859** (0.125)
PHI (see Notes)			–0.172* (0.078)
OTHER VARIABLES			
log, population	0.340** (0.066)	0.413** (0.068)	0.514** (0.679)
Cold war period	–0.024 (0.097)	–0.008 (0.096)	–0.043** (0.104)
Observations	1,519	1,519	1,542
Log likelihood	–941.8	–944.3	–901.8
AIC	1,913.65	1,918.67	1,835.67

Notes: Dependent variable = conflict prevalence (0, 1, or 2); AIC = Akaike Information Criterion;sStandard errors in parentheses; significance levels: ** p < 0.01,* p < 0.05, † p < 0.1; LDG = largest discriminated (against) ethnic group; NHI = negative horizontal inequality (relative gap between mean national income and income level of the poorest group); PHI = positive horizontal inequality (relative gap between mean national income and income level of the richest group).

Looking in Table 3, then, at the first stage or *inflation equation* for specification (1), the results show the GDP variables (level and growth) with a statistically significant negative effect on the likelihood of a country-year not being among the always-zero or peace group and then experiencing any level of civil violence. Additionally, political freedom, measured by the Polity IV index, has the usual nonlinear effect of first increasing the likelihood of civil conflict and then decreasing it past a certain point. Ethnicity also plays an

important role in the different observed zeroes. A higher degree of fractionalization makes it more likely that a country will experience conflict.

The *outcome equation* of the ZiOP specification (1) in Table 3 can then be directly compared to the conventional ordered probit specification (1) in Table 2. While the signs are generally consistent, there are substantial differences as well. Among the opportunity variables, for instance, the coefficient for real GDP under ZiOP is over 10 times larger and the presence of mountainous terrain no longer is statistically significant.

As for the grievance terms, political freedom now is a significant predictor of civil war prevalence and its coefficient estimate is considerably larger. Moreover, the effect is not of the usual "inverse U" shape but decreases throughout. This is an interesting finding. It suggests that *any* improvement in political freedom lowers the likelihood of civil war (albeit with diminishing effect). Fractionalization (ethnic and religious) remains significant, as before.

Table 3: Probit versus ZiOP regressions of civil war prevalence, 1960–2013

	(1) [F&L]		(2) [F&L]	
	Outcome	*Inflation*	*Outcome*	*Inflation*
OPPORTUNITY VARIABLES				
log, real GDP	–0.249** (0.078)	–0.375** (0.095)	–0.269** (0.072)	–1.247** (0.250)
Real GDP/cap. growth	–1.779** (0.669)	–1.722* (0.876)	–3.148** (0.582)	–1.086 (1.535)
Prim. exp./GDP	–8.574** (1.518)		–6.652** (1.366)	–9.256** (2.785)
Prim. exp./GDP squared	12.536** (2.709)		9.957** (2.450)	5.872† (3.617)
log, mountains	0.033 (0.042)		0.341** (0.040)	0.482** (0.067)
GRIEVANCE VARIABLES				
Polity IV	–0.053** (0.014)	0.060* (0.024)	–0.070† (0.040)	–0.013 (0.033)
Polity IV squared	–0.015** (0.003)	–0.008† (0.004)	–0.011* (0.005)	0.036** (0.006)
Ethno fract.	6.882** (1.378)	0.840* (0.380)	2.609* (1.234)	–2.411 (4.334)
Ethno fract. squared	–6.646** (1.265)		–0.284 (1.215)	21.884** (3.509)
Ethnic dominance	0.735** (0.131)		0.686** (0.128)	–3.481** (0.496)
Religious frac.	1.076* (0.510)		–0.196 (0.378)	6.634** (1.607)
OTHER VARIABLES				
log, population	–0.345** (0.117)	1.220** (0.168)	4.126** (0.432)	4.125** (0.432)
Cold war period	0.439** (0.145)		0.445** (0.121)	3.753** (0.491)
Constant	–	–11.212** (1.469)	–	22.239** (4.046)
Observations	1,519		1,519	
Log likelihood	–875.63		–814.56	
AIC	1,795.26		1,631.85	

Notes: AIC = Akaike Information Criterion; Dependent variable: Conflict prevalence; Standard errors in parentheses; Significance levels: ** p < 0.01,* p < 0.05, † p < 0 .1.

Compared to the standard ordered probit model, the ZiOP estimates also have lower standard errors and a lower Akaike Information Criterion (AIC), suggesting that the model better fits the data. As suggested by Cameron and Trevidi (2010), all regressions were estimated using robust standard errors. Again, note that the proportion of zero observations in the sample, at 76.3 percent, falls within the accepted band of 10 to 90 percent (Bagozzi, *et al*., 2015).

To consider the robustness of our results, a number of alternative ZiOP specifications were estimated. Adding horizontal income inequality and ethnic discrimination in place of ethnic dominance and religious fractionalization and replacing Fearon and Laitin's (2003) ethnic fractionalization measure with Collier and Hoeffler's (2004) gave results consistent with Table 3, with the ZiOP model preferred to the ordered probit model in almost all instances.[15] Other tests included replacing primary commodity exports with either mineral dependence, oil production, or oil exports, replacing the Polity IV index with the Freedom House measure, democracy, and autocracy dummies, and substituting income variables with the urbanization rate and life expectancy. The results were fairly robust, with primary commodity dependence increasing civil war risk, and democracy, political freedom, and higher urbanization decreasing civil war risk.

Ward, Greenhill, and Bakke (2010) remind us that

coefficients' statistical significance does not necessarily mean that models predict well, an important concern given the influence of some of the literature's results on policy formulation. To evaluate the predictive power of our models, the receiver operating characteristics (ROC) statistic was used, which takes the estimated probabilities and compares them to the actual values of the conflict variable. Using different thresholds, this finds the number of correctly classified/predicted observations.[16] The ROC can range from 0.5 (a nonpredictive model, no better than chance) to 1.0 (perfect prediction). Since we used ordered probit models, the ROC scores needed to be computed for values of 1 and 2 ("minor" and "major" conflict). As shown in the last two rows of Table 4, our ZiOP model (for the Fearon and Laitin run) resulted in larger ROC scores, namely 0.766 as against 0.716 for the standard ordered probit, when the outcome variable was 1, and 0.872 as against 0.808 when outcome variable was 2.[17] This indicates that with the same specifications, our ZiOP model predicted civil conflict better than the ordered probit.

Another concern raised by Ward, Greenhill, and Bakke (2010) is that variables may be statistically significant and yet not contribute much to a model's predictive power. This can be evaluated by deleting one independent variable at a time and measuring the effect the deletion had on predictive power (that is, the *change* in ROC). Table 4 presents these results. For example, when excluded from the ZiOP model, the Polity IV only decreases its predictive power from 0.766 to 0.763 (a decrease of 0.003) if outcome variable equals 1, and from 0.872 to 0.867 (a decrease of 0.005) if the outcome variable equals 2. Although statistically significant, the Polity IV variable does not appear to provide a substantive contribution to the model.[18]

Table 4: Predictive power and statistical significance, probit versus ZiOP

	Ordered Probit			*Zero-inflated Ordered Probit*		
	p-values	*ΔROC if Outcome = 1*	*ΔROC if Outcome = 2*	*p-values*	*ΔROC if Outcome = 1*	*ΔROC if Outcome = 2*
OPPORTUNITY VARIABLES						
log, real GDP	0.645	–0.002	–0.001	0.001	–0.007	–0.001
RGDPPPC growth	0.000	–0.003	–0.028	0.008	–0.001	–0.004
Pri exports/GDP	0.000	–0.031	0.003	0.000	-0.020	0.003
Pri exports/GDP squared	0.000	–0.021	–0.001	0.000	–0.014	0.002
log, mountains	0.051	–0.005	–0.007	0.420	–0.013	–0.019
GRIEVANCE VARIABLES						
Polity IV index	0.636	0.004	–0.001	0.000	–0.003	–0.005
Polity IV index squared	0.748	0.002	–0.001	0.000	–0.002	–0.008
Ethno fraction. (F&L)	0.000	–0.033	–0.022	0.000	–0.011	–0.004
Ethno fraction. squared (F&L)	0.000	–0.031	–0.021	0.000	–0.013	–0.003
Ethnic dominance	0.015	0.014	–0.011	0.000	–0.004	–0.020
Religious fraction.	0.001	–0.003	–0.09	0.035	–0.009	–0.005
OTHER VARIABLES						
log, population	0.000	0.006	–0.010	0.003	–0.004	0.000
Cold war period	0.803	–0.002	0.002	0.420	0.012	–0.010
Sum		–0.105	–0.107		–0.089	–0.074
ROC AUC if outcome =1		0.716			0.766	
ROC AUC if outcome = 2		0.808			0.872	

Notes: ROC = Receiver operating characteristics; AUC = Area under the curve.

Conclusion

This article revisits the greed–grievance debate within the context of fragility, using a data set of 33 African countries for the period 1960 to 2013. This seemed justified for a number of reasons: the existence of more years of data including more economic shocks and more conflicts, the significant improvements in the operationalization of difficult-to-measure indicators of grievance (i.e., income inequality, ethnic divisions), and the development of a new estimation method that seems well suited to the subject. Estimations using the standard ordered probit technique do not account for the heterogeneous zeroes in the dataset, and an alternative, zero-inflated, model is used that separates out observations of countries with almost no probability of conflict from those of other countries.

The two main results are the following. First, unlike much of the earlier literature, civil war risk is not wholly dominated by greed (or opportunity); the grievance terms are statistically significant. It appears that the matter is not one of a disjunctive "greed *or* grievance," but one of a conjunctive "greed *and* grievance." Second, our zero-inflated ordered probit (ZiOP) models perform better statistically than do the standard probit models and better account for observable and latent factors that

produce different types of peace observations. These results suggest that the standard ordered probit technique results in biased estimates, giving greater weight to opportunity over grievance variables. This has led to most empirical work finding opportunity variables as the main determinant of civil conflict (the "disjunctive" result).

As one takes a deeper look at what type of country is mostly associated with the always zero or "complete peace" group, the answer often is higher-income countries. By not distinguishing the different types of zeroes, the standard ordered probit gives a likelihood of war calculation that includes countries conditioned to not experience war. These countries' main attribute is higher income, and income variables thus are estimated with greater emphasis and significance, crowding out the grievance variables' explanatory power. In contrast, using a zero-inflated probit model and splitting the estimation process into two stages, opportunity and grievance variables are given equal emphasis, which makes it clear that both greed and grievance matter, and both with substantial explanatory power in predicting civil war risk.

Clearly, economic factors are important in determining conflict prevalence, but so are grievances, and this is clearer when the lower probability of higher income/peaceful countries is considered. In postwar situations, it is important to study the causes of the conflicts with some care, both in terms of greed and grievance factors, and to deal with the underlying problems, rather than believing that general prescriptive policies will suffice (Brauer and Dunne, 2012).

Notes

We are grateful to the African Development Bank for support and to an anonymous referee for helpful suggestions. All remaining errors are ours.

1. Dunne and Tian (2017) provide more detail on these studies.

2. In a recent contribution, Buhaug, Cederman, and Gleditsch (2014) argued that the lack of significance had to do with the poor proxy variables used in previous research. They showed that better proxies indicate that grievances do matter.

3. A further development saw Besley and Persson (2010, 2014) create a nonbinary ordinal measure of civil violence, with 0 as the value for peace, 1 for civil repression, and 2 for large-scale civil conflict with more than 1,000 battle deaths. New datasets are allowing more consistent and detailed information to be used, such as the data set of global instances of political violence (http://ucdp.uu.se/ged/).

4. Sourced from the World Bank, the degree of urbanization can also be thought of as a measurement of geographic dispersion: The greater the urbanization, the lower the geographic dispersion. All income figures are adjusted for purchasing power parity (PPP). Male secondary school enrollment was not used in the estimations due to poor and incomplete data.

5. Pickering (2011) criticizes the use of this measure, suggesting it is not mountains *per se*, but the type of terrain that is important. This does not, however, invalidate its use here.

6. Data for the period 1960 to 1999 came from the World Bank and was cross-referenced with Fearon (2005) for consistency, and export data (primary commodities) came from the World Trade Organization (WTO) and was combined with GDP from the World Bank for the remaining years.

7. Oil *exports* are coded as a binary variable: 1 if the share of oil exports in total exports is greater than one-third (33.3%) and 0 otherwise. Oil *production* data, in metric tons annually, are provided by Ross (2013) for the years 1932 to 2011. The additional two years were drawn from Ross' source, the U.S. Department of Energy site for international energy statistics: http://www.eia.gov/cfapps/ipdbproject/IEDindex3.cfm.

8. Initially used by Easterly and Levine (1997), the fractionalization index follows Herfindahl's formula, and is interpreted as the probability that two randomly selected individuals in a population belong to different ethnic groups.

9. They argued that economic grievance is captured by the relative gap between the mean national income and the income level of the poorest and richest groups (positive and negative horizontal inequality), while ethnopolitical grievance is measured by the demographic size of the largest ethnic group discriminated against. The units of measurement are as follows: LDG = demographic size of the largest discriminated against ethnic group relative to the joint size of the discriminated group and the group in power (bound between 0 and 1); NHI = mean country GDP per capita / mean per capita income of poorest group; PHI = mean per capita income of richest group / mean country GDP per capita.

10. This differs from the existing literature, but in light of the empirical setup for Table 2, it makes some sense to find that primary commodity exports, as a share of GDP, are lower for countries not in civil conflict.

11. For a full explanation of the largest discriminated against ethnic growth (LDG), see Buhaug, Cederman, and Gleditsch (2014).

12. See Lambert (1992) and Hall (2000) for a full derivation of the model.

13. Statistical inference becomes increasingly difficult as the proportion of zeroes gets close to one.

14. To reiterate, specification (2) is merely a check on whether the choice of variables in the selection/inflation equation in specification (1) has a drastic impact on the type of results one obtains. Given that all variables are in both equations in specification (2), the results for the two outcome equations are surprisingly similar. The only noticeable differences between the two specifications are that in specification (2), the mountain variable becomes insignificant, two of the grievances terms become insignificant, and population changes sign. The

selection/inflation equation shows the probability of nonparticipation. Coefficient magnitudes can only be interpreted by calculating the marginal effects, not directly from the coefficients. For example, the variable, log real GDP has a coefficient of –0.375, but computing the marginal effect shows that higher GDP reduces the probability of being in the "experienced conflict" group by 9.1 percent.

15. Reported in an Appendix in Dunne and Tian (2017).

16. Normally the threshold is 0.5, so a dichotomous conflict variable is equal to 1 if the estimated probability is greater than 0.5 and 0 otherwise. This is then compared to the actual. The ROC method varies the threshold between 0 and 1, creating a curve plotting the true positive rate against the false positive one. Similar to the well-known Gini coefficient procedure, the area under the ROC curve summarizes a model's overall predictive power.

17. Since ROC's cannot be performed on variables that are not binary, the ordered outcome dependent variable (0,1,2) was divided into two binary (0,1) variables, namely, minor conflict (equivalent to the original variable equaling 1) and major conflict (equivalent to the original variable equaling 2). Separate ROC tests were then conducted to test the predictive power of the models and the individual variables on correctly predicting each type of conflict. Note that Fearon and Laitin (2003) and Collier and Hoeffler (2004) get ROC values of 0.761 and 0.860, respectively for their models. (These ROC values are taken from Ward, Greenhill and Bakke, 2010, who only ran 1 ROC each for F&L and C&H.)

18. Much the same can be said for most of the opportunity and the grievance variables but the Polity IV is of interest because while it became *statistically* significant once we switched from the probit to the ZiOP model, the ΔROC suggests that it is not *substantively* significant.

