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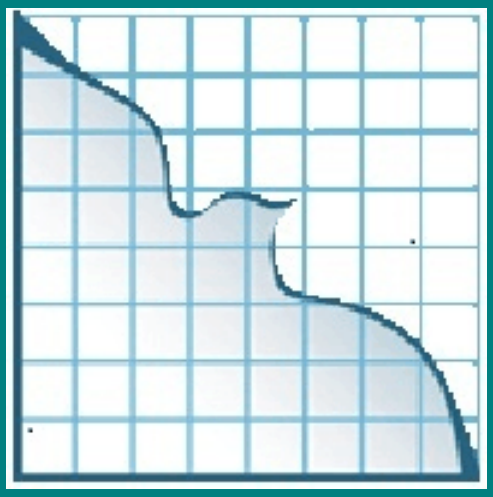

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'Tis but thy name that is my enemy: On the construction of macro panel datasets in conflict and peace economics

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Abstract

The empirical analysis of datasets covering a large number of countries and time periods has become an integral part of conflict and peace economics. As such, numerous studies examine relationships between and among macroeconomic, political, and conflict variables and this often involves the merging of disparate datasets to combine relevant variables for which the country unit of analysis, however, is not necessarily the same. This article highlights difficulties in the data merging process and, by way of example, presents detailed country coding unit comparison for two economic (UN Comtrade and World Development Indicators), two democracy (Polity IV and V-Dem), and two conflict datasets (UCDP/PRIO Armed Conflict Dataset and COW Militarized Interstate Disputes Dataset). We find that merging datasets can result in the elimination of very large numbers of observations due to unmergeable records and that dropped observations often include the very countries or territorial entities most of interest in conflict and peace economics.

In conflict and peace economics, the construction of large panel datasets nowadays forms the basis for the majority of empirical cross-country studies. Originating from different sources, such panel datasets contain measures on variables such as international trade, economic growth, GDP, armed conflict, democratization, and government effectiveness.[1] But bringing these variables together, that is merging them into a single dataset, hinges on the exact identification of the country unit under study. To permit reasonable statistical inference, the country unit for which, for example, the trade value is calculated, should respond to the same entity for which all other variables in the dataset are coded. Unfortunately, the names, and even the physical borders, with which countries are coded vary considerably across different data sources.[2]

At the core of the coding differences lies the question "What's in a (country) name"? We argue that there are two complementary parts to the answer. The first regards the entity under observation, the unit of analysis: What is a country? The answer depends on the research framework. For example, the purpose of the Russett, Singer, and Small (1968) state list as well as of the original Gleditsch and Ward (1999) state list was to capture recognized states in the international system. This particular definition of a country is of utmost relevance in analyses of authority structures. Nevertheless, one cannot blindly assume that the unit of analysis, that is, the country, is defined along the same criteria in economic or political datasets. Unfortunately, the burden of comparing the unit of analysis underlying different macro panel datasets lies with the scholar(s) attempting to merge them. As a consequence, we emphasize the importance of discussing the merging process in empirical studies in conflict and peace economics.

The second part to the "What's in a (country) name?" question concerns the entity's label: Numerous scholars have presented ways to adjust for differences in country labels. For example, Paul Hensel (2016) provides a thorough list of alternative historical state names and Heather Ba has created Stata files allowing for the mapping of country names, Correlates of War (COW) codes, and World Bank codes.[3]

That inconsistent country names across different data sources pose a problem is widely known among scholars working with macro panel datasets. Major attempts to standardize worldwide country coding already were undertaken half a century ago by Russett, Singer and Small (1968) and almost twenty years ago by Gleditsch and Ward (1999). Nevertheless, several problems remain unresolved and, unfortunately—with the emergence of readily available software packages and codes—a discussion of "what is the (country) unit of analysis" has become almost unfashionable. In spite of its tediousness and complexity, the country merging process is generally not discussed in academic papers (or in their supplementary materials).

The contribution of this article is hence twofold: First and foremost, it shows that in spite of all country coding scheme standardization efforts and relevant software packages or

codes, the problem of inconsistent country coding in macro panel datasets persist. We therefore want to re-raise awareness of this problem and encourage a discussion of it in empirical cross-country studies in conflict and peace economics. Second, by way of illustration, in the Appendix to this article we provide overview tables of some of the gravest discrepancies in country coding across datasets which facilitate quick cross-dataset comparisons of country units.

The contribution of this article is twofold. First, it shows that in spite of all country coding scheme standardization efforts and relevant software packages and codes, the problem of inconsistent country coding in merging diverse macro panel datasets persists. This can lead to substantial numbers of "missing" values in merged datasets and possibly affect the reliability of inferences drawn from statistical analysis. This is of particular concern in empirical analysis in conflict and peace economics as inconsistent country coding often affects countries in conflict. Second, by way of illustration, we provide overview tables of some of the gravest discrepancies in country coding across datasets.

A typology of inconsistencies

Inconsistent country names are the tip of the merging iceberg. Not only do names differ, but so does for example the period of existence for some countries. And worse, the documentation on the country coding schemes provided by the data projects is often sparse and contains errors.[4]

The following three types of inconsistencies between country units in different data sources and coding schemes are frequently observed and examined in this article.

Inconsistency type 1: *a state name exists in one dataset but not in the other*. There are several reasons for this, shown here in schematic fashion:

Reason i: Different years (time series do not match and some states do not exist anymore/yet).
Example: When merging PolityIV with Comtrade data the Orange Free State cannot be merged as it ceases to exist before coding of Comtrade data starts.
Result: Country is unmergeable and drops out of analysis because it does not exist in one dataset.

Reason ii: Different definition of statehood.
Example: Some datasets do not code Palestine as they do not consider it to meet formal requirements of statehood.
Result: Country is unmergeable and drops out of analysis because it does not exist in one dataset.

Reason iii: Different state names (labels) or entities/territories (see the third inconsistency described below).
Example: Yugoslavia and its successors are coded in vastly different ways in terms of names and years across datasets. How should these countries or observations be aggregated to make them comparable across datasets and to not loose conflict observations?
Result: Country may drop out of analysis if no action is taken.

Inconsistency type 2: *a country is coded under the same name, but for different years in two datasets (time series for given country are not identical in both datasets)*. Again, in schematic fashion:

Reason i: Missing observations within time series.
Example: In V-Dem, Germany, 1945–1948, is not coded since the institutional framework of Germany during those years does not meet the formal criteria for the definition of their democracy indices.
Solution: Depends on application and on underlying assumptions made about reason for missingness, possibly interpolation.

Reason ii: Country starts or ceases to exist and first/last year is not coded consistently across datasets.
Example: PolityIV codes the former East Germany between 1945–1990, whereas V-Dem codes it from 1949–1990.
Solution: Depends on application, possibly extrapolation.

Inconsistency type 3: *a country is coded under different names either (a) for the same years in two datasets or (b) for different years in two datasets*.

Reason i: It is clearly the same state, only the label is different. This is often the case for 3(a), or for 3(b) in combination with inconsistency type 2, reason ii.
Example: "St." versus "Saint" or official versus colloquial state names ("Plurinational State of Bolivia" and "Bolivia").
Solution: Use Stata and R packages for renaming.

Reason ii: The different names might refer to different underlying entities/ territories.
Example: We provide detailed overviews of these cases

in Table A3 (Democracy Datasets) and Table A6 (Economic Datasets) of the Appendix.
Solution: The 3(b) case is by far the most difficult case as the years coded do not provide additional evidence on the actual entity captured. The question of how these entities could be compared in a meaningful way across datasets has no straightforward answer; rather, the answer is case dependent.

Inconsistent country coding of types 1 to 3 lead to missing values in the final, merged dataset.[5] In this article we show that the extent of these "missing values" (they are not really missing, just missing due to inconsistencies) is vast and of particular relevance to empirical research in conflict and peace economics. Most country coding schemes differ in the naming and dating of a specific set of countries: Countries which have experienced armed conflict are less democratic and less trade open than the consistently coded ones. As a result, a merged dataset can contain a comparatively high share of missing values for this set of countries. Thus, it can no longer be considered a random sample. To minimize "missings," and to avoid losing valuable information, the process of creating large panel datasets should therefore be done with utmost care.

In general, there are three approaches to code countries in macro panel data: By (string) country names, by numeric code, or by alphabetic code. The most common schemes include (but are not limited to) the COW country list, the Gleditsch/Ward state list, and the ISO 3166 list of country codes.[6] In theory, numeric and alphabetic codes should facilitate the merging process. Unfortunately, several numeric and alphabetic codes schemes exist and often they are neither implemented consistently nor are the country codes easily translatable to each other. In R the package "countrycode" and in Stata the package "kountry" help with these issues.[7] These packages map country names and codes from one kind of macro country codes to another. They come with a slight disadvantage, though, as "[t]he mapping between the available dataset_names [types of country coding schemes] is not always perfect."[8] This is especially dire when using a comparatively new dataset such as V-Dem which does not follow any of the coded country schemes exactly. In addition, this assumes that each source dataset correctly applies the country coding scheme it is based on. In the following sections we show that this is not the case for several datasets. By letting Stata or R packages adjust the country names, the renaming—and subsequently the merging process—is put into a black box, inherently making it more vulnerable to mistakes.

We aim to take this data merging process out of its black box and use actual country names to prevent merging mistakes. In what follows we provide a detailed comparison of six datasets covering the indicators trade, democracy, and conflict. For each dataset a table with actual country names and years in the data is provided (see Boese and Kamin, 2018a, 2018b). These tables present an overview of the gravest discrepancies in country coding and allow for quick cross-dataset comparisons of country units. In addition, this article gives an overview of the extent of the country coding problem by comparing structural properties of the set of inconsistently coded countries to those of the uniformly coded ones and by discussing missing data as well as differences in annual coding.

On the one hand, this article provides assistance to scholars merging several source datasets. On the other, it highlights naming inconsistencies between data documentation, such as code books, and actual observations in the data. Such inconsistencies potentially lead to merging problems when blindly using the Stata or R packages (and the country coding scheme specified in the documentation) discussed above. We have the highest respect for all the data projects discussed in this article. We therefore hope that the lists of these inconsistencies are also of assistance to the data projects in aligning their documentation to their respective datasets.

The following three sections respectively provide thorough comparisons of two democracy, two trade, and two conflict datasets, including detailed tables comparing the country coding units. The article closes with a discussion of the results.

Democracy data

This section compares the country coding units of two democracy datasets: V-Dem version 8 and the PolityIV dataset 2016. The tables referenced in this section can be found in the Appendix as well as in Boese and Kamin (2018a).