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Appendix

Zero-inflated models

A zero-inflated ordered probit (ZiOP) model follows a two stage estimation process. The first is a selection or *inflation equation*, and the second stage is a probit *outcome equation*. This splits the observations into two processes, each potentially having different sets of explanatory variables. In the context of civil war prevalence, zero observations in process 0 (w_i=0) include inflated zeroes, consistent with countries that never experience civil conflict (e.g., Botswana), while zero observations in process 1 (w_i=1) includes cases for which the probability of transitioning into a civil conflict is not zero, but civil war casualties have not reached the lower bound of 1,000 battle-related deaths. The binary variable w indicates the split between process 0 and process 1 and is related to the latent dependent variable w_i^*, so that w_i=1 for $w_i^*>0$ and w_i=0 for $w_i^* \leq 0$, where w_i^* now represents the propensity to enter process 1, given by the split probit 1st stage or *inflation equation*:

$$(1)\ w_i^* = x_i\gamma + \mu_i\ .$$

Here, x_i is a vector of covariates, γ is its coefficients, and μ_i is the error term. The probability of country i falling into process 1 (that is, war) is $\Pr(w_i=1|x_i) = \Pr(w_i^*>0|x_i) = \Phi(x_i g)$, and for process 0 (peace) it is $\Pr(w_i=0|x_i) = \Pr(w_i^* \leq 0|x_i) = 1-\Phi(x_i g)$, where $\Phi(.)$ is the standard normal cumulative distribution function. For the probit 2nd stage, or *outcome equation*, the propensity for participation in which the response variable Y_i (i.e, conflict) has a distribution given by:

$$(2)\qquad \Pr(Y_i=y_i)=\begin{cases} w_i+(1-w_i)^{e^{(-\lambda_i)}}, y_i=0 \\ (1-w_i)^{e^{(-\lambda_i)}}\frac{\lambda_i^{y_i}}{i}, y_i>0 \end{cases},$$

where the parameters λ_i and w_i depend on vectors of covariates x_i and z_i, respectively, which are modeled as $log(\lambda_i) = x_i^t\beta$ and $log[w_i/(1-w_i)] = z_i^t\gamma$, with mean and variance as $E(Y_i) = (1-w_i)\lambda_i$ and $var(Y_i) = \mu + [w_i/(1-w_i)]\mu^2$.

In this ZiOP model, the matrices z and x contain different

sets of experimental factor and covariate effects that relate to the probability of the zero-state (zero probability of civil war) and the Poisson mean in the nonzero-state (probable civil war), respectively. Thus, the γ's have interpretations in terms of the factor level effect on the probability that there is a zero probability of conflict and the β's have the interpretation of the effect on the average risk of civil war when the probability is nonzero. Following Lambert (1992), equation (2) in the ZiOP model can then be regressed using maximum likelihood with an expectation-maximum (EM) algorithm. For the full derivation, see Lambert (1992) and Hall (2000).

The incumbent, challenger, and population during revolution and civil war

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Abstract

We consider revolutions and civil war involving an incumbent, a challenger, and the population. Revolutions are classified into eight outcomes. In four outcomes incumbent repression occurs (viewed as providing sub-threshold benefits such as public goods to the population). Accommodation occurs in the other four outcomes (benefits provision above a threshold). The incumbent and challenger fight each other. The incumbent may win and retain power or else lose, thereby causing standoff or coalition. In a standoff, which is costly, no one backs down and uncertainty exists about who is in power. In a coalition, which is less costly, the incumbent and challenger cooperate, compromise, and negotiate their differences. If the population successfully revolts against the incumbent, the challenger replaces the incumbent. Eighty-seven revolutions during 1961–2011, including the recent Arab spring revolutions, are classified into the eight outcomes. When repressive, the incumbent loses 46 revolutions, remains in power through 21 revolutions, and builds a coalition after 12 revolutions. When accommodative, the incumbent loses seven revolutions and builds a coalition after one revolution. The 87 revolutions are classified across geographic regions and by time-period.

Goodwin (2001) describes a revolution as "any and all instances in which a state or political regime is overthrown and thereby transformed by a popular movement in an irregular, extraconstitutional and/or violent fashion."[1] For us, this takes the form that the incumbent is replaced with the challenger. For example, in eastern Europe, the end of the cold war and the collapse of the Soviet Union brought a wave of revolutions which saw the overthrow of communist regimes in these countries along with a decline in Marxist ideology and the introduction of free market-based economic reforms. Later, the 2014 revolution in Ukraine pertained to struggle over orientation either toward Moscow or to western Europe. Further toward the east, the 2014 Thailand revolution pertained to desire for political reform.

Revolutions such as these are caused by various triggers. Examples range from fraudulent elections that stir up the population to a Tunisian street vendor who, harassed by police, unleashed previously untapped frustration on 17 December 2010 causing revolution or, indeed, to any event where an incumbent has to decide whether to react with strategies such as repression or accommodation. The advent of the Arab spring as from 18 December 2010 caused the removal of a number of autocratic leaders across North Africa and the Middle East. Autocrats there usually held either fraudulent elections (e.g., Tunisia) or no elections at all (e.g., Libya). In Tunisia, the population chose revolution and the response of the autocrat was to relinquish power. The autocrat might alternatively have fought the revolutionaries hoping to crush the revolt.

Contribution

As we discuss in the next subsection, the scholarly literature on revolutions is sizeable but that on *classifying* revolutions into distinct types is sparse, and so it appears worthwhile to grapple with classification first. In this article, we present a summative, descriptive overview whereas a forthcoming paper presents the underlying formal modeling.[2] In particular, we consider revolutions and civil wars with an incumbent, a challenger, and a population, which may revolt. The incumbent fights with the challenger and chooses whether to provide the population with benefits that lie below (repression) or above (accommodation) a threshold. The incumbent's provision of benefits affects the participation by rebels, for example in that it may change the cost of contributing effort to a revolution for at least some rebels, may raise the benefits of contributing effort for at least some other rebels, may raise a rebel's potential share of the collective good, and may raise the probability of a successful revolution. Enough people need to participate in the revolution collectively—a population-wide threshold has to be exceeded—to make individual participation worthwhile (Granovetter, 1978). If the population revolts successfully, the challenger replaces the incumbent. If the incumbent loses against the challenger, a costly standoff may follow with

disagreement over who is in power, or else a coalition may follow where the incumbent and challenger cooperate and negotiate their differences. Distinguishing among repression and accommodation, winning and losing, and standoff or coalition when losing, results in eight possible outcomes (see Table 1) which are discussed later on.

Following a brief literature survey as well as a more detailed conceptualization of interactions among incumbent, challenger, and population, the section thereafter classifies 87 revolutions from 1961 to 2011 into the eight outcomes. Since readers are familiar with some or most of the revolutions, they can reflect on which forces have caused each of the eight outcomes we suggest. The revolutions are further classified into how they are distributed across six geographic regions and across three time periods. The penultimate section considers the Arab spring revolutions, and the final section concludes.

Literature

As mentioned, the literature on political revolutions is substantial and considers many facets. A brief overview follows. Kuran (1989) presents a theory of how political revolutions could occur in *unanticipated* ways. Examples include the 1789 French revolution, the 1917 Russian revolution, and the 1978–79 Iranian revolution, all of which took most people by surprise. More recently, the Arab spring revolutions, which began in Tunisia in late 2010, were equally unanticipated. Reasons for the turmoil in North Africa and the Middle East, the MENA region, have been explored by various authors. Kuran (2010; 2012) himself, for instance, argues that the doctrine of Islamic economics is simplistic, incoherent, and largely irrelevant to contemporary economic challenges, and that what slowed the economic development of the Middle East in particular was that, since around the tenth century, Islamic legal institutions hampered the emergence of features such as private capital accumulation, corporations, large-scale production, and impersonal exchange, all leading to economic discontent fostering revolution.

Tullock (1971; 1974) made seminal contributions to our understanding of revolutions, yet viewed them as mythical since an oppressed people wishing to rise up against a tyrant face a *free-rider dilemma* (Olson, 1965). A substantial literature then emerged probing why and how revolutions nevertheless occur (for reviews see, e.g., Kurrild-Klitgaard, 2003; Lichbach, 1995; Gehlbach, Sonin, and Svolik, 2016). Foran (1993) analyzes the earliest revolution theories and argues for the need to move to a more inclusive *broad new paradigm* based on modeling economic, political, and cultural processes, whereas Beissinger (2007) develops an approach to understanding revolutions as an *emulation* of the prior successful example of others, such as the post-communist revolutions in East-Central Europe and in the MENA region.

The article considers interactions among incumbents of high political office, challengers, and the general population. We classify 87 leadership challenges and revolutions from 1961 to 2011 into eight outcomes and discuss their spatial and temporal distribution.

Acemoglu, Vindigni, and Ticchi (2010) analyze the *persistence* of revolutions resulting in long civil wars. Indeed, McFaul (2002), who studies outcomes of revolutions, regards Russia's revolution as unfinished. Migdal (2015) focuses on revolutions and social change in *developing regions*, while Zimmermann (2012) focuses on theories of *violence* and revolutions. Casper and Tyson (2014) consider *popular protest and elite coordination* in coup d'etats, whereas Angeletos, Hellwig, and Pavan (2007) consider *regime change*, specifically, and Edmond (2013) writes on *information manipulation and coordination* related to regime change.

In recent years, the presence of *flawed elections* have received attention in relation to revolutions. Typically held by autocrats, they often involve manipulation and violence (see, e.g., Hermet, Rose, and Rouquié, 1978; Schedler, 2007). The cost to the population of flawed elections involves loss of life, physical and mental injury, suppression of freedom of speech, and human rights violations. While the election process can strengthen democratic institutions, it can also worsen conflict (Collier, 2009). Acemoglu and Robinson (2006) link the violent nature of election processes to countries' colonial roots. Ellman and Wantchekon (2000) consider situations where one strong party controls sources of political unrest. This party likely wins with asymmetric information about its ability to cause unrest. Related studies include Alesina (1988), Alesina and Rosenthal (1995), and Calvert (1985). Egorov and Sonin (2018) find that, on the one hand, regimes with a high degree of repression by the elite are less likely to hold fair elections. On the other, when they face a high cost from protests then fair elections are more likely. For electoral fraud and revolutions also see Little (2012) as well as Lindberg (2006) for an analysis of democracy and elections in Africa.

While considering many facets of revolutions, then, the literature on *classifying* revolutions nonetheless is sparse. Yet classifying revolutions is important since the causes of revolutions may be better understood if they are properly systematized. Along those lines, Basuchoudhary, *et al*. (2018) use *machine learning* to understand civil conflict. Accounting for actors with different objectives and the path-dependent nature of conflict, their algorithm applies out-of-sample techniques to choose among competing hypotheses about the

sources of conflict based on their predictive accuracy. Such a neutral or agnostic approach may avoid challenges associated with missing data, unusual statistical assumptions, the relative rarity of civil conflict, and multi-directional causality between conflict and its correlates. The authors argue that understanding which causes lead to conflict, and through which possible paths, may enable one to better design policy to curtail or even to terminate conflict. Regarding causes of revolution and civil war, the *ideological origins* of the 1775–83 American revolution are presented by Bailyn (1992), wheras Besancon (2005) analyzes the nexus between *economic inequality* and revolutions and conflict.

For a survey on civil war, including causes, see Blattman and Miguel (2010). They synthesize studies of cross-sectional inference using country-level data and panel-data studies accounting for within-country variation. For a survey on the determinants of government repression and human rights violations, see Davenport (2007). These two surveys focus on exploring empirical regularities, and less so on linking theory to data.

Shults (2018) argues that existing approaches to classifying revolutions usually reflect researchers' own theoretical views. Revolutions may thus get classified according to their mission, civilizational features, driving forces, or ideological orientation. Those that fall outside the researcher's view may get ignored. As an alternative, Shults recommends that revolutions should be classified from the point of view of the revolutions themselves, applying two criteria. The first is the algorithm, including the course and stages of revolutions and their temporal sequence. The second is the tasks revolutions address, or the problems they solve. Finally, Marder (2017) analyzes revolutions applying philosophical categories drawn from Aristotle and Kant, applying quantitative and qualitative, modal and positional, spatial and temporal, and substantive dimensions. Our own classification approach is different, of course (focusing on outcomes), and is summarized in the following section.

Conceptualizing interactions among incumbent, challenger, and population

We consider a country with an incumbent, a challenger, and a population. The incumbent is in power, governing the country. In an autocratic country the incumbent may have absolute sovereignty. The challenger opposes the incumbent. The challenger may comprise an ideologically committed opposition, parts of the elite or military, or various industrial interests or ethnic groups. It may consist of groups with incompatible interests, joined by a common goal of replacing the incumbent. The population may support the incumbent if the incumbent provides what the population needs, e.g., prosperity and public goods such as security. Conversely, the population may support the challenger if dissatisfied with the incumbent. If sufficiently dissatisfied, the population itself may initiate a revolution so long as it has the ability to organize so that its revolution gets off the ground.

The incumbent and challenger are in conflict, struggle, or battle (Tullock, 1967) and they fight or compete with each other in various ways (Hirshleifer, 1995). They may seek to undermine each other and seek legitimacy for doing so as viewed by the population. The fighting may be nonviolent or violent and it may or may not constitute a civil war.

Table 1 conceptualizes eight *outcomes*, numbered in rows 1 to 8. Divided into two groups, rows 1–4 and 5–8, column 1 then indicates whether or not the incumbent represses the population. *Repression* means providing no benefits to the population, or providing benefits below a threshold. Not repressing the population is referred to as *accommodation*, i.e. providing benefits above a threshold. Examples of benefits are public goods such as schools, hospitals, infrastructure, water, security, employment, various privileges, human rights, and social and economic rights.

Olson (1965) proposes that dictators will provide public services only to the extent that they increase gross domestic product (GDP). A threshold for providing benefits to the population may be at or above the GDP-enhancing benefits that the incumbent provides to the population. Providing benefits at that threshold is assumed not to decrease the probability of successful revolution. Countries experiencing revolutions often do not provide sufficient benefits to the population due to factors such as unstable governance, poorly developed societal institutions, corruption, poverty, limited education, natural catastrophes, and low GDP.

In Table 1 column 2, the incumbent and challenger fight regardless of whether the incumbent represses or accommodates the population, but the incumbent wins the fight. Hence in row 1 the symbolic outcome is *RP* is used, meaning that the incumbent represses and retains power (see columns 6 and 7). In contrast, in row 5 the incumbent accommodates and retains power, denoted as *AP*.

In columns 3 and 4 the incumbent loses the fight against the challenger, which causes either standoff (column 3) or coalition (column 4). Standoff gives the outcome *RS* if the incumbent represses, and *AS* if the incumbent accommodates. Standoff occurs if the incumbent does not accept its loss or if the challenger fails to acquiesce. Tensions build up and neither the incumbent, the challenger, the military, the population, governmental institutions, the international community, nor anyone else, knows who is in power. Any actor may potentially

Table 1: Formalizing the eight outcomes

Out-come	*1 Incumbent represses*	*2 Incumbent wins against challenger*	*3 Incumbent loses causing standoff*	*4 Incumbent loses causing coalition*	*5 Successful revolution*	*6 Verbal outcome*	*7 Symbolic outcome*
1	**Yes**	**Yes**	No	No	No	Incumbent remains in power	RP
2	**Yes**	No	**Yes**	No	No	Standoff	RS
3	**Yes**	No	No	**Yes**	No	Coalition	RC
4	**Yes**	No	No	No	**Yes**	Challenger becomes new incumbent	RL
5	**No**	**Yes**	No	No	No	Incumbent remains in power	AP
6	**No**	No	**Yes**	No	No	Standoff	AS
7	**No**	No	No	**Yes**	No	Coalition	AC
8	**No**	No	No	No	**Yes**	Challenger becomes new incumbent	AL

support the incumbent, the challenger, both, or neither. A standoff slows down a country and is costly since policy directions, budget allocations, orders, and so on become unclear and negotiations may never end.

Coalition, in column 4, gives the outcome *RC* if the incumbent represses, and *AC* if the incumbent accommodates. In a coalition the incumbent and challenger agree to cooperate and be in power jointly. This is less costly than a standoff but more costly than either the incumbent or challenger being in power since the incumbent and challenger have to negotiate their policy differences and seek compromises. They may for example allocate ministerial positions and choose policies to represent either the incumbent or the challenger.

Column 5 denotes a successful revolution so that the challenger becomes the new incumbent. The outcome is *RL* if the incumbent represses, and *AL* for accommodation. Whether revolution is successful depends on whether, and the extent to which, the incumbent provides benefits to the population. A successful revolution inevitably replaces the incumbent with the challenger. If unsuccessful, or if the population at large does not revolt, the outcome depends on the fight between the incumbent and challenger as discussed above.

Eighty-seven revolutions, 1961–2011

In Appendix Table A1 we show 87 revolutions for 1961–2011. The table shows the years for the revolution, its name, and its outcome (using the aforementioned symbols *RP*, *RS*, *RC*, *RL*, *AP*, *AS*, *AC*, and *AL*). Our outcome coding made use of Table 1. We confine attention to revolutions where the population and/or challenger react to, and seek to replace, the incumbent. This excludes the 1994 Rwandan genocide initiated by the Hutu majority government mass slaughtering the Tutsi. The first and second DR Congo wars are excluded as well since they were initiated by Rwanda and Uganda invading DR Congo. The May 1968 noncivil rebellion in France is included since it was initiated by student protests against traditional institutions, capitalism, and imperialism.

We determined the outcome by researching each of the 87 revolutions subjectively. Judgment and subject matter expertise were applied, of course. Specifically, we first determined whether the incumbent was repressive (outcome *R*), which means providing benefits to the population below a threshold. Benefitting from varying backgrounds and expertise among the research participants, discussions proceeded until agreement emerged on whether the threshold for coding an incumbent as repressive had been reached. The 15 Arab spring revolutions, listed at the end of Table A1, were assessed to have started by the population, recognizing the incumbent as repressive. The 2011 Egyptian revolution was classified as *RL* since the incumbent, Hosni Mubarak, was replaced with the challenger, Mohamed Hussein Tantawi (on 11 February 2011). The 1989 Tiananmen Square Protest in China was classified as *RL* rather than *RP* because of the extensive leadership changes after the protest. For example, General Secretary of the Communist Party, Zhao Ziyang, was replaced by Jiang Zemin on 24 June 1989, and Deng Xiaoping exited the party leadership by

resigning as Chairman of the Central Military Commission. In South Africa, 1961–1990, the incumbent repressed the population applying apartheid policies, which led to the emergence of an anti-Apartheid movement and which eventually replaced the incumbent, also causing outcome *RL*. If the incumbent was determined to be accommodative, we coded this as outcome *A*. For example, for the 1964 Zanzibar revolution in Tanzania the incumbent, the Sultan of Zanzibar and his mainly Arab government, was determined to be accommodative. Frustrated by parliamentary under-representation in spite of winning 54 percent of the July 1963 election, the mainly African Afro-Shirazi Party and left-wing Umma Party mobilized a revolution on 12 January 1964. This resulted in replacement of the incumbent with the challenger, Abeid Karume, causing outcome *AL*.

The most frequent outcome, *RL* (an incumbent loses a challenge, is replaced, and the revolution is successful), occurs 46 times (53 percent). Outcome *RP* occurs 21 times (27 percent) meaning that following a challenge an incumbent retained power. Of ourcome *RC* there are 12 instances (15 percent); here, the challenge to the incumbent ended in a coalition outcome. *AL* occurred 7 times (4 percent), and *AC* occurred only once (1 percent). This accounts for our 87 cases as *RS*, *AP*, and *AS* did not occur at all.

The spatial and temporal distribution of revolutions

Figure 1 shows how the 87 revolutions are distributed across geographic regions, and Figure 2 counts revolutions by region and time period (1961–1989, 1990–2009, and 2010–2011). Again, outcomes *RL* and *AL* differ even though both capture the incumbent losing. Recall that *RL* means that the incumbent first represses but then loses, whereas *AL* means incumbent accommodation but who then loses nonetheless. In both cases, the challenger takes over. This captures, for example, the difference between the East German and Polish outcomes. Repression was the incumbent's strategy for the 1989 East German revolution (classified as *RL*), while accommodation was the incumbent's strategy for the 1990 Polish revolution (classified as *AL*).

The political, social, and economic revolutions that swept throughout Africa, Asia, Eastern Europe, Latin America, and North Africa and the Middle East (MENA) were the result of a variety of causes such as poor and oppressive government and social cohesion challenges with the consequent need to change socio-political institutions and reorganize the economic life of the country. In some cases, such as national independence or liberation movements, the uprisings or revolts were the result of oppression or exploitation by an external power (e.g., colonization or foreign occupation). We inquire

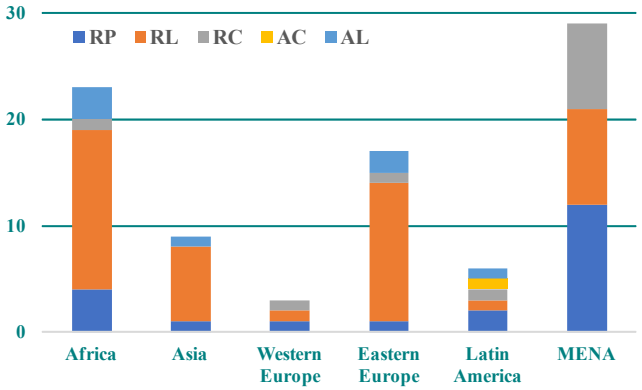

Figure 1: Number of revolutions per region, 1961–2011. *Notes*: The case color codes are as follows. Dark blue: *RP*. Orange: *RL*. Grey: *RC*. Yellow: *AC*. Light blue: *AL*. Cases *RS*, *AP*, and *AS* did not occur.

into the patterns of revolutions over three time periods in an attempt to better understand the conditions for their occurrence, success, or failure, with a focus on the recent revolutionary wave in the Arab world in particular.

Revolution is a complicated phenomenon. Attempts to generalize causes, scope, patterns, and outcomes of revolutions can be misleading. Our 87 revolutions vary widely in terms of conditions for occurrence, methods, duration, motivating ideology, and outcomes. For instance, in Africa, many of the revolutions that took place in the 1960s and 1970s were motivated by a desire to gain national independence from colonial rule or liberalization from the control of a dominant administration. In most cases, the duration of revolutions or liberalization movements ranged from less than a year to more than three decades. The outcomes were deemed successful if they achieved their goal of gaining independence from an imperial power. The picture in Latin America during the same period was somewhat different. Most countries already had gone through the phase of gaining independence from European control but had difficulties addressing social class problems that prevailed in the aftermath of independence—and then led to rebellion by certain classes of society. In the MENA region, the earlier part of our covered time period coincides with the rise of Islamic fundamentalism, which revolted with grievances against policies of westernization and modernization adopted by their countries' leaders. One case is Iran where Islamists, led by Ayatollah Khomeini, rebelled against the Shah and his western ideas, culture, and allies to successfully gain control of the country and transform it into an Islamic Republic.

During the 1980s and 1990s, revolutionary movements spread fairly evenly across Eastern Europe, Africa, the Middle East, and to some extent Asia and Latin America (Figure 2).

The cold war state of political and military tension between the two superpower blocs (Western and Eastern), and the decline of the USSR interplayed differently across cases. During the cold war, the battle between the United States and the Soviet Union for increased diplomatic, military, and economic influence in developing countries fueled several chains of revolutions in Africa, Southeast Asia, the MENA region, and Latin America. Most of those revolutionary movements were short in duration (on average lasting one year) and less intense than the anti-colonial and national upheavals of the 1960s. The end of the cold war and the collapse of the Soviet Union brought a new wave of revolutionary leaders seeking to overthrow communist regimes in Eastern Europe. Leaders with vested interests in gaining and securing political stability, liberal democracy, and domestic development were supported by foreign powers, leading to revolutionary victory. The indubitable outcomes of this wave of upheavals were the decline of Marxist ideology and the rise of the liberalization of eastern European countries away from communist systems and consequent capitalist-oriented economic reforms in many developing and emerging countries.

Some of the reasons given in this section are tentative, laying the groundwork for more systematic future research, but two ideas remain. First, that classifying revolutions is in itself an important task. And, second, that finding reasons for revolution based on classification is an important but separate task as well. Specifically, substantially more evidence would be desirable combined with a clear emphasis on developing causal links between the correlates of revolution and the different classes of revolution.

The Arab spring revolutions

The wave of revolts and protests first in North Africa and the subsequent domino effect across the Middle East, frequently referred to as the Arab spring, has been very intense. Some successfully overthrew autocratic regimes (e.g., Tunisia, Egypt), others still struggle—and may ultimately fail—to overcome repression by the political elite (e.g., Syria). All our cases have evolved in complex ways over time. Few readers will be unfamiliar with the way the events unfolded. Starting in Tunisia in December 2010 with the self-immolation of a street vendor in protest of his ill treatment by the police, there followed violent street demonstrations in Egypt's major cities in late January 2011, followed by unrest in Libya, Yemen, Syria, and much of the remainder of the MENA region. While the protests in Tunisia, Egypt, Libya, and Yemen resulted in the removal of their leaders and governments, those in Sudan and Jordan only partially achieved their objectives as leaders agreed to step down at the end of their then-current terms. At the time of writing (2018), five revolutions are ongoing. This includes Syria, where street protests have escalated to very violent military operations and heavy fighting between Syrian government and rebel forces in cities such as Homs and Hama.

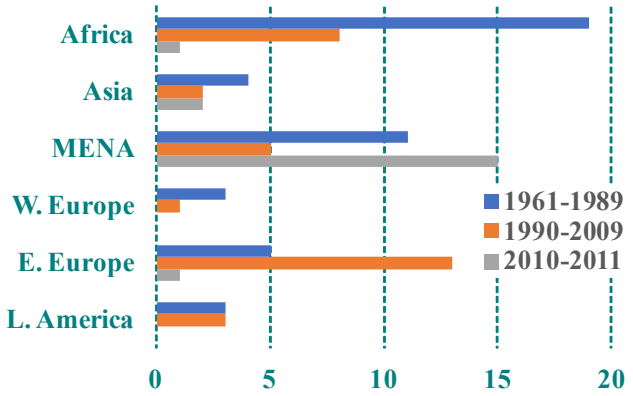

Figure 2: Number of revolutions per region by time period. *Note*: Nine revolutions overlap two time periods and are doubly displayed in the figure. *Source*: African Development Bank, Statistics Department.

One can assert this or that set of conditions for the occurrence of these revolts in the Arab world, but the reasons for success or failure of the actual revolutions can be quite complex. Relevant factors include, among others, authoritarian regimes or monarchy, high corruption, economic decline, unemployment, rising poverty, human rights violations, and structural demographic issues such as dissatisfied youth. To better assess the intricacies of victory of a social revolution, it would be instructive to compare the cases of Tunisia and Egypt with those of Libya and Syria. Fundamental questions remain unanswered such as: Why did the leaders in Tunisia and Egypt succumb faster than those in Libya? Why and how did Syria's government hold its ground, being on the verge of victory today? What role did external factor(s) play in these revolutions?

We define pre-revolutionary Tunisia and Egypt as "autocratic bureaucracies," in which social control rested on the division of labor and coordination of effort between a bureaucratic state and a powerful middle class. As an adjunct to its business interests, the middle class had acquired considerable authority over the majority of the labor force and in that authority it was backed by a central state that extracted taxes and labor from the population in cooperation with individuals of the middle class. Socio-political stability was maintained as the autocrat, bureaucracy, army, and/or police monopolized decisions while accommodating the middle class, even recruiting some of its members into state decisionmaking positions.

Ideally, when confronted with political or socio-economic crises (e.g., fiscal crises, military collapse, tax collection, regional disparities), a state will seek to strengthen itself through relevant reforms such as the abolition of middle class tax privileges. However, a powerful middle class can either block reforms—exemplified by the massive, and credible, demonstrations in 2011 in Tunisia and Egypt, resulting in the open conflict between the middle class and the state—or it can ally with the grievances of the poor against the overweening authority and ill-functioning of the state.

Conclusion

We consider revolutions and revolutionary uprisings, such as civil war and the Arab spring series of events, and consider an incumbent, a challenger, and a population. Systematized into eight outcomes, the incumbent represses the population in four of them (provides either no benefits at all or only below some threshold) and, in the other four, accommodates the population (provides benefits above a threshold).

If the incumbent wins against the challenger, power is retained. If the incumbent loses, a standoff or coalition may ensue. In a standoff it is unclear who is in power since neither incumbent nor challenger back down. A standoff is costly and slows a country since uncertainty exists about policies, budget allocations, and so on. In a coalition, incumbent and challenger share power. A coalition is less costly than a standoff since incumbent and challenger cooperate, negotiate, and compromise regarding policies and decisionmaking. In contrast, if the population succeeds in revolting against the incumbent, the challenger replaces the incumbent.

We consider 87 revolutions, 1961–2011, and map them onto the eight outcomes. The incumbent represses in 79 of the 87 revolutions but lost in 46 of them, remained in power in 21, and built a coalition in 12. When accommodative, the incumbent lost in 7 of the 8 cases and built a coalition in the remaining one. We plot the worldwide geographic distribution of our cases and further subdivide them by three time periods, 1961–1989, 1990–2009, and 2010–2011. Finally, we provide some characteristics of the Arab spring revolutions.

We suggest that an optimal degree of repression may exist (limiting the provision of various goods such as education) that can keep autocratic regimes in power, and this should be analyzed further in future research. Techniques such as partial dependence plots (e.g., Basuchoudhary, *et al*., 2018, p. 132) may be able to identify inflection points. Further analysis using structural modeling may assign causal links. In addition to highlighting nuance into why revolutions happen, such approaches may help prevent bloodshed and show the way to bargained, peaceful regime change that benefit populations. Of course, our classification of outcomes into eight classes should be scrutinized by applying for instance the techniques and approaches of Marder (2017), Basuchoudhary, *et al*. (2018), and Shults (2018).

Notes

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1. Goodwin (2001, p. 9).
2. Hausken and Ncube (2020, forthcoming).

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Table A1: Revolutions and their outcomes, 1961–2011

Case	*Years*	*Revolution*	*Out-come*	*Case*	*Years*	*Revolution*	*Out-come*
1	61–70	First Kurdish-Iraqi War	RP	31	79	Iranian Revolution	RL
2	61	Algiers Putch	RP	32	80	Coconut War (Vanuatu)	RP
3	61–91	Eritrean War of Independence	RL	33	70–80	Zimbabwe	RL
4	61–75	Angolan War of Independence	RL	34	83–05	Second Sudanese Civil War**	RL
5	61–90	Anti-Apartheid Movement	RL	35	86	People Power Revolution (Philippines)	AL
6	62–74	Independence of Guinea-Bissau and Cape Verde*	RL	36	87–91	First Intifada (Palestine)	RP
7	62	Revolution, northern Yemen	RL	37	87	Singing Revolution (Estonia, Latvia, Lithuania)	RL
8	62–75	Dhofar Rebellion (Oman)	RP	38	88	8888 Uprising (Burma/Myanmar)	RL
9	63–69	Bale Revolt, southern Ethiopia	RP	39	89	Caracazo (Venezuela)	RP
10	64	Zanzibar Revolution (Tanzania)	AL	40	89	Tiananmen Square Protests (China)	RL
11	64-79	Rhodesian Bush War/Zimbabwean War of Liberation	RL	41	89	Velvet Revolution (Czechoslovakia)	RL
12	64–75	Mozambican War of Independence	RL	42	89	Peaceful Revolution (East Germany)	RL
13	65	March Intifada (Bahrain)	RL	43	89	Romanian Revolution	RL
14	65	Malawi	AL	44	89	Hungary	RL
15	65	Zambia	AL	45	90	Poland	AL
16	66–88	Namibia Struggle for Independence*	RL	46	90	Riots in Zambia	RL
17	67–70	Biafra (Nigeria)	RP	47	90-95	Log Revolution (Croatia)*	RL
18	68	May 1968 in France	RP	48	90–95	First Tuareg Rebellion (Mali and Niger)	RP
19	68	Prague Spring (Czechoslovakia)	RP	49	91	Shiite Uprising (Karbala, Iraq)	RP
20	69–98	The Troubles (Northern Ireland)	RC	50	91	Soviet Union/Russia	AL
21	70–71	Black September (Jordan)	RP	51	92–95	Bosnian War of Independence	RL
22	71	Bangladesh Liberation War**	RL	52	94	Zapatista Rebellion (Mexico)	RC
23	74	Revolution, Ethiopia	RL	53	94–96	First Chechen War (Chechnya)*	RL
24	75–91	Western Sahara War**	RL	54	97–99	Rebellion in Albania	RL
25	75–90	Lebanese Civil War	RP	55	98	Kosovo Rebellion	RL
26	75–02	Angolan Civil War	RL	56	98	Bolivarian Revolution (Venezuela)	AC
27	77-92	Mozambican Civil War	RC	57	98	Indonesian Revolution	RL
28	78	Saur Revolution (Afghanistan)	RL	58	99–	Second Chechen War (retake by Russia)	RL
29	78	Kurdish–Turkish Conflict	RP	59	00–04	Second Intifada (Palestine)	RP
30	79	New Jewel Movement (Grenada)	AL	60	00	Bulldozer Revolution (Yugoslavia)	RL

Table A1 (continued): Revolutions and their outcomes, 1961–2011

Case	*Years*	*Revolution*	*Out-come*
61	01	Macedonian Conflict	RC
62	01	EDSA Revolution (Philippines)	RL
63	01	Cacerolazo in Argentina	RL
64	03	Rose Revolution (Georgia)	RL
65	03–	Darfur Rebellion	RL
66	04–05	Orange Revolution (Ukraine)	RL
67	05	Cedar Revolution (Lebanon)	RL
68	05	Tulip Revolution (Kyrgysthan)	RL
69	07–09	Tuareg Rebellion (Mali and Niger)	RP
70	09	Malagasy Political Crisis (Madagaskar)	RL
71	10	Thai Political Protests (Thailand)	RP
72	10	Kyrgysthani Revolution	RL
73	10–	Arab Spring (Tunisia)	RL
74	10–	Arab Spring (Algeria)	RP
75	11–	Arab Spring (Jordan)	RC
76	11–	Arab Spring (Mauritania)	RP
77	11–	Arab Spring (Oman)	RC
78	11–	Arab Spring (Saudi Arabia)	RC
79	11–	Arab Spring (Egypt)	RL
80	11–	Arab Spring (Yemen)	RL
81	11–	Arab Spring (Iraq)	RC
82	11–	Arab Spring (Bahrain)	RC
83	11–	Arab Spring (Libya)	RL
84	11–	Arab Spring (Kuwait)	RC
85	11–	Arab Spring (Morocco)	RC
86	11–	Arab Spring (Syria)	RC
87	11–	Arab Spring (Lebanon)	RP

Notes: * Liberation Movement—Liberation from outside powers; ** Liberation Movement—Resulting in secession and new state
Codes: RP: Incumbent succeeds and remains in power; RS: Incumbent loses causing standoff; RC: Incumbent loses causing coalition; RL: Incumbent loses revolution. Challenger becomes new incumbent; AP: Incumbent succeeds and remains in power; AS: Incumbent loses causing standoff; AC: Incumbent loses causing coalition; AL: Incumbent loses revolution. Challenger becomes new incumbent.
Source: African Development Bank, Statistics Department.