We first discuss the countries listed in V-Dem version 8, then discuss the countries in the PolityIV dataset 2016, and then compare characteristics of the observations listed in both datasets with those listed in only one of the datasets.

V-Dem Data version 8

The V-Dem dataset used for this article is V-Dem data version 8, in country year format. The variable of interest is the Electoral Democracy Index, v2x_polyarchy. V-Dem identifies the countries either by name, alphabetical country id, or numerical country id.[9] These country identifiers do not correspond to any of the prevailing country schemes implemented in the Stata or R packages mentioned above. To facilitate the merging process, we therefore provide a detailed list of county coding units in the data[10] and compare it to the country list in the V-Dem code book (Coppedge, *et al*., 2018a).

V-Dem excels in terms of transparency and provides a

supplementary article on "V-Dem Country Coding Units v8" which lists and discusses all polities and countries and the respective years for which they are coded as well as a detailed explanation of the country borders used in the coding.[11] It also provides detailed information on years in which a country is not coded (with the variables gapstart and gapend). However, there are several observations for which v2x_polyarchy is missing. Worksheet "Overview" in Boese and Kamin (2018a) shows the number of years for which each country is coded in V-Dem version 8, as well as its gaps (by coding decision) and its additional missing values.

For ten countries the names in dataset and documentation do not match.[12] These name mismatches are by no means a purely alphabetical problem. Take, for example, Vietnam. While there is no country named Vietnam, North or South, in the V-Dem dataset there is a "Republic of Vietnam" (coded from 1802–1975) and a "Democratic Republic of Vietnam" (coded from 1945–2017). The V-Dem Country Coding Units document, however, provides a detailed overview of the polities forming part of:

> "Vietnam, South (35)
> Coded: 1802–1975. History: (...) Republic of Vietnam (also known as South Vietnam) (1955–1975)" and
> "Vietnam, North (34)
> Coded: 1945– History: Democratic Republic of Vietnam (i.e. North Vietnam) [declared] (1945); Democratic Republic of Vietnam (1945–1949); Democratic Republic of Vietnam [independent state] (1949–). Note: From 1976, the polity also includes areas formerly belonging to Republic of Vietnam (South Vietnam)."[13]

Take another example. In the documentation the numerical country id (365) is coded for two countries: Oldenburg, 1789–1867, and Saxe-Weimar-Eisenach, 1809–1867. In the dataset, however, only Saxe-Weimar-Eisenach is assigned country_id 365 while Oldenburg is assigned code 364.

Table 1: Number of (un)mergeable countries in a merge of the PolityIV dataset wih the COW country list

Merging by	*Country name*	*Numeric code*	*Alphabetic code*
Unmergeable no. of countries in PolityIV	26	11	19
Mergeable no. of countries in PolityIV and COW	169	183	177

Table 2: Description of democracy datasets

Dataset	*A: V-Dem*	*B: PolityIV*
Total no. of obs	26,537	17,228
Total no. of nonmissing obs	24,115	16,992
No. of countries	201	195
Years covered	1789–2017	1800–2016

Table 3: Merging V-Dem and PolityIV data

Merging observations	*A: V-Dem*	*B: PolityIV*
Unmergeable only in A	10,929	n/a
Unmergeable only in B	n/a	1,619
Mergeable in both	15,609	
Nonmissing only in A	9,380	n/a
Nonmissing only in B	n/a	1,571
Nonmissing, mergeable in both	14,376	15,421

Table 4: Two sample *t*-tests of average level of democracy

Dataset	*A: V-Dem*	*B: PolityIV*
Unmergeable group	0.1377	–1.5493
Mergeable group	0.3428	–0.4495
Difference	0.2051***	1.0998***

Note: *** Statistically significant at the 1% level.

PolityIV

A second dataset, capturing political authority patterns worldwide and over long periods of time, is the PolityIV project's dataset on "Political Regime Characteristics and Transitions, 1800–2016" (for short, the PolityIV dataset).[14] In the dataset countries are identified by their name, an alphabetic country code, or a numeric code.[15] These identifiers supposedly follow the COW country coding scheme.[16] Table 1 displays the results from merging the PolityIV data with the COW country list, finding that 13 percent of the countries are unmergeable when merging by country name, 6 percent when merging by numeric code, and 10 percent when merging by alphabetic code.[17] The unmergeable groups largely consist of countries of particular interest in conflict and peace economics such as the Koreas, Congos, Germanies, and Serbias. As a consequence, when merging the PolityIV data using a software package taking the dataset to be in "COW coding scheme" these countries may not be properly dealt with. It is worth noting that

country names and alphabetic and numeric codes are not coded consistently over time within the PolityIV dataset, i.e., there are 195 different country names, but only 194 different alphabetic and numeric codes. This is not due to a single country having different names and only one code, but to a number of countries and several code/label constellations. Examples include Yugoslavia (either *ccode* 345 and *scode* YUG or *ccode* 347 and *scode* YGS; the fact that 347 and YGS also are used for Serbia and Montenegro in the dataset further complicates matters), Ethiopia (either *ccode* 529 and *scode* ETI or *ccode* 530 and *scode* ETH), Pakistan (either *ccode* 769 and *scode* PKS or *ccode* 770 and *scode* PAK). Further, *ccode* 860 and *scode* ETM is used for East Timor and Timor Leste, and *ccode* 255 and *scode* GMY is used for Germany and Prussia.

Additionally, in the PolityIV dataset we note duplicate observations for Yugoslavia in 1991 and for Ethiopia in 1993. This further complicates the merging process as the scholar is forced to decide how to proceed with these duplicates.

Comparison of the democracy data

Table 2 describes both democracy datasets. The variable of interest in each dataset is a democracy index: *v2x_polyarchy* for the V-Dem data and *polity2* for the PolityIV data.[18] The total number of nonmissing observations refers to the number of observations for which the respective variable of interest contains nonmissing values.

When merging the datasets by country name and year, observations of inconsistency types 1 to 3 cannot be merged. Table 3 shows the number of mergeable and unmergeable observations by source dataset. As discussed, even though an observation might be listed, the variable of interest can contain a missing value. Hence the lower half of Table 3 proves the same information for all observations with nonmissing values. To make the number of observations comparable across datasets in Table 3, only observations from the time period covered by both datasets are considered (that is, V-Dem observations before 1800 as well as the year 2017 were left out to match the PolityIV time series). Around 41 percent of the V-Dem and around 9 percent of the PolityIV observations cannot be merged. To assess whether the unmergeable observations are systematically different from the mergeable ones we calculated the average levels of democracy for each group. Table 4 shows the results of two *t*-tests, one for V-Dem, one for PolityIV. In both datasets, the unmergeable group had a significantly lower average level of democracy. (To be clear, the *t*-tests were carried out only on the nonmissing observations noted in Table 3.)

Economic data

UN Comtrade and the World Bank's World Development Indicators (WDI) contain economic data. We first discuss the countries listed in the UN Comtrade data, then those in the WDI, and then compare the country coding schemes of both datasets. The tables and worksheets referenced to in this section can be found in the Appendix as well as in Boese and Kamin (2018b).

UN Comtrade

The indicator taken from UN Comtrade is total exports in current U.S. dollars from each country to the rest of the world. The Comtrade dataset is an unbalanced panel as it only contains years for which countries have reported trade. Hence, time series differ from country to country. The first year for which some countries reported trade is 1962, the last year is 2017 (few observations are available for the start and end years of the time series). Comtrade offers data coded according to two different systems for international trade statistics: The Harmonized System (HS), introduced in 1988, and the Standard International Trade Classification (SITC), introduced in 1962, with the latter being less detailed than the former. To obtain the longest possible time series, we concatenated SITC classification export data, 1962–1987, with HS classification export data, 1988–2017.

In addition to gaps in the time series caused by missing observations (as discussed above) the export variable contains missing values for several observations. Missing information primarily indicates that trade was not reported and is not to be equated with zero trade flows.[19] This is crucial concerning the tackling of zero trade flows and appropriate model choice.[20]

The country name abbreviations of the official UN country list[21] correspond to the country names used in the Comtrade data with the exception of Côte d'Ivoire and Réunion, which contain spelling errors in the downloaded Comtrade dataset ("C√¥te d'Ivoire" and "R√©union").

World Development Indicators

The economic indicator taken from the World Bank's WDI is trade openness, defined as the percentage share of trade of each country's GDP, that is, (imports+exports)/GDP. Starting in 1960, the time series runs to 2016. The distinction between zero trade and missing data in the WDI is equivalent to the one in UN Comtrade. In contrast to Comtrade, however, the WDI data is a balanced panel with one observation for each country and year. Nevertheless, trade openness contains missing values for several observations due to missing information on GDP, exports, or imports. In addition to countries, WDI provides aggregated information on country groups (such as "Europe &

Central Asia" or "Low & Middle Income"). These where taken out of the list to facilitate reading (the full list of country groups removed is available in Boese and Kamin, 2018b, worksheet "Disregarded Country Groups").

To our knowledge, the World Bank does not provide an explicit country coding scheme upon which WDI data are based. However, the World Bank does provide a list of countries upon which the World Integrated Trade Solution (WITS) data are based.[22] It is unclear whether this list also forms the basis of the WDI dataset. Of 15,048 observations in the WDI dataset used in this article, 30 percent (4,560 observations) do not match the WITS list. Several of them are due to naming inconsistencies such as, for example, "Bahamas, The" versus "Bahamas".

Comparing the economic data

In a comparison of the economic datasets[23] the sheer number of naming inconsistencies[24] and single appearances of countries (that is, they appear in one, but not in the other dataset)[25] stands out. Additional cases, difficult to handle when merging datasets, are countries that started and ceased to exist, yielding different country names for different or the same territories and for different years (inconsistency type 3). While WDI refers to each country under one name continuously for the entire time series, this is not the case for the UN Comtrade data. In Comtrade, countries are coded by different names and years. Table A6[26] displays the cases where this kind of inconsistency is in place. The table shows that Comtrade distinguishes the underlying country entities in much more detail. There is, for example, only one "Germany" in the WDI data as opposed to "Germany", "Fmr Fed. Rep. of Germany" and "Fmr Dem. Rep. of Germany" in the UN Comtrade data.

Assuming that the ending of one state and the beginning of a new one are coded in detail through the year variable by WDI, can the country coding units be supposed to be the same across the two datasets? The sparsity of country coding unit documentation renders it impossible to answer this question. There is no information on whether territories changed, and on whether or how much this change was incorporated in the coding. This becomes a severe drawback to the data when complementary variables for the analysis of trade flows, such as country size, GDP, measures of distance and—most importantly—borders are taken into account.[27]

The case of Sudan (see Table A6)[28] illustrates the problem: WDI codes "South Sudan" and "Sudan". For the latter, the measure of trade openness is available for the whole time series (1960–2016). For "South Sudan", the indicator is available from 2008–2015. UN Comtrade codes "Sudan" (2012–2015) and "Former Sudan" (1963–2011, with gaps).

Table 5: Description of trade datasets

Dataset	*A: Comtrade*	*B: WDI*
Total no. of obs	12,768	15,048
Total no. of nonmissing obs*	6,790	10,643
No. of countries	228	264
Years covered	1962–2017	1960–2016

Note: *The total number of nonmissing observations refers to the number of observations for which the respective variable of interest contains nonmissing values.

Table 6: Merging Comtrade and WDI data

Merging observations	*A: Comtrade*	*B: WDI*
Unmergeable only in A*	3,803	n/a
Unmergeable only in B	n/a	6,083
Mergeable in both	8,965	
Nonmissing only in A	1,449	n/a
Nonmissing only in B	n/a	3,765
Nonmissing, mergeable in both	5,341	6,878

Note: *When merging both datasets by country name and year those observations of inconsistencies types 1 to 3 are unmergeable.

Table 7: Two sample *t*-tests of average level of trade and trade openness

Dataset	*A: Comtrade**	*B: WDI***
Unmergeable group	2.72×10^{14}	66.16
Mergeable group	3.98×10^{13}	76.14
Difference	-2.32×10^{14}***	9.98***

Note: *The trade variable in Comtrade is total exports (*TradeValueUS*), range: USD37,310–2.34×10^{16}. **The trade variable in WDI is trade openness (*tradeop*), range: 0–860.8 (in %). *** Statistically significant at the 1% level. The *t*-tests were carried out on the nonmissing observations in Table 5.

Hence, WDI takes 2008 as the year of birth for "South Sudan", while Comtrade (implicitly, because it does not code "South Sudan" as a country)[29] codes a new state "Sudan" from 2012 onward. Similar cases are Serbia (with or without data for Kosovo or Montenegro) and China (with or without data for Hong Kong, Macao, and Taiwan).[30]

The country name by itself does not allow for an exact indication of the territory coded. In a statistical analysis only of

trade, it might not matter whether Sudan or South Sudan is included. In conflict and peace economics, however, where relationships among conflict, politics, and economics are of high interest, such lack of accuracy effectively becomes an impediment to an appropriate econometric analysis.

Table 5 describes both trade datasets. For Comtrade, the variable of interest is total exports in current U.S. dollars (*TradeValueUS*); for the WDI data, it is trade openness as a percentage of GDP (*tradeop*). Table 6 shows the number of mergeable and unmergeable observations by source dataset. As discussed, even though an observation might be listed the variable of interest can contain a missing value. Hence the bottom half of Table 6 provides the same information for all observations with nonmissing values. To make the number of observations comparable across datasets in Table 6 only observations from the time period covered by both datasets are considered, i.e., 1962–2016. About 30 percent of the Comtrade observations, and about 40 percent of the WDI observations, cannot be merged.[31] To assess whether the unmergeable observations are systematically different from the mergeable ones, we calculated average levels of total exports and trade openness for each group. Table 7 shows the results of two sample *t*-tests: For Comtrade, the average export level is statistically significantly *higher* (given the exponent) in the unmergeable than in the mergeable group. For WDI, the unmergeable country group had a significantly *lower* level of average trade openness. Looking at the naming inconsistencies (Table A4) confirms this "higher-lower" difference: The high levels of export values in the unmergeable group in Table 7 are driven by observations from the U.S., Germany, Macao, and Hong Kong.[32] Table 7 hence provides a good intuition to the effects of inconsistent country coding: Either the cases of high export levels or of low trade openness are lost due to merging problems. Either one is problematic in terms of statistics and, depending on the analytic aim, might lead to biased estimates.

Conflict data

In theory, the datasets for economic and political variables code each variable for all years during which a country exists. The conflict datasets, however, are fundamentally different: By design, they only code conflict variables for years in which a conflict occurred in a given country and which surpassed some conflict criteria (for example, 25 battle-related deaths). Consequently, time series and cross-section data dimensions contain gaps for country-years without armed conflict.

The UCDP Armed Conflict dataset version 18.1 (Pettersson and Eck, 2018; also see Gleditsch, *et al*., 2002; UCDP, 2018) studies armed conflict above a yearly threshold of 25 battle-related deaths. The Militarized Interstate Disputes (MID) B dataset version 4.2 (Palmer, *et al*., 2015) captures militarized interstate disputes which can involve, for example, a display of force without incurring any battle deaths. Therefore, the gaps in the datasets will be very different, and merging them by country and years coded does not provide insights on, or a comparison of, country coding units. Nevertheless, both datasets acknowledge the importance of defining country coding units. In the remainder of this section, we show that even within each of these datasets there are inconsistencies between the country coding units as defined by the respective data project and the actual observations in the data. As a result, these observations are either dropped, potentially falsely matched, or have to be manually adjusted when using Stata or R commands for merging countries.

UCDP/PRIO Armed Conflict dataset version 18.1

The UCDP/PRIO Armed Conflict dataset acknowledges the importance of a precise description of country coding units[33] and dedicates an entire section of its code book[34] to the exact definition of country coding units. It includes a country table with numerical and alphabetical country codes, state names, and start and end years for the countries that form part of the international system of states.

Table A7 lists the countries coded in the actual data and compares them to the system membership table from the UCDP/PRIO code book. The system membership table must include more observations since, by definition, it also includes countries without armed conflict. But Table A7 shows that even when restricted to countries with armed conflict there are inconsistencies in the country names (for example "Burkina Faso" and "Burkina Faso (Upper Volta)", "DR Congo (Zaire)" and "Congo, Democratic Republic of (Zaire)", and "Ivory Coast" and "Cote D'Ivoire").

MID B version 4.2

The MID B version 4.2 dataset includes one observation per participant to a militarized dispute, 1816 –2010, with countries taken from the Correlates of War (COW) list. The MID B dataset itself does not contain (string) state names. Instead, countries are coded with a three-digit numerical code (*ccode*) and with an alphabetical code (*stabb*). Before joining variables from the MID B dataset with any other macro panel data, such as WDI, a first step therefore is to merge MID B with COW, but four countries cannot in fact be merged (Table 8). The three-digit alphabetic codes for these countries are RUM, USR, VTM, and ZAI. This is a perfect example of the difficulties associated with merging by country as it is hardly possible to determine with certainty which underlying entity (territory) is exactly covered, for example, by USR or VTM. This also

illustrates why, for this article, we chose to employ merging by country (string) names, not codes. VTM could stand for (Democratic) Republic of Vietnam, Vietnam North, Vietnam South, or Vietnam. While the exact entity coded remains unclear, it is very clear that this case contains information relevant for studies of conflict.

That the MID B dataset states that it follows the COW country list convention when in fact it does not, makes it effectively impossible to determine for some observations which actual underlying entity is considered a country during which period of time.

Table 8: Number of unmergeable countries in a merge of the MID B dataset with the COW country list

Merging by	*Numeric code*	*Alphabetic code*
Unmergeable countries in MID B	4	4
Mergeable countries in MID B and COW	191	191

Discussion and conclusion

Large-scale cross-country datasets are frequently merged in quantitative studies in conflict and peace economics. We find that the coding of country units overlaps across datasets only for a relatively small proportion of countries. Discrepancies in country naming or other forms of country identification such as numerical or alphabetical country IDs are frequent among countries splitting up or (re)uniting during the time period studied. Examples include Yugoslavia, Germany, Vietnam, and Sudan. If the names are not adjusted, these inconsistencies render such observations unmergeable and, when joining variables from several data sources, ultimately result in missing values. When these missing values then are dropped from an analysis, important information is lost. This loss of information is of particular severity in conflict and peace economics as countries which split up or reunite often do so accompanied by armed conflict and thus contain valuable information.

The dataset comparisons made in this article demonstrate that inconsistencies in country coding across macro panel datasets remain a relevant challenge in cross-national studies. They show that for economic datasets as well as democracy datasets the unmergeable group is of a large size (up to about 40 percent of all observations) and significantly differs from the group of mergeable observations. In particular, the group of unmergeable countries is on average less democratic than the mergeable group. Depending on the economic measure analyzed (and, with it, the country naming scheme applied), a group of countries with high exports or another group of countries with low trade openness cannot be merged.

These discrepancies can be attributed, in part, to differences in country labels. Several projects, such as Hensel (2016) and the aforementioned software codes and packages can help adjust them. However, another part of the inconsistent country coding is due to different perceptions and definitions of the unit of analysis. The exercises carried out for this article show that the actual entity captured can differ by source dataset. While this makes creating merged panel datasets consisting of economic, political, or armed conflict factors challenging in its own right, proper merging might be a necessary condition for analysis. For an armed conflict dataset, relevant state units might differ significantly from datasets on democracy or trade flows (the coding of Palestine, Hong Kong, or Macao are examples). As a result, the burden of discussing the unit of analysis studied and of ensuring that countries correspond to the same entity across merged datasets, lies with the individual scholar or team. This article encourages scholars to discuss the merging process in their academic papers (or supplementary materials) and to not take the problem of inconsistent country names lightly. This is particularly the case in conflict and peace economics, where relevant information is systematically lost when unmergeable observations are discarded.

Furthermore, it is worth noting that country names are not the only dimension of macro panels to be carefully compared across datasets before merging. It goes well beyond the scope of this article to additionally compare the actual time periods covered. However, we point out that the time dimension underlying the calendar year coding of macro panels does not necessarily coincide with the actual calendar year. To quote from the World Bank: "In most economies the fiscal year is concurrent with the calendar year ... Most economies report their national accounts and balance of payments data using calendar years, but some use fiscal years." Time inconsistencies, then, are another potential source of erroneous inference, in particular when studying the effect of conflict on the economy or the political system, or vice versa.[35]

Last, but not least, we pay tribute to the creators of the datasets discussed in this article: Assembling and maintaining these datasets is a Herculean task. The challenges associated with inconsistent country names and units across datasets can, however, lead to serious consequences in conflict and peace economics. Unfortunately, while an easy solution to the noted problems is not likely to exist, given the different purposes each of the source datasets is created for, we hope that our comments here increase broader awareness and discussion of these problems and that our tables in the Appendix (and online)

facilitate quick cross-dataset comparisons of country coding.

Notes

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1. Examples of studies using such merged datasets include Hegre, *et al*. (2001), Blomberg and Hess (2006), Gates, *et al*. (2006), Martin, Mayer, and Thoenig (2008), Glick and Taylor (2010), Acemoglu, *et al*. (2019), Dunne and Tian (2015), and d'Agostino, Dunne, and Pieroni (2018).