Combining data on military supply and demand for arms production estimates

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Abstract

Currently there exist no data series comparing arms production values among countries. The article outlines three methods for generating such data series based largely on already available data series relevant to arms production, in particular series on arms imports and exports, procurement, and turnover figures from the world's largest arms producing companies. All three methods have major shortcomings and limitations but with additional effort in data collection they can provide a basis for comparing arms production values among countries as well as for regional and global totals. Furthermore, as the three methods use different definitions of the scope of arms production, comparison of the data produced by them can provide additional insights.

A number of data series exist on military-related issues, including (near-)global series on military spending, arms exports, and the world's largest arms producing companies, as well as more limited data series on procurement spending and military research and development. Global data series on national arms production currently do not exist.[1]

Data series on national arms production would be useful for several reasons. First, such data would be interesting in itself. Like data on other industries, time series could help to assess industry growth and contraction over time and data on several countries would allow for size comparisons. Second, when combined with other military-related data, such as figures on military expenditure and the arms trade, such data could broaden our understanding of military-industrial matters, for example the degree of countries' self-sufficiency in military matters. Third, set in relation to broad economic data such as GDP and overall industrial production, data on arms production could serve as an additional measure of the importance of military-related aspects of a country's economy. Fourth, a global estimate and a comparison of its national and regional components would be an additional indicator of military affairs both among states and globally.[2]

This article presents three methods for estimating countries' arms production values. They primarily use existing data from the data series mentioned above. This distinguishes them from estimates that, in addition to using available data on other aspects of military sectors, also use economic data, such as input-output tables (see below). The first method has already been used, for instance, to estimate arms production in Latin American countries (Lopes da Silva, 2018). The second method has been used in a number of earlier publications by various authors, while the third, which includes data on the world's largest arms producing companies collected by SIPRI, seems not to have been presented before. These methods can yield data for individual states and regions in addition to global estimates. To illustrate commonalities and differences among the three methods, this article produces data for a small selection of major arms producing countries. Estimation methods are explained below, and additional information on the methods is provided in Tables A1 to A3 (in the appendix).[3]

Each method can be used for the purposes listed above. The development of the methods adds the option of comparing data series that correspond to differing definitions and arms production valuations. It must be stressed, however, that all three methods are beset by major difficulties and shortcomings. The prime difficulty regards the glaring gaps in the data needed to calculate production values. This pertains both to the information as such and to the data's conformity to standardized definitions. Using available data series such as those described above presents something of a shortcut, as they are said to conform to standardized definitions. However, not least because of gaps in the available information necessary for checking conformity with standardized definitions, the data quality varies among countries.

As will become clear in the following discussion on the three methods, data on countries with fairly small arms industries tends to be particularly problematic. In addition, there also are highly contentious cases, such as Russia and China, among the major arms producers. Comparisons among countries with differing data sources are therefore problematic and need to be qualified. The comparative data presented below thus is intended as a first approximation in need of

improvement through further work on the data. For regional and global estimates, data gaps for smaller arms producers may be less relevant than the problems associated with estimating production values in countries such as Russia and China.

The article proceeds as follows. It first discusses the current state of the data on domestic arms production and then presents the three aforementioned methods, including estimates for a limited selection of countries. The article then draws key conclusions, including from a comparison of the three series.

The article first discusses the current state of the data on domestic arms production. It then presents three methods to estimate such data (with examples for selected countries). Finally, the article draws key conclusions, including from a comparison of the three series.

Efforts to fill data gaps on arms production

Data on the value of arms production is rare, nationally and even more so for sets of countries. There are a number of reasons for this. One is the difficulty of distinguishing arms production from civilian production as the two are becoming increasingly integrated. One solution to this problem is to estimate data corresponding to different definitions of arms production. Sometimes, direct and indirect arms production are distinguished, with direct production limited to goods specifically produced for military purposes and indirect arms production covering the production of dual-use goods sold to armed forces and all types of goods used as pre-products and components in military goods.[4]

Another reason lies in the way in which most countries construct industrial statistics. Traditionally, the *materials* they primarily work with, such as wood, chemicals, and metals, have defined most industries. Increasingly, however, major *product lines* have been added as defining industries, such as automobiles, machinery, airplanes, and information technology products. These generally also focus on some type of material, particularly metals, but not exclusively. While they overlap, in general these industries can still be clearly distinguished. In principle, the whole of arms production could be added to this list, although this would create substantial overlap with other industries, such as chemicals and metals mentioned above. International bodies in charge of defining industries have preferred in principle to stick with the larger industries rather than attempt to classify a broad arms industry. In the current 4-digit system of the International Standard Industrial Classification (ISIC, Rev. 4), various types of artillery, light weapons, small arms, ammunition, and the like are contained in category 2520. Military fighting vehicles also have a separate category (3040), as do Defense Activities (8422), which cover military operations. Warships, however, are not separated from civilian vessels (all in category 3011); nor are space and aircraft (3030), and all types of electric and electronic commodities (categories 2610 to 2790). The List of all Industrial Products, which forms the basis for the Industrial Commodity Statistics produced by the United Nations, is constructed in a similar way, distinguishing only a limited segment of arms production within categories that are separate from civilian production.[5]

Classifications can go deeper than 4 digits, and a limited number of countries have opted to add additional digits to their national classification systems, which separate civilian and military production (for instance in shipbuilding and space and aerospace). For some countries, it is therefore possible in theory to construct data for arms production from official sources beyond what the 4-digit ISIC classification provides. Gaps remain, however, particularly with respect to electric and electronic industries, where deep classification is especially difficult due to the similarities between many products used by the military and their civilian counterparts.

Finally, governments may be reluctant to publish data on national arms production, even where classification systems would allow for it. A few defense ministries have published estimates over the years, but definitions and the sources of the data basis have generally remained obscure. This raises an additional problem: Even if governments were to publish official data, it would not necessarily correspond to comprehensive definitions unless some international body were to provide them. The difficulties encountered in the past regarding the definition of military spending and arms transfers in the context of the reporting instruments of the United Nations indicate that this would be challenging with respect both to determining the proper boundaries of the defense sector and governments' political interests.[6]

Researchers interested in arms production data have therefore had to generate their own estimates. Employing a variety of methods—such as input-output analysis, collections of data on value-added by relevant companies, and the combination of procurement and trade data—these have only covered single countries, however, sometimes over several years, or groups of countries (such as the European Union) over a single year.

The closest researchers have come to a global estimate of arms production (or rather a near-global estimate, as data on China is missing) is SIPRI's annual data on the world's 100 largest arms producers, the Top-100 list. However, SIPRI collects data not on *national* arms production but on *sales by major companies* produced across national jurisdictions. As will be shown, this data series can still be used to supplement

the estimation of data on national arms production.[7]

This brief discussion of the current situation with respect to arms production data indicates the variety of both estimation methods and data sources. Methods and data are closely interrelated, with specific methods requiring specific data. Unfortunately, this data is often only available as rough and/or gap-ridden estimates. Furthermore, all methods require that researchers make judgments, for instance on what counts as civilian and what counts as military production, as well as assumptions, for instance about the relative importance of industrial sectors in arms production.

As there is no generally accepted international definition of arms production, one can play with various potential conceptions, from major weapon systems to all goods used by the military. In what follows, I focus on three methods that use available data on both supply and demand with regard to military goods. The presentation of these methods does not imply that they produce more accurate data than other methods that involve integrated economic data, for instance data from (civilian) industrial sectors or input-output analysis. Indeed, as already mentioned and further detailed below, all estimation methods—mine and others—come with serious shortcomings and limitations. That said, each of the three methods considered here has the potential to provide rough estimates for comparisons among countries and for regional and global aggregates. Their advantage is that they are less dependent on national idiosyncrasies with respect to the primary data than methods using national data. Furthermore, the methods are simple and the necessary data is comparatively easily available, making it more likely that data on many (or even all) countries can and will become available in the future.

Method 1: Major conventional weapons

The first method addresses a very narrow conception of arms production. While it clearly does not cover all arms production, it does correspond to a widespread perception of what makes the industry special. In this regard, it is similar to the SIPRI data on trade in major weapons, which is widely accepted as an indicator of the arms trade in general. The basic conception of this method has recently been presented by Lopes da Silva (2018).

The core idea is to calculate a given government's *procurement* of major conventional weapons based on *domestic production* (which may, however, include imported intermediate products, discussed later on) and to add to this the value of its *exports*. Data on the quantity of major conventional weapons exports is readily available from SIPRI's Arms Transfer Database.[8] Data on the procurement of major conventional weapons is in principle available from the same type of sources as used for the SIPRI arms trade data. Key sources for data on the procurement of major conventional weapons include national ministries of defense, handbooks, relevant company publications, specialized magazines, and the annual *Military Balance* publication from the International Institute for Strategic Studies (the primary source for the illustrative example in Appendix Table A1), which contains data on new procurement of weapon systems. Since annual domestic weapon procurement of weapon systems is the basis for estimation, data on domestic procurement can in principle be made consistent with the SIPRI arms trade data. However, while the same sources used for the data on arms trade can also be used for procurement data, the effort to create this data series would probably be demanding as procurement concerns more, and often not very common, items. SIPRI uses a pricing system that is designed to present the use-value of major weapon systems. This pricing system is used to provide what SIPRI calls Trend Indicator Values (TIV) for arms exports in its database on major weapon exports and can also be used to value domestic procurement.[9]

TIVs focus on a particular aspect of weapon systems, namely their competitive production costs. In principle, TIVs are based on the known unit production costs of a core set of weapons.[10] A further assumption is that weapon systems with similar characteristics have similar prices. Prices for weapon systems for which data is not available are calculated based on these assumptions by comparing a limited number of physical characteristics.[11] These estimates may differ from actual production costs. They are therefore not directly comparable to economic data such as data on industrial statistics or national income. Furthermore, arms producers may be more or less efficient than producers whose production costs are known, which implies that using TIVs may lead to the over- or underestimation of actual production values. As is the case for the SIPRI arms transfer data, comparisons of arms production among countries based on TIVs therefore focus on the military capabilities of industries rather than actual production values.[12]

Table 1 provides a summary of the example of this method a(nd is presented more fully in Appendix Table A1). As its purpose is to demonstrate the method, there is no claim to completeness with respect to domestically procured major weapon systems. A global estimate would need to be aggregated from national data. This would be a major effort at the gathering of data currently only available in scattered sources, and it is likely, as is the case with the SIPRI data on major weapons transfers,[13] that some gaps and puzzles would remain and would need to be filled and resolved by expert judgment.

There are several additional problematic issues with this

method. One is the conception of arms production on which it relies. It obviously only covers a part of arms production. While major conventional weapons such as aircraft, warships, and battle tanks are very costly items that usually make up the bulk of procurement spending, there are important gaps in arms production that are not covered by SIPRI's definition of major conventional weapons.[14] The most important of these concern small and light weapons,[15] as well as military support equipment (such as unarmed special vehicles) not included in the SIRRI data. A second issue stems from the use of data on the *delivery* of weapons to customers of major weapon systems rather than data for *production* itself. Delivery schedules often do not coincide with production schedules, particularly when new weapon systems are introduced. A third issue concerns imports of components and pre-products, which are then integrated into other countries' arms production. This issue is largely ignored when only the final delivery of systems to customers is considered, but it can be important, particularly for countries with small production bases for such components compared to their major weapons production capabilities. Some major components, such as engines and radars, are separated out in SIPRI's arms trade data and could in principle also be separated out for procurement data; however, these are only some of the components and pre-products that, if imported, are not elements of a country's arms production.

These various limitations add up to the proposition that data calculated via this method is likely to be more reliable for countries and country groupings with large arms industries, primarily those that produce major weapons and their components. But for smaller arms-producing countries, where the production of small arms and light weapons and the importation of components and pre-products generally make up a higher share of total arms production than in countries with large arms industries, method 1 is likely to lead to a significant underestimation of arms production. Comparisons among countries with different arms production structures may therefore be misleading. The same goes for comparisons of arms production over time, which are likely to be more reliable for countries with large volumes of arms production than for countries where few items are produced and/or where deviations between production and delivery schedules may have a greater influence on trend data. These limitations, however, are less relevant to *global* estimates of arms production, which will be dominated by major producer countries.

Table 1: Method 1 estimates of major weapons production, selected countries, 2015

	Category	*Total mn TIV*
France	Domestic procurement (from national sources only)	837
	+Exports	2,017
	=Total	2,854
Russia	Domestic procurement (from national sources only)	7,504
	+Exports	5,842
	=Total	13,346
USA	Domestic procurement (from national sources only)	21,648
	+Exports	9,931
	=Total	31,579

Method 2: National procurement and arms trade data

The second method starts off with a broader definition of arms production, corresponding to what is often classified as equipment investment in procurement or viewed as the industrial source for military exports, for instance in the European Union's Common Military List.[16] No further effort to construct a consistent definition is made here, although such a definition would be required to arrive at more valid estimates than those presented here. Arms production in a particular country P_i is calculated by adding national procurement (D_i) and exports (X_i) but subtracting imports (M_i),[17] so that

$$(1) \qquad P_i = D_i + X_i - M_i \,.$$

Appendix Table A2 illustrates the method and uses data that allows for the constructing of corresponding data series on arms production. Since data for arms exports, arms imports, and procurement needs to conform to standard definitions, data sources that claim to have standardized the data are preferable. Examples of such sources for regions include the European Union's arms transfers data and NATO procurement data.[18] NATO provides a short list of items the procurement of which should be included in this subcategory of defense expenditures and which one would hope to see included in a comprehensive definition of arms production. It is likely that some non-NATO member countries will publish data on procurement which is similar to the NATO data, but many will not.