2. Hence the title of this article. 'Tis but they name that is my enemy (Romeo and Juliet, Act II, Scene ii, Shakespeare, 2003).

3. See http://heatherba.web.unc.edu/data-code/.

4. For discussion, see the sections on democracy, economic, and conflict data in this article.

5. Note the difference between *missing values* and *missing observations*. For example, on the one hand, in the V-Dem dataset version 8 there are no observations for Germany between 1945 and 1948, leaving the panel unbalanced. In the World Development Indicators, on the other hand, the panel provided is balanced, that is, there is one observation for each country in each year. However, for a number of years the variable of interest contains a missing value. Ultimately, when merging two such sources and using the final dataset for statistical analysis, missing values and missing observations come down to the same thing: *missing information*. For most regressions or other analyses, software like Stata disregards observations whenever they contain missing values.

6. COW: A country coding scheme employed by several of the macro panel datasets studied in this article. Data can be obtained from http://www.correlatesofwar.org/data-sets/cow-country-codes. There are three variables: numeric and alphabetic country codes and statename. The dataset covers 217 countries. The country list includes 26 duplicate observations. Gleditsch/Ward: The Gleditsch and Ward (1999) state list builds on and revises the COW country list. First published in 1999, a current version is available at http://ksgleditsch.com/statelist.html. ISO: See https://www.iso.org/iso-3166-country-codes.html.

7. R: See https://cran.r-project.org/web/packages/countrycode/countrycode.pdf. Stata: See Raciborski (2008).

8. Quote: Raciborski (2008, p. 392). Raciborski (2008) continues with a short overview of the most striking inconsistencies.

9. Alphabetical country_text_id: "Abbreviated country names," V-Dem Codebook v8, p. 36. Numerical country_id: "Unique country ID designated for each country. A list of countries and their corresponding IDs used in the V-Dem dataset can be found in the country table in the codebook, as well as in the V-Dem Country Coding Units document." V-Dem Codebook v8, p. 36. The *codebook* itself is Coppedge, *et al*. (2018a). The *country coding units document* is Coppedge, *et al*. (2018b).

10. See Boese and Kamin (2018a), worksheet "V-Dem Codebook vs. Data".

11. See Coppedge, *et al*. (2018b).

12. These are: Democratic Republic of Congo, Democratic Republic of Vietnam, German Democratic Republic, Mecklenburg Schwerin, North Korea, Republic of Vietnam, Republic of the Congo, South Korea, São Tomé and Príncipe, and Timor-Leste.

13. Coppedge, *et al*. (2018b, p. 27).

14. See Marshall, Gurr, and Jaggers (2017b).

15. Alphabetic: The variable *scode* ("Alpha Country Code: Each country in the Polity IV dataset is defined by a three-letter alpha code, derived from the Correlates of War's listing of members of the interstate system" (Marshall, Gurr, and Jaggers, 2017a, p. 12). Numeric: *ccode* (numerical, "Numeric Country Code: Each country in the Polity IV dataset is defined by a three-digit numeric code, derived from the Correlates of War's listing of members of the interstate system" (Marshall, Gurr, and Jaggers, 2017a, p. 11).

16. Supposedly: See Marshall, Gurr, and Jaggers (2017a, p. 11).

17. To be clear, the share of unmergeable countries is calculated as: number of unmergeable countries/ total number of countries in PolityIV (i.e., 26/195~13.3%, 11/194~5.7%, and 19/194~9.8%. Note that the rows are labeled correctly although one could in fact omit "and COW" from the second row since, if countries are mergeable in a merge between COW and PolityIV, they must exist in both datasets. In the first row, however, are unmergeable countries only, i.e., those which exist only in the PolityIV dataset.

18. V-Dem's *v2x_polyarchy*: Range 0 to 1 (most democratic). PolityIV's *polity2*: Range –10 to +10 (most democratic).

19. For a discussion of missings in trade data see, for example, Keshk, Reuveny, and Pollins (2010, Section 3.3, p. 10), Barbieri, Keshk, and Pollins (2009, p. 476), and Boehmer, Jungblut, and Stoll (2011).

20. See, for example, Santos Silva and Tenreyro (2006).

21. The UN provides a list of country codes and names at https://unstats.un.org/unsd/tradekb/Knowledgebase/50377/Comtrade-Country-Code-and-Name.

22. https://wits.worldbank.org/wits/wits/witshelp/content/codes/country_codes.htm.

23. See Boese and Kamin (2018b), worksheet "Overview".

24. See Table A4 or Boese and Kamin (2018b), worksheet "naming inconsistencies" for inconsistency type 3, reason i (one country coded with different names but for the same year and years).

25. See Table A5 or Boese and Kamin (2018b), worksheet "existence asymmetry" for inconsistency types 1 and 3.

26. Also see Boese and Kamin (2018b), worksheet "inconsistency type 3".

27. Anderson and van Wincoop (2003), for example, demonstrated that national borders are a highly important impediment to trade.

28. Boese and Kamin (2018b), worksheet "inconsistency 2.0", rows 36–38.

29. The fact that no "South Sudan" is included in the UN Comtrade data is itself somewhat astonishing since trade data is available (otherwise WDI would not be able to code it).

30. See World Bank (2017a, p. XVII).

31. Again, to be clear: 3,803/(3,803+8,965)~29.7% and 6,083/(6,083+8,965)~40.4%.

32. This is shown in Boese and Kamin (2018b), worksheet "Unmergeable Outliers Comtrade". It contains all unmergeable Comtrade observations sorted by export values (highest first) to show the outliers driving the results.

33. "The definition of a state is crucial to the UCDP/PRIO conflict list" (UCDP/PRIO Armed Conflict Dataset Codebook, 2018, p.13).

34. See Section 4: "System Membership Description" (UCDP/PRIO Armed Conflict Dataset Codebook, 2018, p. 13).

35. Quote from World Bank (2017b, p. 117).

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Appendix Tables A1, A2, and A3

Democracy datasets comparison

See Boese and Kamin (2018a) for a very detailed listing of all countries and their respective time series covered. Countries for which only the names/labels differ are listed in Table A1 (that is, countries of inconsistency type 3, reason i.) In the worksheet "Overview" (Boese and Kamin, 2018a), these countries are highlighted in grey.

Countries for which the underlying entity has no perfect match in the other dataset are listed in Table A2. A "perfect match" refers to a counterpart in terms of names and years (and potentially borders). This includes countries of inconsistency types 1 and 3. Countries representing the same or similar historical units are grouped.

Countries unmergable due to name and time inconsistencies are listed in Table A3. This includes countries of inconsistency type 3. Note: # obs=number of observations; N=total number of available observations in data; missing=number of missing years/observations for given country between its first and last year.

Table A1: Countries for which only the names/labels differ (democracy datasets)

V-Dem Version 8	*Polity IV, Version 2016*
Bosnia and Herzegovina	Bosnia
Burma/Myanmar	Myanmar (Burma)
Democratic Republic of Congo	Congo Kinshasa
German Democratic Republic	Germany East
North Korea	Korea North
Piedmont-Sardinia	Sardinia
Republic of Vietnam	Vietnam South
Republic of the Congo	Congo Brazzaville
Slovakia	Slovak Republic
South Korea	Korea South
South Yemen	Yemen South
United Arab Emirates	UAE
United States of America	United States
Würtemberg	Wuerttemburg

Table A2: Countries for which the underlying entity has no perfect match in the other dataset (democracy datasets)

V-Dem Version 8	*Polity IV, Version 2016*
Barbados	
	Yugoslavia
Brunswick	
Colombia	Colombia Gran Colombia
Czech Republic	Czech Republic Czechoslovakia
Democratic Republic of Vietnam	Vietnam North Vietnam
German Democratic Republic	Germany East
Germany	Germany Prussia Germany West
Guatemala	United Province of CA (Central America)
Hamburg	
Hanover	
Hesse-Darmstadt	
Hesse-Kassel	
Hong Kong	
Iceland	
Ivory Coast	Ivory Coast Cote D'Ivoire
Maldives	
Mecklenburg Schwerin	
Nassau	
Oldenburg	
	Orange Free State
Palestine/British Mandate	
Palestine/Gaza	
Palestine/West Bank	
Russia	USSR
Saxe-Weimar-Eisenach	
Serbia	Serbia Serbia and Montenegro
Seychelles	
Somaliland	
South Korea	Korea South Korea
South Sudan	South Sudan
Sudan	Sudan Sudan-North
São Tomé and Príncipe	
Timor-Leste	Timor Leste East Timor
Vanuatu	
Yemen	Yemen Yemen North
Zanzibar	

Table A3: Countries unmergeable due to name and time inconsistencies (democracy datasets)

V-Dem Version 8, 201 countries

Country	First Year	Last Year	# obs in data N	# obs in data Missing
Bosnia and Herzegovina	1992	2017	26	0
Colombia	1789	2017	229	0
Czech Republic	1918	2017	100	0
Democratic Republic of Vietnam	1945	2017	73	0
Germany	1789	2017	225	4
Ivory Coast	1900	2017	118	0
Russia	1789	2017	229	0
Serbia	1804	2017	213	1
South Korea	1789	2017	229	0
Sudan	1900	2017	118	0
South Yemen	1900	1990	91	0
Yemen	1789	2017	162	67
Timor-Leste	1900	2017	118	0

Polity IV, Version 2016, 195 countries

Country	First Year	Last Year	# obs in data N	# obs in data Missing
Bosnia	1992	2016	25	0
Yugoslavia	1921	2002	83	-1
Colombia	1832	2016	185	0
Gran Colombia	1821	1832	12	0
Czech Republic	1993	2016	24	0
Czechoslovakia	1918	1992	75	0
Vietnam North	1954	1976	23	0
Vietnam	1976	2016	41	0
Germany	1868	2016	105	44
Prussia	1800	1867	68	0
Germany West	1945	1990	46	0
Ivory Coast	1960	2015	56	0
Cote D'Ivoire	2016	2016	1	0
Russia	1800	2016	148	69
USSR	1922	1991	70	0
Serbia	1830	2016	102	85
Serbia and Montenegro	2003	2006	4	0
Korea South	1948	2016	69	0
Korea	1800	1910	111	0
Sudan	1956	2011	56	0
Sudan-North	2011	2016	6	0
Yemen South	1967	1990	24	0
Yemen	1990	2016	27	0
Yemen North	1918	1990	73	0
Timor Leste	2016	2016	1	0
East Timor	2002	2015	14	0

Appendix Tables A4, A5, and A6

Economic datasets comparison

Table A4 is a listing of unmergeable names/labels in the UN Comtrade and WDI datasets, due to inconsistency type 3, and shows a large share of countries with high export levels (Boese and Kamin, 2018b, contains the list sorted by total exports; worksheet "Unmergable Outliers Comtrade". The spreadsheet also provides a list of country groups/regions which were not included in the comparison; worksheet "Disregarded Country Groups").

Table A5 shows countries for which the underlying entity has no perfect match in the other dataset. A "perfect match" refers to a counterpart in terms of names and years (and potentially borders). This includes countries of inconsistency types 1 and 3. Countries representing the same or similar historical units are grouped.

Table A6 show countries unmergable due to name and time inconsistencies. This includes countries of inconsistency type 3 (N=total number of available observations in data).

Table A4: Countries for which the names/labels differ (economic datasets)

UN Comtrade exports	*WDI trade openness*
Bolivia (Plurinational State of)	Bolivia
Bosnia Herzegovina	Bosnia and Herzegovina
Cabo Verde	Cape Verde
Cayman Isds	Cayman Islands
Central African Rep.	Central African Republic
China, Hong Kong SAR	Hong Kong
China, Macao SAR	Macao SAR, China
Congo	Republic of the Congo
Czechia	Czech Republic
Côte d'Ivoire	Ivory Coast
Dem. Rep. of the Congo	Democratic Republic of Congo
Dominican Rep.	Dominican Republic
FS Micronesia	Micronesia, Fed. Sts.
Faeroe Isds	Faroe Islands
Gambia	The Gambia
Lao People's Dem. Rep.	Laos
Myanmar	Burma/Myanmar
Rep. of Korea	South Korea
Rep. of Moldova	Moldova
Russian Federation	Russia
Saint Kitts and Nevis	St. Kitts and Nevis
Saint Lucia	St. Lucia
Saint Vincent and the Grenadines	St. Vincent and the Grenadines
Sao Tome and Principe	São Tomé and Príncipe
Solomon Isds	Solomon Islands
TFYR of Macedonia	Macedonia
Turks and Caicos Isds	Turks and Caicos Islands
US Virgin Isds	Virgin Islands (U.S.)
USA	United States of America
United Rep. of Tanzania	Tanzania
Viet Nam	Vietnam
Yemen	Yemen, Rep.

Table A5: Countries for which the underlying entity has no perfect match in the other dataset (economic datasets)

UN Comtrade exports	*WDI trade openess*
	American Samoa
Belgium	Belgium
Belgium-Luxembourg	
	British Virgin Islands
	Channel Islands
Cook Isds	
	Curacao
Czechia Czechoslovakia	Czech Republic
East and West Pakistan	
	Equatorial Guinea
Ethiopia Frm Ethiopia	Ethiopia
Frm Tanganyika	
Fmr Yugoslavia	
French Guiana	
Germany Fmr Dem. Rep. of Germany Fmr Fed. Rep. of Germany	Germany
	Gibraltar
Guadeloupe	
	Guam
India India, excl. Sikkim	India
	Isle of Man
	Kosovo
	Liechtenstein
	Marshall Islands
Mayotte	
	Monaco
Montserrat	
	Nauru
Neth. Antilles Neth. Antilles and Aruba	
Niue	
	North Korea
	Northern Mariana Islands
Panama Fmr Panama, excl. Canal Zone	Panama
Pensinsula Malayia	
	Puerto Rico
Réunion	
Sabah	
Saint Kitts, Nevis and Anguilla	
Saint Pierre and Miquelon	
	San Marino
Serbia and Montenegro	
	Sint Maarten (Dutch part)
State of Palestine	
	West Bank and Gaza
	St. Martin (French part)
Sudan Fmr Sudan	Sudan South Sudan
USA USA (before 1981)	United States of America
	Uzbekistan
Viet Nam Frm Rep. of Vietnam	Vietnam
Yemen Frm Arab Rep. Of Yemen	Yemen, Rep.

Table A6: Countries unmergeable due to name and time inconsistencies (economic datasets)

UN Comtrade exports years available (coded and nonmissing)				*WDI tradeopenness years available (coded and nonmissing)*			
Country	*First Year*	*Last Year*	*N*	*Country*	*First Year*	*Last Year*	*N*
Belgium	1999	2017	19	Belgium	1960	2016	57
Belgium-Luxembourg	1962	1998	30				
Bosnia Herzegovina	2003	2017	15	Bosnia and Herzegovina	1994	2016	23
Czechia	1993	2017	24	Czech Republic	1990	2016	27
Czechoslovakia	1968	1987	20				
Pakistan	1972	2017	31	Pakistan	1967	2016	50
East and West Pakistan	1962	1971	10				
Ethiopia	1995	2016	21	Ethiopia	2011	2016	6
Fmr Ethiopia	1962	1987	21				
Fmr Yugoslavia	1962	1987	26				
Germany	1991	2017	27	Germany	1970	2016	47
Fmr Dem. Rep. of Germany	1985	1987	3				
Fmr Fed. Rep. of Germany	1962	1990	29				
India	1975	2017	43	India	1960	2016	57
India, excl. Sikkim	1962	1974	13				
Panama	1978	2016	32	Panama	1960	2016	57
Fmr Panama, excl.Canal Zone	1962	1977	16				
Serbia	2005	2017	13	Serbia	1995	2016	22
Serbia and Montenegro	1992	2004	9				
State of Palestine	2007	2016	10	West Bank and Gaza	1994	2016	23
Sudan	2012	2015	2	Sudan	1960	2016	57
Fmr Sudan	1963	2011	37				
				South Sudan	2008	2015	8
Viet Nam	2000	2016	17	Vietnam	1986	2016	31
Fmr Rep. of Vietnam	1963	1973	11				
Yemen	2004	2015	12	Yemen, Rep.	1990	2016	27
Fmr Arab Rep. of Yemen	1975	1981	6				

Appendix Table A7

Conflict dataset and codebook comparison

Table A7 is a comparison of country coding units in the UCDP/PRIO Armed Conflict dataset 18.1 to the coding units supplied in the code book. Countries with inconsistent labels are highlighted in **blue**; countries which only exist in the dataset but not in code book are highlighted in **red**.

Table A7: Comparison of country coding units in UCDP/PRIO Armed Conflict dataset 18.1 and the coding units supplied in the respective code book

Countries coded as state actors in side A or B of the UCDP/PRIO Armed Conflict dataset 18.1				*System membership table (Table 3) UCDP/PRIO Armed Conflict Dataset Codebook (pp.15–20)*		
Country	*First Year*	*Last Year*	*# obs*	*State Name*	*First Year*	*Last Year*
Afghanistan	1978	2017	47	Afghanistan	1946	2012
Albania	1946	1946	2	Albania	1946	2012
Algeria	1963	2017	30	Algeria	1962	2012
Angola	1975	2017	36	Angola	1975	2012
Argentina	1955	1982	8	Argentina	1946	2012
				Armenia	1991	2012
Australia	2003	2003	2	Australia	1946	2012
				Austria	1946	2012
Azerbaijan	1991	2017	15	Azerbaijan	1991	2012
				Bahamas	1973	2012
				Bahrain	1971	2012
Bangladesh	1975	2017	21	Bangladesh	1971	2012
				Barbados	1966	2012
				Belarus (Byelorussia)	1991	2012
				Belgium	1946	2012
				Belize	1981	2012
				Benin	1960	2012
				Bhutan	1949	2012
Bolivia	1946	1967	3	Bolivia	1946	2012
Bosnia-Herzegovina	1992	1995	9	Bosnia-Herzegovina	1992	2012
				Botswana	1966	2012
				Brazil	1946	2012
				Brunei	1984	2012
				Bulgaria	1946	2012
Burkina Faso	**1985**	**1987**	**3**	**Burkina Faso (Upper Volta)**	**1960**	**2012**
Burundi	1965	2015	19	Burundi	1962	2012
Cambodia (Kampuchea)	1967	2011	42	Cambodia (Kampuchea)	1953	2012
Cameroon	1960	2017	10	Cameroon	1960	2012
				Canada	1946	2012
				Cape Verde	1975	2012
Central African Republic	2001	2013	8	Central African Republic	1960	2012
Chad	1966	2017	43	Chad	1960	2012
Chile	1973	1973	1	Chile	1946	2012
China	1946	2008	45	China	1946	2012
Colombia	1964	2016	53	Colombia	1946	2012
Comoros	1989	1997	2	Comoros	1975	2012
Congo	1993	2016	6	Congo	1960	2012
DR Congo (Zaire)	**1960**	**2017**	**30**	**Congo, Democratic Republic of (Zaire)**	**1960**	**2012**
Costa Rica	1948	1948	1	Costa Rica	1946	2012
Ivory Coast	**2002**	**2011**	**4**	**Cote D'Ivoire**	**1960**	**2012**
Croatia	1992	1995	3	Croatia	1991	2012
Cuba	1953	1961	5	Cuba	1946	2012
Cyprus	1974	1974	2	Cyprus	1960	2012
				Czech Republic	1993	2012
				Czechoslovakia	1946	1992

Table A7 (continued)

Countries coded as state actors in side A or B of the UCDP/PRIO Armed Conflict dataset 18.1				*System membership table (Table 3) UCDP/PRIO Armed Conflict Dataset Codebook (pp.15–20)*		
Country	*First Year*	*Last Year*	*# obs*	*State Name*	*First Year*	*Last Year*
				Denmark	1946	2012
Djibouti	1991	2008	7	Djibouti	1977	2012
Dominican Republic	1965	1965	1	Dominican Republic	1946	2012
				East Timor	2002	2012
Ecuador	1995	1995	2	Ecuador	1946	2012
Egypt	1948	2017	29	Egypt	1946	2012
El Salvador	1969	1991	16	El Salvador	1946	2012
				Equatorial Guinea	1968	2012
Eritrea	1997	2016	12	Eritrea	1993	2012
				Estonia	1991	2012
Ethiopia	1960	2016	131	Ethiopia	1946	2012
				Fiji	1970	2012
				Finland	1946	2012
France	1946	1962	55	France	1946	2012
Gabon	1964	1964	1	Gabon	1960	2012
Gambia	1981	1981	1	Gambia	1965	2012
Georgia	1991	2008	8	Georgia	1991	2012
				German Democratic Republic	1949	1990
				German Federal Republic	1949	2012
Ghana	1966	1983	3	Ghana	1957	2012
Greece	1946	1949	4	Greece	1946	2012
Grenada	**1983**	**1983**	**2**			
Guatemala	1949	1995	34	Guatemala	1946	2012
Guinea	2000	2001	2	Guinea	1958	2012
Guinea-Bissau	1998	1999	2	Guinea-Bissau	1974	2012
				Guyana	1966	2012
Haiti	1989	2004	3	Haiti	1946	2012
Honduras	1957	1969	3	Honduras	1946	2012
Hungary	1956	1956	2	Hungary	1946	2012
Hyderabad	**1947**	**1948**	**4**			
				Iceland	1946	2012
India	1948	2017	220	India	1947	2012
Indonesia	1950	2005	52	Indonesia	1946	2012
Iran	**1946**	**2017**	**62**	**Iran (Persia)**	**1946**	**2012**
Iraq	1948	2017	78	Iraq	1946	2012
				Ireland	1946	2012
Israel	1948	2014	86	Israel	1948	2012
				Italy/Sardinia	1946	2012
				Jamaica	1962	2012
				Japan	1946	2012
Jordan	1948	2016	6	Jordan	1946	2012
				Kazakhstan	1991	2012
Kenya	1982	2017	4	Kenya	1963	2012
				Kosovo	2008	2012
Kuwait	1990	1991	2	Kuwait	1961	2012
				Kyrgyz Republic	1991	2012