In the *World Military Expenditures and Arms Transfer* series, the U.S. government publishes data on arms imports and exports, for which a comprehensive definition is provided.[19] However, it is questionable whether data on exports and imports actually corresponds to a common standard.[20] National data on procurement, arms exports, and imports, while often

corresponding to idiosyncratic definitions, can help to fill gaps and to determine the plausibility of data in broader series. For some countries, no relevant data is available, and the figures must be estimated. One possible way to do this is via parametric estimation of different factors, such as GDP and military spending per soldier, which likely shape procurement spending in countries where data is available. But the profiles of these countries (e.g., NATO member states) may be quite different from countries where estimates are needed.[21]

Table 2: Method 2 estimates of arms production, selected countries, 2010–2015 (real USD billions, in 2015 prices)

	2010	*2011*	*2012*	*2013*	*2014*	*2015*
FRANCE	14	13	14	11	11	11
Procurement from domestic production	5	5	4	5	5	7
+ Exported arms production	19	18	18	16	16	18
= Total arms production						
RUSSIA	11	12	13	13	14	16
Procurement from domestic production	11	14	16	16	16	15
+ Exported arms production	22	26	29	29	30	31
= Total arms production						
USA	167	190	179	161	154	146
Procurement from domestic production	57	67	53	42	83	95
+ Exported arms production	224	257	232	203	237	241
= Total arms production						

Source: Appendix Table A2.

Table 2 is an illustration of the data produced for Appendix Table A2 which, itself, is provisional and does not represent the best data that would be available with more time and effort. For Appendix Table A2, NATO data on "equipment expenditures" was used for NATO member countries. Estimation will be necessary to fill the gaps. The data for China and Russia in Table A2 was estimated using a very simple method: It was assumed that the share of procurement in military expenditures was the same as that of the United States. More complex estimation procedures would have to be used for more accurate estimates of missing data. Data for arms imports and exports is taken from WMEAT data series,[22] except for the U.K., where the U.S. government data for 2015 was far beyond what is plausible given earlier data for the U.K. and data on exports of major weapons from that country as reported by SIPRI.

While attractive in principle due to its wider scope (which corresponds to a broader, more widespread definition of arms production), method 2 nonetheless faces serious issues related to definitions and data requirements. Furthermore, as with method 1, there is the issue of estimating production based on the weapons' final consumption—composed of *total* national procurement plus exports. That is, the method assumes that all imports are imports by the final consumer, the national government, when imports may in fact include pre-products and components for the importing countries' arms industries and thus should not count as final consumption but as intermediate products. Put differently, there is an element of double-counting at least some imports. Still, because of the broader scope of products included in this method, these problematic issues may be of lesser importance if they are dealt with properly, particularly if moving averages rather than annual data are used.

The various limitations mentioned above, as well as the shortcuts made for the sake of presenting the principle of the method, allow for only rough country estimates in Tables 2 and A2. Much more work would be needed to arrive at more concise and comparable estimates. In some respects, a global estimate would be easier to produce than a set of many national estimates. Procurement data for all countries would suffice, as imports and exports would cancel each other out. Nevertheless, export data would be helpful in constructing procurement data, particularly for countries in which domestic arms industries are either very small or nonexistent.

Method 3: Combining procurement, export, and sales data

As discussed, methods 1 and 2 are afflicted with deficient reporting on imported components and pre-products—method 1 because this aspect of arms production is wholly ignored, and method 2 because all imports are taken as final consumption by national governments. Method 3 reveals, in a preliminary way, how using the SIPRI data on arms *producers* (rather than *production*) might help to address the second issue. Method 3 thus is an extension of method 2, at least for countries that have a good number of companies in SIPRI's Top-100 list.

The SIPRI Arms Industry Database reports sales (sometimes called turnover). In many countries, sales data (by industry) is aggregated as gross output in national account statistics. As pre-products, services, and so on bought from other companies are also included in sales, adding up sales results in double-counting items which are traded among

companies. For this reason (there are some additional, less important issues, e.g., regarding taxes, which I will not deal with here), many economists prefer net production values, known as gross domestic or, with some differences, national product or national income (respectively, GDP, GNP, GNI).[23] Similarly, I would assume that most observers would find national, and global, data on arms production (without double-counting) preferable to sales data. As only the arms sales data in the form of the SIPRI Top-100 data is available, however, it is often perceived, in my view justifiably, as a valid indicator of arms production by the largest companies.[24] But it is more problematic to use this data as an indicator of the relative weight of countries. One reason is that sales by companies may stem from production in a different country than that in which the company is registered and thus located in the SIPRI Arms Industry Database.[25]

In full awareness of the major problems associated with the SIPRI Top-100 data—the potential double-counting, the exclusion of all smaller companies, and production in countries other than that in which the company is registered—method 3 makes use of this data. More concretely, method 3 uses the crucial, and certainly controversial, assumption that sales by companies from the Top-100 in a country ($S100_i$) are a rough estimate of domestic arms production (P_i) in that country. Put differently, arms sold by smaller companies to governments as final customers are treated as if they were pre-products and components bought by the Top-100 companies from domestic sources. Clearly, if it is valid at all, this is likely to be more valid in some countries than in others. In particular, there is a bias against countries with no or few companies which make it into the Top-100 list, even though domestic arms production is substantial. Method 3 is therefore systematically skewed toward countries with large arms producing companies, severely limiting the validity of comparisons among countries. For some countries, having data on many smaller companies might help to at least partially address this problem.

As mentioned several times before, the purpose here is to outline a method rather than to provide adequate estimates. The method is based on the idea that not all relevant components and pre-products will come from domestic sources and that some will instead be imported (MP_i) as a part of the total arms imports (M_i). Method 3 thus differs from method 2, where arms imports are not divided between those that are government procurement purchases (MG_i) and those that are used as components and pre-products by arms producing companies (MP_i).

Formally, the two preceding paragraphs can be written as

(2) $S100_i = P_i + MP_i$

Table 3: Percentage comparison of estimates of arms production with methods 2 and 3, selected countries, 2010–2015

	'10	*'11*	*'12*	*'13*	*'14*	*'15*
France	22	24	24	35	29	27
Germany	20	11	7	14	29	1
Russia	–84	-99	–75	–40	–24	–21
UK	43	30	59	42	47	24
USA	23	5	7	14	–5	–12

Note: Positive values show the extent to which sales by Top-100 arms producers are greater than estimates based on method 2; negative values occur where sales by the Top-100 arms producers are lower than method 2 estimates. *Source*s: Appendices 2 and 3.

(3) $M_i = MP_i + MG_i$.

Combining (2) and (3) then gives

(4) $S100_i = P_i + (M_i - MG_i)$

and, hence,

(5) $-MG_i = S100_i - P_i - M_i$.

Equation (1)—that is, $P_i = D_i + X_i - M_i$ from method 2—can now be restated to accommodate the fact that only imports by governments constitute proper final consumption, whereas other imports are pre-products and components, which do not count as government final consumption. Thus,

(6) $P_i = D_i + X_i - MG_i$.

Combining (5) and (6) then yields

(7) $P_i = D_i + X_i + (S100_i - P_i - M_i)$,

(8) $2P_i = D_i + X_i + (S100_i - M_i)$, and

(9) $P_i = [(S100_i) + (D_i + X_i - M_i)] / 2$,

so that method 3 works out as averaging data from method 2 and data on the Top-100 arms producers. Note, however, that this correction for aspects of the component imports issue cannot work for countries that do not have companies in the

Top-100 list. Furthermore, in no case will the companies in the SIPRI Top-100 list represent *all* of the arms production being carried out. The share covered by companies from the list is also likely to differ from country to country, which means that the extent of the correction for component imports will differ, limiting the validity of comparisons among countries.

Method 3 has major limitations and shortcomings that reduce its value in correcting for the shortcomings of method 2. Still, appendix Table A3 presents preliminary estimates using this method. The data produced with method 3 differs substantially, at least for most countries, from that resulting from method 2. The data showing the percentage differences between the two estimate methods is presented in Table 3. The difference is small for the United States and, for Russia, has been steadily decreasing over time, having been quite large in earlier years. It is also large for most other countries, including the U.K. This is largely due to the lack of data on smaller companies in the SIPRI dataset, which leads to an upward bias in estimates that use method 3 for countries that host a disproportionate number of large arms producing companies and an underestimation of national production for countries where there are comparatively few large arms producing companies. While the lack of data on all arms producers introduces a bias in estimates based on method 3, the inclusion of data beyond sales to procurement authorities adds additional information which must be interpreted in light of this bias.

Table 4: Comparing methods to estimate countries' arms production

	Method 1	*Method 2*	*Method 3*
Definition of arms production	Very narrow but fairly clearly defined.	Broader but vague.	Similar to method 2 but with corrections for imports of components and pre-products by companies.
Components of calculation	Exports; procurement from domestic sources.	National procurement; arms imports; arms exports.	As for method 2, plus sales by SIPRI Top-100 companies.
Available relevant data series	SIPRI major weapons exports data; IISS *Military Balance*.	National and NATO data on procurement; national data on arms exports; U.S. government data.	As for method 2, plus SIPRI Top-100 companies data.
Major additional data required	Data on procurement of weapon systems by year from multiple sources.	Estimation of procurement and exports for many cases, reconstruction of imports from export data.	As for method 2, plus data on smaller arms producing companies.
Problematic issues	Data is not of production *per se* but consumption; inclusion of imports of most components and pre-products; estimates necessary for many countries.	Still consumption rather than production, although less problematic than for method 1; data from sources with differing definitions; estimates necessary for many countries.	As for method 2, plus differences between countries with respect to the share of national arms production represented by major arms producing companies from a particular country.

Conclusion

The three methods suggested in this article are based on different primary data sources, each of which comes with its own drawbacks. Combining data and comparing the results of the three methods can shed light on national arms production, where data has been particularly scarce for most countries. Nevertheless, all three methods have major limitations and shortcomings resulting from data gaps, differences between reported data and presumed definitions, and assumptions that must be made about the importation of pre-products and components used in national arms production. The methods adopt different approaches for overcoming these and other limitations and shortcomings, in part by introducing new ones. Without very substantial work on filling data gaps and making available data more commensurate with standard definitions, comparisons among countries and regional or global estimates will remain problematic. That said, rough estimates are possible with more limited extensions to available datasets.

Table 4 presents a summary of the three methods, highlighting in particular the differences between method 1, on the one hand, and methods 2 and 3 on the other. Method 1 corresponds to a very narrow conception of the arms industry, while the other two cover a wide range of production activities linked to demand for military products.

A rough comparison of three countries for which estimates were made using simple variants of the three methods reveals

substantial differences, particularly between method 1 as against methods 2 and 3 (see Table 5). The main reason is that the data for method 1 is constructed by using SIPRI's trend indicator values (TIVs), which in turn use estimates of prices for major weapon systems corresponding to their military use-value, whereas the other two methods are based on market exchange rates. To the extent that TIVs reflect actual military use-values, the prices used in the SIPRI system express a variant of purchasing power parities for major weapon systems.[26] Limited by the data that goes into their calculation, SIPRI's price estimates represent the international competitiveness of particular weapon systems based on physical characteristics.

The differences among the estimates are particularly striking for the case of Russia. Measured in terms of purchasing power parities, the output of its arms industry is substantially larger than the figure obtained when using market exchange rates. It is worth stressing again, however, that data issues, in particular the roughness of the estimate of Russian procurement and the different sizes of the various companies in the countries, may bias the comparison.

Despite the shortcomings and limitations of all three methods, the data presented in Table 5 does point toward an interesting result which requires further analysis (with the help of better data). Other comparisons may lead to other interesting conclusions.

None of the methods presented here provide an alternative to better and more standardized data on arms production. However, they may help to generate estimates that are in the right ballpark for comparisons among countries and for regional and global aggregates. To achieve this goal, all methods require additional work with regard to data, but that is likely to be more limited in scope than to get reliable standardized estimates for arms production by other methods.

Estimates that use method 1 are likely to produce valid results with substantial but reasonable effort based on proven methods for collecting data on procurement. The other two methods have more intricate data demands, particularly with respect to standardization. However, with some corrections to the available and rough estimates of missing data, they may serve to reduce the shortcomings of method 1, particularly its limited definition of arms production.[27]

The purpose of this article is to demonstrate that progress in producing estimates of arms production values can be achieved by methods that are largely based on existing data series on particular aspects of the military sector. My hope is to stimulate further work that extends to comparing the methods suggested here with other, previously proposed, methods. On the one hand, this work must be conceptual; it must assess whether there are better ways to deal with some of the shortcomings and limitations of the methods presented in this article. On the other hand, and primarily, it must also involve an empirical component, in particular the production of more standardized data on procurement.

Table 5: Relative size of arms production, selected countries, 2015 (USA=100)

	Method 1	*Method 2*	*Method 3*
France	9.1	7.5	11.4
Russia	42.3	12.5	11.7
USA	100	100	100

Sources: Appendix Tables A1, A2, and A3.

Notes

1. *Military spending*: SIPRI, Military Expenditure Database, https://www.sipri.org/databases/milex; United States Department of State, various years; IISS, various years. World Bank, World Bank Open Data, https://data.worldbank.org/. *Arms exports*: SIPRI, Arms Transfer Database, https://www.sipri.org/databases/armstransfers; United States Department of State, various years; Theohary (2017). *Arms producing companies*: SIPRI, Arms Industry Database, https://www.sipri.org/databases/armsindustry. *Procurement spending*: For NATO member states: NATO, various years. *Military R&D*: For OECD member states: OECD, Research and Development Statistics, https://stats.oecd.org/Index.aspx?DataSetCode=GBAORD_NABS2007.

2. *For several reasons*: See also Fleurant and Tian (2018); Dunne (2009); Hartley (2018); Hartley and Belin (2020). *Industrial production*: Beyond what is proposed by Wulf (2018), using data from the SIPRI Arms Industry Database. *A country's economy*: Care would have to be given to ascertain the compatibility of the results of such estimations with economic data.

3. *Three methods*: Unless stated differently, "arms" and "arms production" are used throughout this text interchangeably with "military equipment" and "defense production".

4. *Data is rare*: Exceptions include Brzoska and Ohlson (1986) and Neuman (2006); see also Wulf (2018) and Hartley and Beilin (2020). *Increasingly integrated*: See, e.g., Wulf (2003); Dunne (2009); Hartley (2018).

5. *International Standard Industrial Classification*: United Nations (2008). *Industrial Commodity Statistics*: https://unstats.un.org/unsd/industry/Commodity/TechNotes.cshtml.

6. *Difficulties encountered*: See, e.g., Brzoska (1995); Chalmers, Donowaki, and Greene (1997).

7. *Sales by major companies*: Fleurant and Nan (2018).

8. https://www.sipri.org/databases/armstransfers.

9. SIPRI kindly provided TIV data for the weapon systems used for the calculation in Table A1 for which TIVs exist in the SIPRI database.

10. See https://www.sipri.org/databases/armstransfers/sources-and-methods#Coverage.

11. The same method could in principle be used for weapon systems that are only procured domestically and for which there is no TIV in the SIPRI system. However, estimation of prices on the basis of the physical characteristics of weapon systems may be preferable to using actual production costs, even where available. In economic terms, there is a fundamental difference between the two methods used by SIPRI for obtaining TIVs described above. While actual cost may or may not be competitive, using physical characteristics for estimation assumes that they are. The difference is likely to be less important for weapons that are internationally traded (and thus generally need be competitive) than for weapon systems that are only procured domestically (and may therefore be highly subsidized). When combining procurement and trade data, as suggested here, it makes sense to use TIVs that assume the competitiveness of prices of weapon systems based on their values. Further information of the details of SIPRI's TIV estimation, would, however, be necessary for any outside researcher who wanted to make such estimates independently of SIPRI.

12. Robertson and Adrian (2017) have developed, for the example of China, a relative military cost/price index based on the relative unit costs of inputs into arms production. This is in the spirit of using data on (civilian) industries (see, Wulf, 2018, and Yesilyurt, *et al*. 2014) but would require much additional work to arrive at estimates for many countries.

13. See SIPRI's method section regarding its major weapons transfer data, https://www.sipri.org/databases/armstransfers.

14. SIPRI Arms Transfer data covers all fixed-wing aircraft and helicopters, including unmanned aircraft (UAV/UCAV) with a minimum loaded weight of 20 kg, air defense systems, naval ships, anti-submarine warfare weapons, all vehicles with integral armor protection, including all types of tanks, guided missiles, bombs and shells, reconnaissance satellites, and artillery with a caliber equal to or above 100 mm, as well as engines, major sensors, and selected components for the listed weapon systems. For details, see Sources and Method, https://www.sipri.org/databases/armstransfers/sources-and-methods#Coverage.