Table A7 (continued)

Countries coded as state actors in side A or B of the UCDP/PRIO Armed Conflict dataset 18.1				*System membership table (Table 3) UCDP/PRIO Armed Conflict Dataset Codebook (pp.15–20)*		
Country	*First Year*	*Last Year*	*# obs*	*State Name*	*First Year*	*Last Year*
Laos	1959	1990	22	Laos	1954	2012
				Latvia	1991	2012
Lebanon	1948	2017	17	Lebanon	1946	2012
Lesotho	1998	1998	1	Lesotho	1966	2012
Liberia	1980	2003	7	Liberia	1946	2012
Libya	1987	2017	8	Libya	1951	2012
				Lithuania	1991	2012
				Luxembourg	1946	2012
Macedonia, FYR	**2001**	**2001**	**1**	**Macedonia (FRY)**	**1991**	**2012**
Madagascar	**1971**	**1971**	**1**	**Madagascar (Malagasy)**	**1960**	**2012**
				Malawi	1964	2012
Malaysia	1958	2013	15	Malaysia	1957	2012
				Maldives	1965	2012
Mali	1985	2017	18	Mali	1960	2012
				Malta	1964	2012
Mauritania	1975	2011	6	Mauritania	1960	2012
				Mauritius	1968	2012
Mexico	1994	1996	2	Mexico	1946	2012
Moldova	1992	1992	1	Moldova	1991	2012
				Mongolia	1946	2012
				Montenegro	2006	2012
Morocco	1963	1989	17	Morocco	1956	2012
Mozambique	1977	2016	18	Mozambique	1975	2012
Myanmar (Burma)	1948	2017	275	Myanmar (Burma)	1948	2012
				Namibia	1990	2012
Nepal	1960	2006	14	Nepal	1946	2012
Netherlands	1946	1962	5	Netherlands	1946	2012
				New Zealand	1946	2012
Nicaragua	1957	1990	13	Nicaragua	1946	2012
Niger	1991	2017	10	Niger	1960	2012
Nigeria	1966	2017	20	Nigeria	1960	2012
North Korea	1949	1953	10	North Korea	1948	2012
				Norway	1946	2012
Oman	1957	1975	8	Oman	1946	2012
Pakistan	1948	2017	55	Pakistan	1947	2012
Panama	1989	1989	3	Panama	1946	2012
Papua New Guinea	1990	1996	6	Papua New Guinea	1975	2012
Paraguay	1947	1989	3	Paraguay	1946	2012
Peru	1965	2010	24	Peru	1946	2012
Philippines	1946	2017	104	Philippines	1946	2012
				Poland	1946	2012
Portugal	1961	1974	36	Portugal	1946	2012
				Qatar	1971	2012
Rumania	1989	1989	1	Rumania	1946	2012
Russia (Soviet Union)	1946	2017	44	Russia (Soviet Union)	1946	2012
Rwanda	1990	2016	17	Rwanda	1962	2012

Table A7 (continued)

Countries coded as state actors in side A or B of the UCDP/PRIO Armed Conflict dataset 18.1				*System membership table (Table 3) UCDP/PRIO Armed Conflict Dataset Codebook (pp.15–20)*		
Country	*First Year*	*Last Year*	*# obs*	*State Name*	*First Year*	*Last Year*
Saudi Arabia	1979	1979	1	Saudi Arabia	1946	2012
Senegal	1990	2011	10	Senegal	1960	2012
Serbia (Yugoslavia)	**1991**	**1999**	**5**	**Serbia**	**2006**	**2012**
				Yugoslavia (Serbia)	**1946**	**2006**
Sierra Leone	1991	2001	11	Sierra Leone	1961	2012
				Singapore	1965	2012
				Slovakia	1993	2012
				Slovenia	1992	2012
				Solomon Islands	1978	2012
Somalia	1964	2017	32	Somalia	1960	2012
South Africa	1966	1988	30	South Africa	1946	2012
South Korea	1949	1953	5	South Korea	1948	2012
South Sudan	2011	2017	9	South Sudan	2011	2012
Spain	1957	1991	11	Spain	1946	2012
Sri Lanka	1971	2009	27	Sri Lanka	1948	2012
Sudan	1963	2017	49	Sudan	1956	2012
Suriname	**1987**	**1987**	**1**	**Surinam**	**1975**	**2012**
				Swaziland	1968	2012
				Sweden	1946	2012
				Switzerland	1946	2012
Syria	1948	2017	27	Syria	1946	2012
Taiwan	1949	1958	4	Taiwan	1949	2012
Tajikistan	1992	2011	10	Tajikistan	1991	2012
Tanzania	**1978**	**1978**	**2**	**Tanzania/Tanganyika**	**1961**	**2012**
Thailand	1946	2017	32	Thailand	1946	2012
				Tibet	1946	1950
Togo	1986	1986	1	Togo	1960	2012
Trinidad and Tobago	1990	1990	1	Trinidad and Tobago	1962	2012
Tunisia	1961	2016	3	Tunisia	1956	2012
Turkey	**1974**	**2017**	**41**	**Turkey/Ottoman Empire**	**1946**	**2012**
				Turkmenistan	1991	2012
Uganda	1971	2017	41	Uganda	1962	2012
Ukraine	2014	2017	7	Ukraine	1991	2012
				United Arab Emirates	1971	2012
United Kingdom	1946	2003	56	United Kingdom	1946	2012
United States of America	1950	2017	23	United States of America	1946	2012
Uruguay	1972	1972	1	Uruguay	1946	2012
Uzbekistan	1999	2004	3	Uzbekistan	1991	2012
Venezuela	1962	1992	3	Venezuela	1946	2012
Vietnam (North Vietnam)	**1965**	**1988**	**24**	**Vietnam, Democratic Republic of**	**1954**	**2012**
South Vietnam	**1955**	**1975**	**32**	**Vietnam, Republic of**	**1954**	**1975**
Yemen (North Yemen)	**1948**	**2017**	**27**	**Yemen (Arab Republic of Yemen)**	**1946**	**2012**
South Yemen	**1972**	**1986**	**5**	**Yemen, People's Republic of**	**1967**	**1990**
				Zambia	1964	2012
				Zanzibar	1963	1964
Zimbabwe (Rhodesia)	1967	1979	9	Zimbabwe (Rhodesia)	1965	2012

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Burden-sharing for global cooperation on safety and security

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Abstract

Across the world, the perceived common ground regarding global safety and security is changing. Facing divergent threats, in addition to their cooperation on defense states will increasingly need to collaborate on additional dimensions to protect their citizens. Hence, next to the military burden-sharing debate, questions as to whether states are contributing their fair shares in other arenas as well will be subject to debate also. This article analyzes national contributions by 28 NATO states to five dimensions connected to today's safety and security situation, namely military expenditures, foreign aid, combating terror financing, carbon dioxide reductions, and refugee protection. We find that states vary in their contributions to safety and security, each preferring to fund some dimensions more than others. We suggest that acknowledging and allowing for a certain degree of complementarity among states could help transform the debate on burden-sharing, which is cost-focused, to include benefit-sharing behavior. Thus, it may become possible to value every country's contributions and, building on national strengths, to further cooperation for safety and security along all necessary dimensions.

From its inception in 1949, the topic of military burden-sharing behavior has featured among NATO member states, at time covertly but often prominently. Former U.S. ambassador to the European Union, Anthony Gardner, has argued that burden-sharing discussions should not only focus on military expenditures alone, but should include soft power issues such as immigration and climate change. In its reaction, the Trump administration stated that it is not pursuing burden-sharing agreements regarding soft issues.[1]

According to Cottey, global security threats refer to multiple public goods ranging across widely divergent realms, such as environment, health, mass migration, and transnational organized crime. Depending on the public goods they contribute to, states may under-contribute in one realm and over-contribute in another. In developing a two-country, two-public goods model allowing for tradeoffs between alliance members, Boyer broadened the scope of the burden-sharing debate beyond "the narrow military approach." Testing the model empirically by analyzing member states' contributions to military expenditure and foreign aid, Boyer finds different policy preferences among states to be beneficial as it allows for specialized contributions to alliance security.[2]

Adding states' contributions to the United Nations and to world CO_2 reductions on top of the parameters of defense and development aid, Chalmers extended Boyer's research. More recently, instead of analyzing national contributions to various dimensions of safety and security separately, Sandler and Shimizu used a broader security burden-sharing measure, totaling all expenditures on defense, peacekeeping, and foreign aid per ally.[3]

Against this background, our article investigates national contributions to common safety and security dimensions, comprising defense, terror, irregular migration, poverty, and climate change. As such, the article builds on Boyer's and Chalmers' previous work. In contrast to Sandler and Shimizu, we do not provide an overarching burden-sharing yardstick on safety and security. Neither do we provide, for each state, the sum of its contributions to various dimensions. One reason for this is that to obtain insight into how burdens are being shared we will not analyze financial contributions only but other measures as well. Moreover, one should consider adding weighting factors to divergent safety and security dimensions for the purpose of generating an overarching burden-sharing yardstick.

The normative use of language in this article is grounded in our hope to help recast the burden-sharing debate, however slightly. To do so necessarily requires a "what *should* be measured" criterion. This is not to say that the particular measures we put forward are the only ones worth considering. On the contrary, we acknowledge that our indicator choices are indicative and are meant to help start a broadened discussion.

The remainder of the article is structured as follows. The

next section explains our method of inquiry and presents the data sources. This is followed by a descriptive record of national contributions to international safety and security on five dimensions: military contributions, foreign aid, combating terror finance, carbon dioxide reductions, and refugee protection. Although this could be extended to comprise all countries in the world, we limit it here to NATO member states (as of 2015, hence excluding Montenegro which accessed in 2017) as these are part of an alliance and data are readily available. The section thereafter uses pairwise Spearman rank correlation tests to analyze relations between states' contributions to the five dimensions. We find that member states not only contribute differently to safety and security but that each state appears to prefer investments in specific dimensions over other dimensions. In the concluding section we argue that acknowledging and allowing for a degree of complementarity among member states regarding their national preferences, the debate on burden-sharing behavior could be transformed into one that emphasizes benefit-sharing behavior. Thus, it may become possible to value every contribution and, building on national strengths, to further cooperation for safety and security on all necessary dimensions.

Table 1: Overview

Dimension	*Measure*	*Period*	*Source*	*Table*
Military	Defense expenditure (% of GDP)	2005–15	NATO (2017)	A1
Foreign aid	ODA (% of GNI)	2005–15	OECD (2017)	A2
Combating terror finance	Compliance rate	Last available	FATF (2017)	A3
Carbon dioxide	Metric tons of CO_2 reductions	2005–15	EU (2017)	A4
Refugee protection	Recognition rate	2005–15	UNHCR (2017)	A5

Method

We cannot analyze the vast array of all possible contributions to international safety and security and limit ourselves to just five: military contributions (defense expenditures), foreign aid (overseas development aid, ODA), combating terror financing (compliance with financial standards), carbon dioxide reductions (metric tons of CO_2 reductions), and refugee protection (asylum acceptance, or recognition, rates). Selected in accordance with threats mentioned in various national strategy documents, these comprise threats posed by states, terror, irregular migration, poverty, and climate change.

Between and among these threats causalities appear. For instance, the UN Intergovernmental Panel on Climate Change (IPCC) reports that if baseline global warming exceeds 1.5 degrees Celsius, droughts, floods, extreme heat, and poverty will increase significantly, potentially affecting the livelihoods of hundreds of millions of people and causing uncontrollable migrant flows. Similarly, Carleton, Hsiang, and Burke find that climatological factors relate to a range of conflict outcomes across the globe. As mentioned, the specific measures we use are merely indicative and primarily serve to broaden the debate. Taking foreign aid as an example: In the United States it is argued that as private-sector foreign aid flows are relatively large as compared to public ODA flows, the sum total of private and public funding used for foreign aid purposes would constitute a better measure. Whether to use the broad or the narrow measure is debatable. Meanwhile, for each safety and security dimension, Table 1 shows our preferred measure, the time period, data source, and the specific tables with the data details per NATO member state.[4]

On some dimensions (e.g., military contribution, CO_2 reductions, refugee protection) multi-criteria burden-sharing measures are available. Compared to single-criterion measures, these are less sensitive to special circumstances characterizing individual states. However, an analysis based on multiple measures is complex. Different measures will result in different rankings and outcomes, and it is not clear what weighting factors to apply. For simplicity, we apply a single burden-sharing measure to each dimension, underpinned by literature, and for which data covering reasonably long time periods are available.[5]

Raw data

Military contributions

Researchers have studied burden-sharing behavior regarding military contributions using dissimilar methods. We use the within-ally parameter to measure NATO members' burden-sharing behavior, i.e., the ratio of defense spending to GDP. In the Wales Summit Declaration of 2014, NATO member heads of state committed themselves formally to aim for a minimum of defense spending of two percent of their GDP, including a minimum of twenty percent of the defense budget on major new equipment. States failing to comply would be allowed one decade of time to increase defense expenditures and investment in major weapon systems accordingly.[6]

For the period 2005–15, Table A1 (in the Appendix) shows defense expenditures as a percentage of GDP. In 2015, five states meet the two percent goal: Estonia (2.07), Greece (2.38), Poland (2.23), the U.K. (2.09), and the U.S. (3.59). The U.S. bears the heaviest burden. In 2015, states that contributed less than one percent were Luxembourg (0.43), Belgium (0.91), Spain (0.92), Hungary (0.94), and Canada (0.98). The average contribution of the European states (1.33) was lower than that of the North American states (2.29).

A long-standing academic and policy debate exists on NATO members' burden-sharing behavior, particularly in regard to military expenditure levels. At times, the debate has been extended to include other measures such as foreign aid and peacekeeping contributions. This article broadens the discussion by thinking normatively about safety and security at large. Empirical analysis shows a correlation of zero of members' spending across five global safety and security dimensions examined. States' spending is idiosyncratic. In the conclusion, the article suggests that it might be helpful to change to debate from one that emphasizes *burden-sharing* to one that emphasizes *benefit-sharing*.

Foreign aid

At least as from 1945 onward, when the United States initiated its Marshall Plan to help rebuild war-torn Western European economies, thereby preventing the region to be unduly affected by communist influence, financial aid has been seen as serving a security function, at least in part. During the 1950s and 1960s, U.S. financial foreign aid mainly aimed to fortify cold war-allied partners in Europe and East Asia; in contrast, Western European states spent much of their foreign aid to protect economic interests in (former) colonial territories.[7]

In 1961, the Organization for Economic Cooperation and Development (OECD) was founded. It commissioned its Development Assistance Committee (DAC) to provide a framework for distributing foreign aid burdens more equally among donor countries. By the mid-1980s, DAC, excluding the U.S. and Switzerland, agreed on a target of spending 0.7 percent of gross national income (GNI) on development assistance. Little empirical research exists on the burden-sharing behavior of national governments regarding their expenditures on foreign aid (e.g., the amount of funding spent to benefit aid agencies). Table A2 shows overseas development assistance as a percentage of GNI for NATO states.[8]

For 2015, the table shows five states scoring above the 0.7 percent target: Norway (1.05), Luxembourg (0.95), Denmark (0.85), the Netherlands (0.75), and the U.K. (0.70). The lowest-scoring states comprise former Warsaw Pact members: Bulgaria (0.09), Latvia (0.09), Romania (0.09), Poland (0.18), and Slovakia (0.10). The average foreign aid contribution of European member states (0.34) surpasses the U.S. contribution (0.17). European member states, and particularly northern European nations, bear the heaviest burden.

Combating terror financing

One strategy to eliminate, or at least to contain, terror threats is to understand the ways in which terror organizations and networks obtain financial resources. Following the money trail can lead to financiers and perpetrators of acts of terror. After the 9/11 attacks, the UN Security Council adopted resolution 1373, which mandates all UN member states to prevent and suppress the financing of terror acts, to criminalize the provision of funds to terror organizations, and to freeze funds of persons and groups engaged in terror-related activities. Additionally, the Financial Action Task Force (FATF) decided to expand on its 40 standards for combating money laundering with eight standards to fight terror financing. (In October 2004 an additional, ninth, standard was put forward.) The standards aim to provide a comprehensive and consistent framework for states to combat money laundering and terror financing. Over 190 jurisdictions worldwide are committed to FATF standards, and compliance levels of individual states are assessed by experts associated with FATF. In line with FATF assessment methodology, compliance with each standard is validated across four categories. "Compliant" means that a country observes a standard fully with respect to all essential criteria; "largely compliant" means there are only minor shortcomings, with a large majority of the essential criteria being fully met; "partially compliant" says that a country has taken some substantive action and complies with some essential criteria; finally, countries assessed as "non-compliant" on a standard are judged to suffer major shortcomings, with a large majority of the essential criteria not being met. "Compliant" scores 3 points, and "largely compliant," "partially compliant," and "'non-compliant" score 2, 1, and 0 points, respectively.[9]

Table A3 shows average compliance scores. Numeric column 4 lists compliance ranks using all 49 standards; column 6 does so using only the 40 anti-money laundering standards, and column 8 shows the ranks for the nine anti-terror financing standards. On anti-terror financing, three states (Albania, Croatia, Iceland) score below even the partially compliant level (i.e., <1), indicating major shortcomings. States scoring between 1 and 2 have taken some substantive action but, as yet, do not comply with all essential criteria. States scoring over 2 show minor shortcomings, fully meeting most essential criteria. The U.K., Spain, and Italy hold the top spots, followed by Canada and the U.S. Combating terror financing is a weakest-link good in that high-performing states cannot

Table 2: NATO member states' contributions to five dimensions of global safety and security

Military		Foreign aid		Anti-terror finance		CO_2 reductions		Refugee protection	
Lowest	*Highest*	*Lowest*	*Highest*	*Lowest*	*Highest*	*Lowest*	*Highest*	*Lowest*	*Highest*
Luxemb.	U.S.	Bulgaria	Norway	Croatia	U.K.	Estonia	Greece	Hungary	Netherl.
Belgium	Greece	Latvia	Luxemb.	Albania	Spain	Turkey	Italy	Croatia	Canada
Spain	Poland	Romania	Denmark	Iceland	Italy	Iceland	Spain	Slovenia	Italy
Hungary	U.K.	Poland	Netherl.	Romania	Canada	Albania	U.K.	Greece	Bulgaria
Canada	Estonia	Slovakia	U.K.	Greece	France	Bulgaria	Denmark	Slovakia	U.S.

compensate for lower-performing partners. Consequently, higher average performance levels across all states can only be reached by helping weakest-link states to increase compliance levels.[10]

Carbon dioxide reductions

Climate change challenges international security. During the 21st UN Climate Conference in Paris, December 2015, 195 states agreed to prevent a global average temperature rise to exceed two degrees Celsius, and hoping to limit temperature increases to a maximum of 1.5 degrees Celsius. But to reach even the two-degree limit, significant reductions of CO_2 emissions are necessary. Table A4 shows the extent to which states have been doing so, for 2005–15. Greece, Italy, Spain, the U.K., and Denmark show the largest reductions—perhaps in part because of economic decline or stagnation in the first three of these—whereas Albania, Bulgaria, Estonia, Iceland, and Turkey show increased CO_2 emissions but, except for Turkey, these are on a fairly small scale. Canada and Germany have reached large absolute emission reductions, yet in percentage terms they are relatively small, perhaps too small to help reach the stated goal of the global climate agreement.[11]

Mass migration and refugee protection

Mass migration can endanger international security because of destabilizing effects resulting from refugee flows and border tensions. Schuck argues that criteria for allocating refugee burdens across nations should be based on states' capacity to provide refugees with minimal safeguards and comfort to which they are entitled under the Refugee Convention and consequently suggests to apply national wealth as a criterion for assigning refugees quotas. Using a state's wealth as the sole criterion neglects, however, important factors such as population density, land surface, national cultures and traditions, public support, and/or national labor markets, all of which affect states' willingness and ability to receive and protect refugees, or other migrants.[12]