15. The prime reason for SIPRI's limitations in the coverage of arms transfers is the scarcity of data beyond major conventional weapons. That said, there have been several attempts to estimate production and trade in small arms, particularly by the Geneva-based Small Arms Survey (http://www.smallarmssurvey.org/) and the now-defunct Norwegian Initiative on Small Arms Transfers (http://nisat.prio.org/). This data could provide a basis for rough estimates on small arms and light weapon production to supplement the data produced with the methodology outlined here.

16. See https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX%3A52015XG0421%2805%29.

17. As discussed later on, not all imports are carried out by governments. At this point, because of a lack of data to distinguish imports by governments from imports by companies (which become pre-products and components), this issue will be ignored.

18. *European Union data*: Available since 1999 and published in pdf format by the European External Action Service https://eeas.europa.eu/headquarters/headquarters-homepage/8472/annual-reports-on-arms-exports-_en. *NATO data*: See https://www.nato.int/nato_static_fl2014/assets/pdf/pdf_2017_06/20170629_170629-pr2017-111-en.pdf. Listed are: (2.1.1) missile systems; (2.1.2) missiles (conventional weapons); (2.1.3) nuclear weapons; (2.1.4) aircraft; (2.1.5) artillery; (2.1.6) combat vehicles; (2.1.7) engineering equipment; (2.1.8) weapons and small arms; (2.1.9) transport vehicles; (2.1.10) ships and harbor craft; and (2.1.11) electronic and communications equipment. NATO's "equipment expenditures" list does not include the procurement of components and pre-products for spare parts or for ammunition. On the other hand, procurement for nuclear forces is included. One of the academic studies that have used this data is Bove and Cavatorta (2011).

19. United States Department of State (2017). Prior editions are available at various places on the internet and, for earlier years, in printed form. See also Theohary (2017) for another report that uses U.S. government data on arms exports in a differing format.

20. WMEAT arms import and export data is a mixture of actual financial flows and estimates of the value of arms transferred. WMEAT therefore cautions against comparing the value of arms imports to values for other parameters, such as GDP or military expenditure. Inconsistencies in the WMEAT data relate to inconsistent services, dual-use goods, as well as data on licence applications versus actual deliveries; see United States Department of State (2017), Sources, Data and Methods section. A particularly puzzling case is that of U.S. arms exports, which are extremely high in the WMEAT data, corresponding to over 80 percent of the global total for the period 2011–2015 (United States Department of State, 2017, Table II). The high U.S. numbers are explained in WMEAT as deriving from the inclusion of commercial sales in the data. In many countries, however, arms sales are predominantly by commercial sellers.

21. In principle, a synthetic control approach would be preferable by constructing a "synthetic" country for which data is needed from the range of countries of which reliable data is available. At least for the time being, however, the data on procurement is so limited that such an estimation does not seem possible.

22. United States Department of State, various years; see also Theohary (2017).

23. National income can, for instance, be arrived at by aggregating the value-added of the relevant companies, thus eliminating double-counting. Some economists, however, argue that much can be learned about the state of the economy by analyzing gross output; see, e.g., Skousen (2015). For 2016, the U.S. Bureau of Economic Analysis estimated U.S. gross output of USD32.4 trillion, compared to GDP of USD18.7 trillion.

24. In addition to the three methods suggested here, it would of course be possible in principle to arrive at estimates of national and global arms production by expanding the scope of companies to include smaller companies and by using company data on net production (or value-added) instead of sales data. This would, however, require collecting much additional data, some of which, such as value-added in arms production, does not seem to be widely available.

25. This is partly corrected for in the SIPRI data by listing major subsidiaries separately.

26. In addition to Robertson and Sin (2017), see also United States Department of State (2017), the Sources and Methods section of which contains a discussion on PPPs in the military sector.

27. Global estimates for methods 2 and 3 could be reduced to the aggregation of national procurement, such as imports and exports of arms, and certain components would cancel each other out.

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Appendix Table A1
Method 1. Illustrative estimates of major weapons production, selected countries, 2015 (2015 TIVs in millions)

Country/item	*No.*	*TIV/ item*	*TIV total*
France			
Rafale M F3	5	55	275
ASMP-A	5	10	50
**VBCI/VCP*	*10*	*10*	*100*
EC665 Tiger HAD	6	12	69
NH90 TTH	2	7	14
Rafale B F3	2	55	110
AS555UN Fennec	18	1	23
AS532UL Cougar	3	7	22
A-400M	2	73	146
SCALP Naval	20	1	28
Sum sub-total			**837**
+ major weapons exports			**2,017**
= Sum total			**2,854**
Russia			
SU-30 (SU-30MK)	30	55	1,650
**SSBN Borey Class*	*1*	*800*	*800*
**SU-34*	*16*	*50*	*800*
SU-35S	23	60	1,380
**Transport aircraft*	*28*	*20*	*560*
**SSBN improved Kilo Class*	*1*	*800*	*800*
BTR-82A	100	1	95
**Bumerang*	*100*	*1*	*100*
**TU-160*	*2*	*80*	*160*
MiG-29K (MiG-35SMT)	10	35	350
Ka-52 Hokum B	15	16	233
Mi-28N Hovoc	10	16	155
Mi-8AMTSh Hip	15	7	101
Mi08AMT-5 Hip	15	7	101
96K6 Pantsir-51	19	11	209
**96K6 Pantsir-51 9M311 missile*	*80*	*0*	*10*
Sum sub-total			**7,504**
+ major weapons exports			**5,842**
= Sum total			**13,346**

Country/item	*No.*	*TIV/ item*	*TIV total*
United States			
MQ9 Reaper UAS	12	8	90
C-130J Hercules	14	35	490
F-35 Joint Strike Fighter	34	55	1,870
**V-22 Osprey*	*19*	*30*	*570*
AH-64E Apache Helicopter	25	15	375
CH-47F Chinook Helicopter	32	20	640
UH-60M Black Hawk Helicopter	79	7	573
MH-60R Multi Mission Helicopter	29	18	522
MH-60S Fleet Combat Helicopter	8	18	144
P8A Poseidon	8	125	1,000
**E-2D Advanced Hawkeye*	*4*	*50*	*200*
**KC46A Tanker*	*7*	*80*	*560*
**AEGIS BMD System*	*30*	*120*	*3,600*
THAAD BMD System	31	250	7,750
AIM-120C AMRAAM Missile	15	1	9
**SM6 Standard Missile*	*110*	*1*	*55*
**DDG 51 AEGIS Destroyer*	*2*	*500*	*1,000*
**LCS Littoral Combat Ship*	*3*	*200*	*600*
**Virginia Class Submarine*	*2*	*800*	*1,600*
Sum subtotal			**21,648**
+ major weapons exports			**9,931**
= Sum total			**31,579**

Note: The data on the number and type of nationally produced weapon systems accepted into service in 2015 is preliminary and is used here to present the principles of method 1 alone. *Sources*: Procurement: France: IISS; United States; U.S. Department of Defense; Russia: IISS *Military Balance 2016*. TIVs and major weapons exports: SIPRI (2019) and author's estimates (in *italics*). Items listed in *italics* and preceded by an asterisk (*) are the author's estimates, not SIPRI's.

Appendix Table A2
Method 2. Illustrative estimates of arms production, selected countries, 2010–2015 (USD billions, in 2015 prices)

	2010	*2011*	*2012*	*2013*	*2014*	*2015*
FRANCE						
Procurement	15,692	14,207	15,508	12,437	12,345	12,423
Exports	5,332	5,342	4,435	5,242	5,109	6,721
Imports	1,600	1,700	1,800	1,400	1,100	1,100
Total	19,429	17,849	18,144	16,279	16,354	18,044
GERMANY						
Procurement	8,138	7,448	7,679	5,578	5,590	5,221
Exports	6,844	7,930	6,239	7,896	5,279	8,716
Imports	3,700	3,800	4,000	3,000	2,400	2,100
Total	11,282	11,578	9,918	10,474	8,469	11,837
UNITED KINGDOM						
Procurement	14,760	13,066	10,654	12,758	13,111	12,129
Exports	4,084	10,258	3,534	7,068	3,434	8,893
Imports	1,180	1,230	1,230	1,180	1,000	920
Total	17,664	22,094	12,957	18,646	15,545	20,102
UNITED STATES						
Procurement	173,073	195,849	185,092	166,526	158,710	150,800
Exports	56,865	66,852	52,836	41,918	82,686	95,393
Imports	5,900	5,400	5,900	5,400	5,200	4,700
Total	224,038	257,301	232,027	203,043	236,195	241,493
PR CHINA						
Procurement	37,903	40,126	43,523	44,585	49,178	51,521
Exports	3,500	1,800	2,300	2,800	2,100	2,900
Imports	1,200	700	1,100	1,000	1,300	500
Total	40,203	41,226	44,723	46,385	49,978	53,921
RUSSIA						
Procurement	11,630	12,171	14,089	13,846	15,083	15,984
Exports	10,870	14,436	15,691	15,974	15,618	14,500
Imports	700	600	600	500	700	200
Total	21,800	26,007	29,181	29,320	30,002	30,284

Note: The data is rough, and preliminary estimates are for the purpose of presenting the principle of estimation only.
Sources: *Procurement data*: For NATO countries, estimated by multiplying defense spending with the relevant share of "equipment expenditures"; data taken from NATO (2017). For Russia and China, estimated by multiplying data from the SIPRI military expenditure data series with the average share of "equipment expenditures" for France, U.K., and U.S.; data taken from NATO (2017). *Export data*: Taken from SIPRI database on the financial value of the global arms trade, except for China, which is taken from U.S. Department of State (2017) (identical to the export values in Theohary, 2016). Where the SIPRI database offered different options, the following were chosen: France, arms exports; Germany, arms export licences; U.K., arms export licences; U.S., arms exports (FMS) + arms export licences (commercial sales). *Import data*: U.S. Department of State (2016), except for the U.K., which is the author's own estimate.

Appendix Table A3
Method 3. Illustrative estimates of arms production, selected countries, 2010–2015 (USD billions, in 2015 prices)

	2010	*2011*	*2012*	*2013*	*2014*	*2015*
FRANCE						
Method 2	19,429	17,849	18,144	16,279	16,354	18,044
SIPRI Top-100 sales	24,864	23,338	23,955	24,925	22,936	24,763
Average of methods 2 and 3	22,146	20,594	21,049	20,602	19,645	21,403
GERMANY						
Method 2	11,282	11,578	9,918	10,474	8,469	11,837
SIPRI Top-100 sales	14,150	13,065	10,642	12,152	11,959	11,913
Average of methods 2 and 3	12,716	12,322	10,280	11,313	10,214	11,875
UNITED KINGDOM						
Method 2	17,664	22,094	12,957	18,646	15,545	20,102
SIPRI Top-100 sales	31,091	31,775	31,447	31,897	29,253	26,477
Average of methods 2 and 3	24,377	26,934	22,202	25,271	22,399	23,289
UNITED STATES						
Method 2	224,038	257,301	232,027	203,043	236,195	241,493
SIPRI Top-100 sales	291,151	269,765	248,498	236,295	225,958	214,778
Average of methods 2 and 3	257,594	263,533	240,263	219,669	231,077	228,135
RUSSIA						
Method 2	21,800	26,007	29,181	29,320	30,002	30,284
SIPRI Top-100 sales	11,856	13,089	16,634	20,923	24,223	25,093
Average of methods 2 and 3	16,828	19,548	22,907	25,122	27,122	27,688

Notes: The data for companies was aggregated by country, as given in the SIPRI database. Where subsidiaries are listed under a different country than their holding companies, corrections were made to the sales of holding companies. Sales of trans-European companies were divided between France (40%), Germany (40%) and the UK (10%). *Source* for sales data: SIPRI arms industry database.

Don't just click 'download': The case of U.S. military expenditure data

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Abstract

With a view toward two purposes, the article examines measures of United States military expenditure. It first discusses what types of data would be most suitable for the analysis of the economic burden of such expenditure and it explains, second, why existing databases with U.S. (and global) military expenditure coverage have limited validity, and therefore utility, for the first purpose. The article advances the concepts of minimal and maximal augmented military expenditure measures to better capture the full economic resource burden imposed on an economy than do the presently available measures. The difference of current measures to the proposed augmented measures is not primarily one of an intercept change but mostly of a slope change. The article claims that, by fiscal year 2018, the economic burden imposed by military expenditure on the U.S. economy when using the augmented measures is about double the size of the burden as measured by all of the currently available measures. Since, to date, most empirical work on the effects of military expenditure on economies tends to rely on inappropriate measures, misleading findings may have resulted.

Extensive theoretical and statistical literatures have developed which theorize, measure, and analyze countries' military expenditure and their economic causes and effects, if any. Among these, the literature on the relationship between military expenditure (as cause) and economic development in general and economic growth in particular (as effects) is very large, with recent reviews coming to the still hedged conclusion that most countries in most circumstances do not benefit economically from military spending, and probably suffer adverse economic consequences therefrom.[1] Yet inferential statistical analysis of any hypothesized relation between military spending and economic growth depends on a number of validity concepts among which are construct validity and content validity, the notions that (1) a measure used in an empirical setup should equal, or well correspond to, its theoretical companion construct and (2) that a measure should correspond to all facets of a given construct, not a selection thereof.[2]

To date, in applied statistical work, the often implied construct and content of military expenditure are not equal to its measure, and it tends to be the measure that drives causal conclusions in regard to the effect of military expenditure on economic growth. Needless to say, this harbors the possibility of unreliable findings having been reported in the literature. The reason for the mismatch between theory and empirics is straightforward: In most cases, the data used in empirical studies rely on countries' select budgetary rather than full (or at least, fuller) economic resource use measures of military expenditure. It is one thing to use readily downloadable military expenditure data compiled to give users a sense of countries' current-year military activity (often taken as a proxy for military capacity or capability) or to provide a sense of current-year government budget allocations. It is another matter entirely to use the same readily downloadable data in studies regarding economic questions. Indeed, even studies restricted to investigating budgetary trade-offs between military and nonmilitary budget items such as health, education, housing, and welfare cannot unquestionably rely on budget data alone as government line items in *agency* budgets (the departments of health, education, housing, and so on, as well as national defense) first need to be assigned to larger *functional* rubrics lest some portion of military expenditure, for example, be spread across various agencies and therefore not be captured within a single agency budget such as that of a Ministry of Defense (MOD). In the United States, for instance, military-nuclear activities are budgeted under the agency of the Department of Energy (DOE) but, clearly, contribute to the larger national defense function.

Most national government budgets are constructed on a cash basis for administrative purposes.[3] It is important to acknowledge that, as such, they have their valid uses. However, even if agency budgets are mapped onto larger government functions to which various agencies may contribute, such as national defense, and even if relevant aspects of various agency budgets are added up toward an overarching national defense functional category, they still may

not fully capture all economic resources devoted to a state's defense function. Further adjustments may be necessary. In the United States, the National Income and Product Accounts (NIPA) produced by the Department of Commerce's Bureau of Economic Analysis (BEA) can be viewed as an exercise in that direction. All federal receipts and expenditures are subject to adjustments, including coverage and timing adjustments (see the Appendix) and then are translated into NIPA categories. They are "measured on a national income and product account (NIPA) basis" (Ludwick and Brankin, 2018, p. 18). To emphasize the purpose of the translation, the BEA authors write that "[u]nlike the federal budget, which is a financial plan of the government, the NIPA federal sector estimates are designed to facilitate macroeconomic analyses of the effects of federal government activity on economic activity" (Ludwick and Brankin, 2018, p. 18).

It is this national income accounting framework that is relevant for economic analysis of defense or military activity (rather than for, say, current-year force capacity or force-capability analysis). As will be shown, however, even the BEA's NIPA numbers violate content validity as they still capture only a selection of the economic resources devoted to the U.S. military sector so that further adjustments to the data are required before their use in empirical economic analysis.

This article extends (and corrects) the descriptive portion of Brauer (2007), expanding his Augmented NIPA-based measure of United States military expenditure by an additional 38 years—from the 1962–2002 period (n=41) to the 1940–2018 period (n=79)—and compares it to six measures such as those of WMEAT, NATO, and SIPRI, which are among the most frequently used military expenditure data employed by researchers and global news media.[4] The six customary measures are discussed in the next section, along with an indication of why they are selective in their coverage of military expenditure. The section thereafter discusses two enhanced measures. First, the NIPA measure essentially converts the federal budget from a cash to an accrual basis and, it turns out, is little different from the customary measures, largely because it, too, is selective in its coverage. In contrast, an Augmented NIPA-based measure of military expenditure is less selective, and the resulting numeric difference is very large indeed. The article concludes that empirical economists might wish to rethink their reliance on found military expenditure data alone and construct their own country-specific series of economically relevant military expenditure data.

The article discusses construct and content validity of measures of U.S. military expenditure. It finds that all of the currently available measures fall short of capturing the opportunity cost of diverting resources from an economy's civilian to its military sector. The article then constructs minimal and maximal augmented measures of U.S. military expenditure and finds that for fiscal year 2018, the augmented measures are about double the size of measures currently used by researchers and global news media outlets. It is likely that scholars, journalists, government officials, and policymakers do not fully appreciate the size of the opportunity cost of U.S. military expenditure.

Six customary measures of U.S. military expenditure

For the United States, there exists a single, ultimate source for military expenditure data. That is the *Budget of the United States Government* or, more precisely, the *Historical Tables*, a document supplementary to the fiscal year budget request made each year by the governing administration to Congress and issued annually by the Office of Management and Budget (OMB) within the president's office. In contrast to fiscal year budget requests and subsequent congressional budget authorizations and appropriations (which are fiscal year spending limits and subsequent to which supplementary appropriations may be made), the *Historical Tables* (HT) capture the actual outlays incurred in prior fiscal years. The outlays data presently are compiled in two ways, important to understand the distinction between agency-based data and economic resource use data. This section discusses details of the first of these. The second compilation and a new, third, one are discussed in the follow-on section.

Compilation No. 1

The first type of compilation—of the type that WMEAT, NATO, and SIPRI construct, and therefore the one that most researchers and news media looking for cross-national military expenditure data rely upon—picks a selection of U.S. federal government agency budget line items that, for example, fit NATO's military expenditure definition.[5] So do WMEAT and SIPRI. For SIPRI, at least, the intention is to measure current-year military activity not as a proxy for military output or strength but as an input, "an easily identifiable measure of the scale of resources absorbed by the military," and this may or may not equate to its full opportunity cost.[6]

As it turns out, WMEAT and NATO data are virtually identical for 1989–2016 (the latest available, comparable data), as were NATO and SIPRI data for 1949–2005. As from 2006, however, NATO (and therefore WMEAT) data exceed SIPRI's. This may be due to NATO's recent inclusion of budget items relating to U.S. intelligence services, which SIPRI excludes, although NATO's definition does not refer to intelligence services directly.[7] Since 2006, the NATO-to-SIPRI overage has averaged 8 percent, ranging from a high of 17.5

percent in FY2008 to a low of 3.2 percent in FY2010. Even at 8 percent, the difference nonetheless is small relative to the economically more relevant Augmented NIPA-based account, as shown in the next section.

Still other U.S. military expenditure data sources include the World Bank, the International Monetary Fund (IMF), and the United Nations Office for Disarmament Affairs (UNODA).[8] The World Bank reports military expenditure data under license from, and therefore equals, SIPRI's. The IMF also relies on SIPRI (e.g., IMF, 2019). UNODA's data are not used by research economists, for multiple reasons. The time periods covered can be short (for the United States only as of FY2002) and the data are at times inconsistent and often miss many years. UNODA also reports data with a greater time-lag than do the other sources. Its numbers stem from states' self-submitted data, based on states' agency budgets adjusted to fit UNODA's rather than NATO's more expansive definition of military expenditure.[9] Indeed, NATO's numbers generally exceed UNODA's, in one instance by over USD106 billion (in FY2008). State responses to UNODA data requests are voluntary, and the number of respondents has dropped from more than 70 states in the 2000s to about half that number in 2018.

Table 1: Comparison table (real 2012 dollars, in billions, fiscal years)

	2014	*2015*	*2016*	*2017*	*2018*
Compilation No. 1					
HT 051 = DOD	557.4	536.8	533.7	527.0	554.9
HT 050 = NDO = DOD+	582.0	562.7	560.1	554.6	582.7
UNODA	571.9	n/a	n/a	n/a	n/a
SIPRI	588.3	568.9	566.5	564.9	n/a
NATO	630.7	611.9	619.3	595.6	620.0
WMEAT	630.8	611.7	619.2	n/a	n/a
Compilation No. 2					
NIPA NDCE	577.4	560.6	555.3	555.8	577.4
NIPA NDGIE	138.7	135.6	131.2	133.3	128.2
NIPA NDCGIE	716.1	696.2	686.6	689.1	705.6
Compilation No. 3					
Augmented NIPA	1,089.0	1,065.6	1,092.2	1,107.8	1,202.8

Note: Latest available data. Numbers are rounded. *Sources*: See text.

In sum, until 2005 the three major international data sources researchers and news media have used to gauge U.S. military expenditure data—WMEAT, NATO, and SIPRI—all reported almost identical figures. Since then WMEAT and NATO remain almost identical but, as noted, NATO/WMEAT and SIPRI data have begun to diverge (on average by USD47 billion/year; or USD35 billion/year when excluding the untypically large divergences of FY2008 and FY2009).

As indicated, the *Historical Tables* distinguish outlays by government function from outlays by the agencies that carry out one or more functions. For example, while functional budget line item 051 includes outlays only of the Department of Defense (DOD) agency, the budget of the Department of Energy (DOE), another agency, includes some defense-related functions, specifically military-atomic energy (functional budget line item 053). Thus, the overall National Defense Outlays (NDO) functional budget line item 050 is broader than that of the DOD agency alone and therefore exceeds the DOD-related functional line item 051. Compared even to this more comprehensive functional budget line item, NATO adds an average of USD14 billion/year (averaged over 1949–2018). As with the case of SIPRI, this average annual "add-on" increased drastically with FY2006, rising to an average of USD55 billion/year. Compilation No. 1 in Table 1 shows comparative data for some recent years. Consider the numbers for FY2017 for example. *Historical Tables* functional budget line item 051 (the DOD agency budget) amounts to USD527.0 billion. Summing line items 051, 052, 053, and 054 (defense-relevant items from DOD, DOE, and some others) results in functional line item 050 (National Defense Outlays) of USD554.6 billion (the DOD+ line in the table). SIPRI's number adds another USD10.3 billion to reach a total of USD564.9 billion. NATO ups this to USD595.6 billion.[10]

NIPA and Augmented NIPA data

Compilation No. 2

The second compilation of *Historical Tables* data comes from the National Income and Product Accounts (NIPA), produced by the Bureau of Economic Analysis (BEA) in the U.S. Department of Commerce (DOC).[11] NIPA's NDCE number, that is, National Defense Consumption Expenditure, which for FY2017 comes in at USD555.8 billion, includes a depreciation charge due to prior years' National Defense Gross Investment Expenditure (NDGIE). Adding the FY2017 gross investment of USD133.3 billion brings the combined National Defense Consumption and Gross Investment Expenditure (NDCGIE) to

a total of FY2017 USD689.1 billion.

As mentioned, the NIPA's are constructed with a national income accounting purpose in mind, that is, ultimately the production of a figure for Gross Domestic Product (GDP). For current-year GDP production—the monetary value of all goods and services produced in a country in a year's time—it is fine to include both investment in new defense-related equipment, structures, and other assets as well a depreciation charge to account for the use (or consumption) of past such investments in the current year's production of defense services. But as a measure of current-year military-related readiness activity, this would amount to double-counting—as investment is geared at future readiness while past investment is captured through the depreciation charge—and thus cannot stand. Instead, the relevant number to track is just the defense consumption item (NDCE), shown as the bold-font, red-colored, dashed line in Figure 1.[12] As can be seen, NIPA's NDCE numbers lie well within the cluster of the six customary Compilation 1 lines that show the two functional *Historical Tables* measures (budget line items 050 and 051), SIPRI, NATO, WMEAT, and UNODA for all years since FY1940. (All numbers are inflation-adjusted using the GDP implicit deflator measure, with 2012 serving as the base year.)

Yet, as is argued shortly, by FY2018 all Compilation 1 and 2 lines fall well short, by over a half trillion dollars, of a fuller economic accounting of U.S. military expenditure. This is the Augmented NIPA-based measure (Table 1, Compilation 3). If the red-colored, dashed NDCE line in Figure 1 has, in effect, no effect, the same cannot be said of the eighth line, the bold-font, teal-colored, dashed line that runs atop all others in Figure 1. Since 1940, this Augmented NIPA line rises with a noticeably steeper slope than do the others and might well affect regression coefficients in model estimations of the effect of military expenditure on economic growth. This is the point that, I believe, applied research economists need to grasp. What accounts for this massive (and rising, over time) increase?

Compilation No. 3

To construct an Augmented NIPA-based measure of U.S. military expenditure, a third data compilation is necessary as the BEA's translation of U.S. budget numbers into the national income and product accounting framework still leaves out three crucial aspects. First, BEA's treatment of homeland security expenditure in the NIPA; second, its treatment of military legacy costs of which, third, the quantitatively most important one concerns net interest payments incurred on federal debt obligations. The detailed discussion that follows constructs what might be called a *Maximal Augmented NIPA* measure of

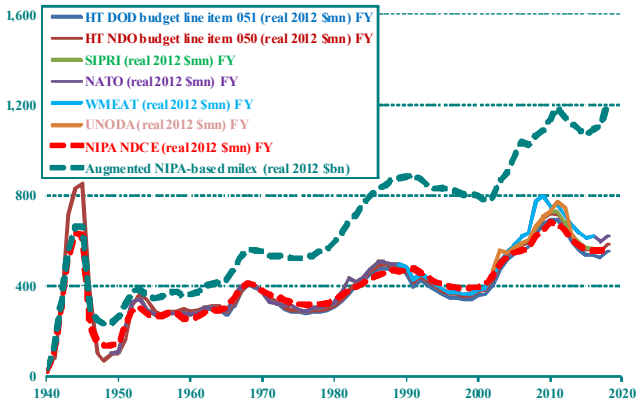

Figure 1: Measures of United States military expenditure (real 2012 millions of dollars). Sources: See text.

military expenditure. Thus, in the subsequent discussion and conclusion, a suggestion also is made on how one might construct a corresponding *Minimal Augmented NIPA* measure.

Homeland security

To understand treatment of homeland security data, first go back to the U.S. budget data (*Historical Tables*). The U.S. Department of Homeland Security (DHS) was established in 2002, following the 11 September 2001 terror attacks on the country. In terms of the U.S. budget documents, the *Historical Tables* data reclassify all pre-2002 spending so as to create a "phantom" DHS agency line item that starts in 1962.[13] According to the DHS website, the department was constructed by compiling some "22 different federal departments and agencies into a unified, integrated Cabinet agency."[14] Thus, going backward, the budget items for the departments and agencies that formerly carried out DHS-type activities now carried out by DHS were reduced in order to establish the DHS "phantom" agency line item for 1962–2001. Thereafter, of course, the phantom agency became an agency in its own right. Unfortunately, from the budget documents alone one cannot determined just how the DHS agency budget—phantom and otherwise—in turn is reclassified into function budgets as correspondence tables are not made public as part of the *Historical Tables* document. As mentioned, functional budget line item 050 is the whole of National Defense Outlays (NDO). The next super-category line item is 150 (International Affairs), followed by 250 (General Science, Space, and Technology), and so on. As there is no corresponding super-category functional budget line for homeland security, the implication is that DHS spending is distributed across all the other government functions but in an unknown way.[15] Even without a correspondence table, one can in some cases deduce the mapping of agency-to-function items, either fully or

partially. For instance, there is a super-category line item 700 (Veterans Benefits and Services) with a FY2018 function budget of USD85,535 million as opposed to USD178,513 million for the Department of Veterans Affairs (DVA) agency, which leaves DVA outlays of USD92,978 million (about 52 percent) reclassified to other, unknown, budget functions which may or may not include the defense function. The DOD–Military Programs agency budget in FY2018 was USD600,714 million, and for an agency referred to as Other Defense Civil Programs it was another USD55,367 million. The DOD agency budget shows up unchanged in the functional assignment (line item 051) but of the Other Defense Civil Programs only USD9,528 million carry over to function budget line item 054, called "Defense-related activities".

From Ludwick and Brankin (2018), confirmed by personal correspondence with BEA economists, neither BEA's NIPA NDCGIE, nor its consumption and gross investment components, include any homeland security agency or function outlays and, therefore, the whole of the DHS agency budget could be added to NIPA's NDCGIE figure but it is not known which DHS portion should be added to NDCE and which to NDGIE. For the illustrative purposes of this article, and at least as an initial step, the whole DHS agency budget has been added to NIPA NDCE. Including the entire DHS budget in the Augmented NIPA surely overstates things. DHS does support military activities such as intelligence gathering, and possibly the U.S. Coast Guard, but not all DHS spending will be military related. But without a detailed, line-by-line DHS budget breakdown, one cannot know which parts to count nor know which parts are consumption and which are gross investment.[16] Of course, one can simulate and include just half or even none of DHS in the Augmented NIPA-based numbers and this is briefly discussed in the concluding section.

Legacy costs

As is clear by now, measuring military expenditure is not a straightforward exercise. Another problem area concerns the legacy cost of past military activity. Note, for instance, the treatment of retirement pensions. SIPRI includes pension contributions to former military employees of ministries of defense (MOD)—the Department of Defense (DOD) in the case of the United States—regardless of whether pension contributions are budgeted under the MOD rubric or elsewhere.[17] In contrast, NATO claims that it includes both the in-service pension contributions as well as the post-service actual pensions of MODs' military and civilian personnel (NATO Press Release, 14 March 2019, pp. 14–15). But if this is so, NATO and SIPRI numbers should not have been equal (until 2005). SIPRI defines military expenditure on its website to include expenditure on "personnel, including: a. salaries of military and civil personnel; b. retirement pensions of military personnel, and; c. social services for personnel" (quoted from SIPRI's military expenditure data website) but clarifies in an email that "retirement pensions of military personnel" refers to the employer's contribution to the employees' retirement fund, not to the pensions themselves. In any case, SIPRI excludes retirement fund contributions to MODs' civilian personnel, whereas NATO claims to include that.

Either way, if SIPRI aims to exclude all legacy costs[18] and if NATO includes one type of legacy cost (pensions) but not others in their respective military expenditure compilations, the question arises of why to exclude any or all legacy costs in the first place if one wants to use military expenditure data to study its economic effects on an economy? If one does include pensions, as NATO claims, why not also include coverage of veterans' continuing health care and other needs? This is inconsistent: If pensions are included as current expenditure due to prior military activities, one cannot exclude other current expenditure on prior military activities such as veterans' health care coverage and other benefits they may receive. Similarly, BEA's NIPA numbers include pensions and pension contributions (see Ludwick and Branklin, 2018, Table 4) but exclude the whole of the budget of the Department of Veterans Affairs (DVA) from the computation of national defense consumption and gross investment expenditure (NDCGIE).

The crux of the matter regards content validity: Does the content of a measure match the theoretical construct one hopes to capture with that measure? To be valid, the measure cannot be selective in its coverage. In the case of military expenditure, one cannot include some legacy costs (pensions) and leave out others (health care and other non-pension services). Either exclude both or include both. For the purposes of this article, the whole of the DVA agency budget has been added to NIPA's NDCE fiscal year numbers on the economic argument that one must look at the allocation of current-year economic resources regardless of when a future resource-use obligation may have been incurred. (As noted, for FY2018 the super-category function budget for Veterans Affairs is but half of the DVA agency budget. Later on I will comment on the size of the "add-in" when producing Augmented NIPA numbers.) Thus, if hiring a soldier in the year 1970 includes or implies a contractual promise to provide pension and health care benefits in the year 2018, then the actual 2018 spending fulfills the promise made and is to be counted as military expenditure in terms of the use of total economic resources available to government in 2018. If, in contrast, one were interested in current-year force capacity or capability—which, one ventures,

is what most defense and media analysts are interested in—then it would be proper to exclude all legacy-related payments, not just health care but also all pensions. In a word, when downloading numbers off websites, one must consider the purpose of one's analytic interest and must possibly reconstruct any downloaded numbers so that they fit that purpose.

Net debt (issued and redeemed) and net interest paid

A third adjustment to NIPA's NDCGIE numbers concerns the treatment of net debt and net interest paid. Net debt issued finances current-year government activity, including military-related activity. For instance, by the end of FY2018 that year's federal budget deficit amounted to (nominal) USD779 billion, covering the shortfall between receipts of USD3,329 billion and outlays of USD4,108 billion. The U.S. Treasury's *Monthly Treasury Statement* details how this shortfall is financed.[19] These are complex and offset, for example, total borrowing needs against intra-governmental borrowing and changes in government cash-on-hand. The upshot is that FY2018 total net borrowing from the public amounted to USD1,084,458,000 (~$1 trillion). With the national defense budget function constituting USD665 billion or 16.2 percent of the USD4,108 billion in outlays that year, one might argue, incorrectly I think, that 16.2 percent of the net borrowing from the public (USD176 billion) is attributable to the defense function, a diversion of national resource flows away from the private sector or nonmilitary government purposes.

The issue to consider here is whether to count debt as an opportunity cost. Does the financing of government activity matter or only its claim on actual real resources, as NIPA suggests? If, say, USD100 of national defense outlays (NDO) were financed not by net borrowing from the public but by increases in taxation or a corresponding reduction in nonmilitary government outlays, what would change? To illustrate, consider two scenarios in one of which the USD100 is wholly financed by taxes and, in the other, wholly by new debt. In either case, we would count NDO of USD100—since that is the outlay, which NIPA converts to an accrual basis—so that the opportunity cost is no less when financed by taxes than when financed by debt. The future redemption of the debt amounts to a deferred tax, and the question would be when to count the tax burden, in the year debt was issued or the year, or years, during which the debt was redeemed (the principal repaid). None of this reasoning invalidates the construction of the NIPAs, given BEA's GDP objective. (In practice, much of the net borrowing is needed to roll over debt—redeem old debt by issuing new debt—and to pay interest obligations on past debt raised.) At issue is not the debt, nor its redemption, but the additional cost that the debt imposes, the interest on debt.

The U.S. president's own budget proposal, forwarded to Congress for debate, acknowledges the overwhelming contribution of military expenditure to the nation's accumulated debt and, hence, to the payment of interest on that debt (e.g., *Historical Tables*, FY2020, pp. 5, 6, 7, 8, etc., as well as in any number of *Historical Tables* documents for preceding fiscal years). Yet NIPA does not allocate a military-assignable portion of the interest paid to the NDCGIE category (nor does any other data source). BEA's argument is the following: "Government interest payments, although included elsewhere in the NIPA's, are not considered to be a payment for factor services; they, therefore, are not recorded in the government production account" (BEA, 1988, p. 4). And elsewhere: "Estimates of real spending by function refer to real government consumption expenditures and gross investment by function, which appear in NIPA table family 3.15 and which constitute a portion of GDP. These estimates exclude other types of government expenditures—such as social benefit payments, grants-in-aid, interest payments, and subsidies—that do not directly contribute to GDP" (BEA, 2005, p. IV-4).

Interest paid on debt obligations is not a current-year military production or service activity, true. Yet such payment does absorb current-year economic resources, the opportunity cost of which lies in foregone nonmilitary uses, private or public. Moreover, the interest has to be paid both on the defense consumption and on the defense gross investment expenditure. One might argue that interest payment recipients recycle the receipts into private sector consumption or gross investment, thus channeling the monies back onto the nonmilitary expenditure side of GDP (at home or overseas, for the portion of debt held outside the U.S.). True again, but had debt investors not lent funds to finance government military activity in the first place, they would have invested elsewhere and also recycled any interest received. At any point in time, funding the military side of GDP makes the nonmilitary side smaller than otherwise it might have been. It is not just adherents to the Austrian school of economics who appreciate that military-related debt and interest payments can help finance a skew in the economy's productive structure.[20]

For the purposes of this article, therefore, the portion of federal net interest payments on federal debt assignable to the total (consumption and gross investment) military function of government has been added to NIPA's NDCGIE numbers.

The Augmented NIPA numbers

To illustrate the construction of Augmented NIPA numbers for FY2018, the latest available at the time of writing, consider the following computations (Table 2). Ignoring rounding errors,

Table 2: Augmented NIPA (in real 2012 billions of dollars, rounded, FY2018)

NDCE*	577.4
+ Dept. of Veterans Affairs (DVA)	160.2
+ Dept. of Homeland Security (DHS)	76.3
= Subtotal	813.9
+ Allocated net interest on national debt	389.0
= Augmented NIPA	**1,202.8**
Computation of allocated net interest	
Federal government net interest payments	493.7
NDCGIE*+DHS+DVA	942.0
FGCGIE*	1,195.7
NDCGIE/DHS/DVA share in FDCGIE (USD942.0 bn / USD1,195.7 bn)	78.8%
=>Allocated net interest (78.8% x USD493.7bn)	389.0

Sources: See text. *Note*: NDCE, NDCGIE and FGCGIE are, respectively, national defense consumption expenditure, national defense consumption and gross investment expenditure and federal government consumption and gross investment expenditure.

the Augmented NIPA of USD1,202.8.5 billion (that is, 1.2 trillion dollars) is the sum of (1) NIPA's NDCE [USD577.4 billion], (2) the Department of Veterans Affairs' budget [USD160.2 billion], and (3) the Department of Homeland Security's budget [USD76.3 billion]—for a subtotal of USD813.9 billion—plus (4) interest payments assigned to the country's total military function [USD389.0 billion]. The interest payments are computed as follows: NIPA records federal government net interest payments as USD493.7 billion. It also records Federal Government Consumption and Gross Investment Expenditure (FGCGIE)—defense and nondefense combined—as USD1,195.7 billion, the NDCGIE defense share (USD705.6 billion), plus DVA (USD160.2 billion) and DHS (USD76.3 billion), of which is 78.8 percent. That percentage applied to the net interest payments equals USD389.0 billion (0.788 x 493.7). One can repeat the exercise back to FY1940 and plot the resulting line, the bold teal-colored, dashed line in Figure 1. Thus constructed, the augmented U.S. military expenditure measure has grown over time far more than have the other measures as the underlying accumulated debt and hence interest attributable to the total military function (NDCGIE+DHS+DVA) have grown.

For the United States, the global news media frequently report a military burden—the percentage of military expenditure (milex) to gross domestic product (GDP)—such as 3.1 percent for FY 2018, if military expenditure is taken to be the budget's functional National Defense Outlays (NDO) line item 050. (In real 2012 dollars, that is USD582.7 billion divided into USD18,571.3 billion.) Even though SIPRI's and NATO's military expenditure data are larger than the NDO figure, this rarely moves even the first decimal in the military burden number as the U.S.'s underlying GDP is so large.[21] In contrast, when using the Augmented NIPA measure the military burden rises from 3.1 to 6.5 percent of the nation's GDP, more than double the 3.1 number researchers and the news media tend to use. Put differently, in terms of economic resource use, for FY2018 the burden is not three cents on the dollar, but six-and-a-half cents on the dollar.

Discussion and conclusion

Whichever data one applies to statistically test a hypothesis developed from theory, it should at least meet the criteria of construct and content validity. As constructed in this article, the Augmented NIPA data probably overstate the economic resource use for military or defense purposes—the data may be thought of as *Maximal Augmented NIPA*—but they possibly do capture the vast proportion of such spending. For example, if the DVA and DHS agency budgets contribute even half as much as assumed here, then the overstatement would be less than 10 percent, and the resulting military burden 5.9 instead of 6.5 percent—still a substantial increase over the 3.1 number generally reported in the news media. This is because the annual net interest paid on national debt due to the total national defense effort is so large as to overwhelm the addition or subtraction of a few other items. Indeed, given the national debt loads carried by central governments around the world, it should be relatively straightforward to make progress toward a *Minimal Augmented Milex* measure by adding to national defense budgets the annual net interest obligation due to national defense budgets' share in annual central governments' budget deficits.

I reach three conclusions. First, research economists would do well to refocus on a "Mind Your Purpose, Mind Your Data" stance. If the research purpose at hand concerns current-year military capacity or capability, then military legacy costs, including net interest on national debt, are irrelevant, of course. But if the purpose concerns opportunity costs, then something akin to the Augmented NIPA data should be used (and developed for countries other than the United States, many of which carry far larger interest burdens, relative to GDP, than does the United States). Put differently, "don't just click on the 'download data' button".

A second, related, conclusion concerns the need to deposit new data assemblies, along with documentation, to a reliable

and credible institutional home for eventual panel dataset collation across time and countries.

Third, inasmuch as global news media help generate public understanding and sentiment regarding countries' military expenditure they, too, need to heed the main lesson of this article as it is quite possible that whether the United States expends "merely 3" or "about 6" percent of the value of its entire annual economic production on its military efforts alone could make a political and/or electoral difference.

Notes

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1. Put differently, the case for military expenditure is best not made on economic grounds but on its own merits. See, e.g., Dunne and Tian (2016), Brauer, Dunne, and Tian (2019), Smith (2019), and the literatures cited therein.

2. In economics much data is collected using Keynesian constructs but not necessarily used in that way, so problems of construct validity are common. There is also an issue of temporal validity, measuring the same thing over time (Dunne, 1991). Additionally, there are problems of content validity. Gross domestic product (GPD), for instance, excludes non-traded production such as (most) household-related work and thus measures production selectively.

3. Some countries, such as Australia, Sweden, and the U.K., have switched to an accrual basis but this will not change the larger point the article makes as even on an accrual basis some types of military-related expenses are left out of the accounting. The U.S. also has an accrual measure, rarely used in practice, but fundamental to the discussion in this article.

4. Respectively, the *World Military Expenditures and Arms Transfers* publication, issued annually by the U.S. Department of State (see www.state.gov/t/avc/rls/rpt/wmeat/), the North Atlantic Treaty Organization (www.nato.int), and the Stockholm International Peace Research Institute (www.sipri.org). Data downloads are free of charge.

5. For NATO's lengthy definition, see p. 14 of its latest defense expenditure-related press release of 14 March 2019 at www.nato.int/cps/en/natohq/news_164482.htm?selectedLocale=en. Note that, despite its length, NATO's published definition of military expenditure remains opaque. Unhappily, NATO's press releases are not issued as standard downloadable spreadsheet files but as PDF files with uneven dating (sometimes in January, sometimes in March, sometimes in July, and so on) and with inconsistent coverage of time periods.

6. SIPRI is explicit about its intention: "The main purpose of the data on military expenditure is to provide an easily identifiable measure of the scale of resources absorbed by the military. Military expenditure is an input measure, which is not directly related to the 'output' of military activities, such as military capability or military security. Long-term trends in military expenditure and sudden changes in trend may be signs of a change in military output, but interpretations of this type should be made with caution." See Stålenheim and Sköns (2008, p. 242). For example, if a U.S. soldier is, say, twice as "productive" as a non-U.S. soldier but costs thrice as much, then mere monetary accounting as an input measure does not, of course, capture comparative military capacity or strength as output measures.

7. Personal communication from Dr. Nan Tian, Arms and Military Expenditure Program, Stockholm International Peace Research Institute (1 April 2019).

8. *World Bank*: See https://data.worldbank.org/indicator/MS.MIL.XPND.CD?view=chart. *UNODA*: See http://www.un-arm.org/Milex/home.aspx [accessed 11 April 2019]. An additional source, not widely used among researchers but gaining prominence in the news media, is the Sydney-based Institute for Economics and Peace (IEP) which publishes an annual *Global Peace Index* (GPI). The GPI includes a measure of military expenditure as a percentage of GDP, which is taken from the International Institute for Strategic Studies' annual *Military Balance* publication. Researchers tend not to use *Military Balance* data as its data sourcing remains opaque (and is not available free of charge either).

9. The U.S. data submission for FY2014 to UNODA states that it includes the following: "The military expenditures (actual outlays) are of the individual military departments (Army, Navy, Air Force) and the defense agencies within the Department of Defense, as well as the Department of Energy (for defense nuclear programs) and the Department of Homeland Security (for defense-related activities)."

10. As of 11 April 2019, WMEAT and UNODA numbers were not yet available for FY2017. As discussed, they tend equal or lie below NATO's.

11. NIPA's nominal US dollar numbers are available online at BEA's interactive data tables site, https://apps.bea.gov/iTable/index_nipa.cfm. Click "Begin using the data ..." and then click on Section 3 (Government Current Receipts and Expenditures). Then scroll down to and click on Table 3.9.5. (Government Consumption Expenditures and Gross Investment). At this point, the table can be modified to select all available years. When finally displayed, scroll down to the table's Line 17 [accessed 11 April 2019] to see the figures for National Defense Consumption and Gross Investment Expenditures (NDCGIE).

12. Unlike federal budget outlays, which are recorded on a cash basis, NIPA expenditures are recorded on an accrual basis. In the end, we are talking about the same aircraft, missiles, ships, and so on, just differently accounted for. The

dollar difference between the budget's NDO and NIPA's NDCE numbers is relatively minor.

13. See *Historical Tables*, FY2020, Table 4.1 Outlays by Agency, 1962–2024, starting on p. 74 of the document.

14. See https://www.dhs.gov/history [accessed 12 April 2019].

15. A 12 April 2019 request to the president's Office of Management and Budget (OMB) to supply a correspondence map or table went unanswered.

16. Similarly, portions of the DOD budget should be classified as nondefense outlays. The United States Army Corps of Engineers, for instance, is part of the DOD budget yet almost all the funding (~USD5 billion in 2017) is not related to military activities and thus would need to be deducted from the DOD budget. Presumably NIPA does this, but detailed correspondence tables to translate the DOD budget into NIPA categories are not (made) available.

17. To be clear, what is included is the in-service monthly fringe benefit contribution (the pay-in) to service personnel's future, post-service pension claims, not the post-service pension itself. The legacy cost is excluded.

18. SIPRI is explicit in its exclusion of non-pension benefits: "... current expenditures on previous military activities, such as veterans' benefits, demobilization, conversion and weapon destruction are excluded" (https://www.sipri.org/databases/milex/sources-and-methods#definition-of-military-expenditure [accessed 11 March 2019]).

19. https://www.fiscal.treasury.gov/reports-statements/mts/.

20. See, e.g., Kjar and Anderson (2010) for an Austrian School view. In contrast, Anderton and Carter (2019, pp. 148–149) for example discusses military–nonmilitary resource diversion within the context of the neoclassical Edgeworth box.

21. For FY2017, the latest available, SIPRI reports a military burden of 3.1 percent (SIPRI Yearbook, 2018, p. 158), which is the same 3.1 percent military burden number as computed off budget line item 050 (National Defense Outlays).

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Appendix

Major conceptual differences exist between national defense spending as reported in the *Monthly Treasury Statement* and in the *United States Budget* and how defense consumption expenditures and gross investment are measured in BEA's National Income and Product Accounts (NIPAs). To reconcile these differences, BEA makes certain adjustments to the raw data.

As discussed in the main text, National Defense Outlays are outlays include more than just the Department of Defense's own military outlays and include, for example, the Department of Energy's nuclear weapons programs. In addition to those sorts of adjustments, **coverage adjustments** are made to account for certain transactions that are included in the Budget but are excluded from the NIPAs (and vice versa). Some are *additions*. For example, there are imputations for the consumption of fixed capital, a depreciation-like measure included in defense consumption expenditures to reflect the contributions of fixed assets (aircraft, structures, ships, etc.) to

current period defense production—the largest difference, in dollar terms, between the NIPAs and the Budget—as well as imputations for pensions (accrual less cash), that is, an adjustment made to reflect when pension liabilities are accrued, not when they are funded. Other adjustments concern *subtractions*. For example, retiree Tricare benefits (Tricare provides civilian health benefits for U.S Armed Forces military personnel, military retirees, and their dependents), which are classified as transfers to individuals in the NIPAs and, additionally, transfers to the rest of the world, which include for example Afghan and Iraq Security Forces Funds, the Syria Training and Equipment Fund, and the Commander's Emergency Response Program.

Further, NIPA makes several **timing adjustments** to account for transactions that are recorded on a cash-basis in the Budget but are recorded on an accrual-basis in the NIPAs. These include *compensation timing* (e.g., payday adjustments if the first day of the month falls on weekend or holiday and paychecks are issued the previous work day and Medicare-eligible retiree health fund adjustments, an annual "lump sum" payment for a component of compensation that is spread out across the year in the NIPAs) and *procurement timing* (e.g., DOD disbursements occurring during quarters before and after the delivery of the weapon system, not just during the quarter in which it is delivered and timing adjustments made to reconcile NIPAs delivery approach to the disbursements approach used in the Monthly Treasury Statement).

See Ludwick and Brankin (2018) for further discussion and literature.

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