To investigate burden-sharing behavior, we instead derive recognition rates, i.e., the number of positive asylum decisions divided by the total number of applications. Accordingly, for 2005–15, Table A5 shows the average number of applications submitted, the average number accepted, and the resulting recognition rates (columns 2, 4, and 6, respectively). Germany, France, and the U.S. score high in absolute numbers on the "applied" and the "accepted" parameters, but Germany and France score only average on the relative measure, the recognition parameter. Hungary scores relatively high on the number of submitted applications (rank 8), but has the lowest recognition rate of all states (rank 28). The Netherlands, Canada, Italy, Bulgaria, and the U.S. sport the highest recognition rates.[13]

Analysis

Table 2 synthesizes Tables A1 to A5 and shows the five lowest and the five highest contributors across our five dimensions. On four of the five dimensions, the U.K. performs in the top-5, whereas the U.S. comes in first on just one dimension, its military contribution to the NATO alliance. Some member states contribute relatively much to one dimension and little to another. Estonia, for example, spends over two percent of its GDP on military contributions, as opposed to 0.15 percent of its GNI on foreign aid. It also shows the highest percentage increase in CO_2 emissions. Luxembourg, in contrast, spends little on its military (0.43%) and relatively much on foreign aid (0.95%). Excepting the U.K., it appears that states that devote a large part of their GDP to military contributions do not always contribute as highly to foreign aid, CO_2 reductions, counter-terror financing compliance, and/or refugee protection. To supplement this qualitative analysis, we use pairwise Spearman rank correlation tests. The null (H_0) and alternative (H_A) hypotheses are as follows:

H_0: No association exists between states' contributions to the five safety and security dimensions.

Table 3: Pairwise Spearman rank correlation coefficients and probability values

Year	*ρ (1,2)*	*ρ (1,3)*	*ρ (1,4)*	*ρ (1,5)*	*ρ (2,3)*	*ρ (2,4)*	*ρ (2,5)*	*ρ (3,4)*	*ρ (3,5)*	*ρ (4,5)*
2005	–0.08	–0.63	–0.08	–0.06	–0.63	0.49**	0.51**	0.00	–0.40	0.16
	(0.71)	(0.37)	(0.70)	(0.78)	(0.37)	(0.02)	(0.012)	(1.00)	(0.60)	(0.41)
2006	–0.21	–0.05	–0.18	0.06	–0.15	0.27	–0.03	0.58**	–0.27	0.00
	(0.33)	(0.89)	(0.39)	(0.78)	(0.64)	(0.21)	(0.91)	(0.05)	(0.40)	(0.99)
2007	–0.33	–0.21	–0.40*	0.12	0.06	0.40*	0.16	0.65***	0.00	–0.09
	(0.13)	(0.47)	(0.06)	(0.58)	(0.84)	(0.06)	(0.46)	(0.00)	(0.99)	(0.64)
2008	–0.37*	–0.17	0.25	0.07	0.11	–0.18	0.21	–0.08	0.14	–0.02
	(0.08)	(0.50)	(0.24)	(0.75)	(0.66)	(0.38)	(0.31)	(0.73)	(0.54)	(0.93)
2009	–0.05	–0.17	–0.22	0.15	0.25	–0.45**	0.23	0.30	0.06	–0.33*
	(0.81)	(0.47)	(0.28)	(0.47)	(0.30)	(0.02)	(0.26)	(0.18)	(0.79)	(0.08)
2010	0.03	–0.02	0.11	0.12	0.24	0.17	0.19	–0.04	0.23	–0.20
	(0.88)	(0.95)	(0.58)	(0.56)	(0.29)	(0.41)	(0.37)	(0.86)	(0.30)	(0.32)
2011	0.07	0.01	–0.02	0.13	0.24	0.31	0.36*	0.36*	0.21	0.01
	(0.74)	(0.95)	(0.91)	(0.52)	(0.23)	(0.12)	(0.07)	(0.06)	(0.27)	(0.97)
2012	0.14	–0.06	–0.05	0.15	0.12	0.17	0.38*	0.24	0.36*	0.10
	(0.51)	(0.76)	(0.79)	(0.45)	(0.55)	(0.39)	(0.06)	(0.22)	(0.06)	(0.61)
2013	0.12	0.03	–0.21	–0.04	0.10	–0.38*	0.21	–0.03	0.21	0.15
	(0.57)	(0.88)	(0.30)	(0.85)	(0.63)	(0.06)	(0.30)	(0.88)	(0.29)	(0.45)
2014	0.15	–0.18	0.08	–0.03	0.06	0.17	0.16	0.24	0.09	0.11
	(0.49)	(0.37)	(0.68)	(0.88)	(0.77)	(0.42)	(0.43)	(0.22)	(0.67)	(0.58)
2015	–0.05	–0.21	0.34*	0.26	0.06	0.35*	0.32	–0.29	–0.14	0.21
	(0.81)	(0.29)	(0.09)	(0.20)	(0.77)	(0.08)	(0.11)	(0.13)	(0.47)	(0.28)

Notes: 1. Military expenditure/GDP (%); 2. ODA/GNI (%); 3. anti-terror finance compliance (%); 4. CO_2 reductions (%); 5. refugee recognition rates (%). Statistically significant at the 1% (***), 5% (**), and 10% (*) levels, respectively.

H_A: An association does exist between states' contributions to the five safety and security dimensions.

Rejection of H_0 is indicative of either a positive or negative correlation between states' ranks on any pairwise set of safety and security dimensions. Table 3 lists the rank correlations (with probability values in parentheses). The tabulated coefficients show no systematic pairwise associations. Across the years, signs flip for pairwise tests, and very few of the tests are statistically significant (statistically different from zero), with none concentrated in any one pairwise comparison column. Thus, for the entire set of comparisons, the null hypothesis cannot be rejected. This is consistent with our previous conclusion, from Table 2: States scoring high on one safety and security dimension do not necessarily perform likewise on any other dimension.

Conclusions

This article shows how 28 NATO states contribute, across five dimensions, to global safety and security. We investigated four nonmilitary dimensions, using a limited number of measures. On its own, military expenditure as a percentage of GDP does not take into account political and societal complexities regarding safety and security. We expect therefore that measuring contributions solely in military terms will not deliver meaningful information on burden-sharing behavior. Our findings show that member states can and do contribute in different ways to global safety and security. Except for the U.K., which scores among the top-5 countries on four of our five dimensions, other states vary the extent to which they contribute across the five dimensions. No one state ranks lowest on all five. Instead, each state appears to invest in some dimension more than in others. As to why states contribute as they do, additional research seems necessary. From a defense economics perspective, it appears that states do not all value certain public goods equally, nor do they agree on any one particular scenario of pursuing shared strategies.

As member states seem to hold specific preferences regarding the production of (global) public goods, implicitly and explicitly agreeing on task specialization may ease disputes over burden-sharing behavior and increase mutual understanding, and may even offer new opportunities. Any one country could over-contribute to the production of a specific public good while under-contributing to others, presuming that the other states would condone and complement this behavior along other dimensions.

In terms of today's burden-sharing debate, seemingly geared toward the negative (i.e., the costs), this may appear

infeasible. At the heart of any burden-sharing debate on safety and security, however, there are objectives coveted by all. No single state possesses all of the necessary political, economic, and cultural resources to achieve all of the objectives. If, next to addressing the military costs incurred, states also devote some attention to highly desirable nonmilitary safety and security benefits, the burden-sharing debate may transform into a dialogue on benefit-sharing behavior. Using one's own and the other states' strengths to achieve mutual benefits, mutual understanding, and mutual recognition of the value of each other's contributions may sustain cooperation across all dimensions of safety and security.

Notes

We thank two anonymous reviewers for their helpful comments and cautionary notes. All remaining errors are ours.

1. CNBC (2017).

2. Boyer (1990).

3. Multiple public goods: Cottey (2007). Broadened the scope: Boyer (1989, p. 700). Adding UN and CO_2 measures: Chalmers (1993; 2000). Broader measure: Sandler and Shimizu (2014).

4. Migrant flows: IPCC (2018, p.53). Conflict outcomes: Carleton, Hsiang, and Burke (2016).

5. Less sensitive: Kawashima (1996).

6. Dissimilar methods: Olson and Zeckhauser (1966); Sandler, Cauley, and Forbes (1980); Oneal (1990); Khanna and Sandler (1996); Sandler and Murdoch (2000); Solomon (2004); Sandler (2005); Sandler and Shimizu (2014). Within-ally: Sandler and Hartley (2001). Wales Summit: NATO (2014, paragraph 14).

7. Chalmers (2000).

8. Little research: But see Boyer (1989); Chalmers (1993); Khanna and Sandler (1997); Addison, McGillivray and Odedokun (2004).

9. Money trail: Beeres and Bollen (2011, p. 92). Scoring of standards: Arnone and Padoan (2008).

10. Weakest-link: Bogers and Beeres (2013).

11. Reductions necessary: Ringius, Torvanger, and Holtsmark (1998); Ringius, Torvanger, and Underdal (2002); Hof, den Elzen, and van Vuuren (2010); Clémençon (2016).

12. Destabilizing effects: Thieleman (2018). Allocating refugee burdens: Schuck (1997).

13. Recognition rates: Vink and Meijerink (2003).

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Table A1: Defense expenditures as a percentage of gross domestic product

	2005–09	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*	*Rank*
Albania	1.52	1.56	1.53	1.49	1.41	1.34	1.16	14
Belgium	1.11	1.08	1.05	1.05	1.01	0.97	0.91	23
Bulgaria	2.53	1.67	1.33	1.35	1.46	1.32	1.29	12
Croatia	1.62	1.54	1.60	1.53	1.47	1.41	1.37	10
Czech Republic	1.56	1.29	1.07	1.06	1.03	0.96	1.06	17
Denmark	1.33	1.41	1.30	1.34	1.23	1.16	1.14	16
Estonia	1.70	1.70	1.68	1.89	1.90	1.94	2.07	5
France	2.34	1.96	1.87	1.87	1.86	1.84	1.80	6
Germany	1.34	1.35	1.28	1.31	1.23	1.19	1.19	13
Greece	2.78	2.64	2.38	2.29	2.22	2.22	2.38	2
Hungary	1.25	1.04	1.05	1.04	0.95	0.87	0.94	21
Iceland	-	-	-	-	-	-	-	-
Italy	1.56	1.35	1.30	1.24	1.20	1.09	1.02	19
Latvia	1.48	1.06	1.02	0.89	0.93	0.94	1.04	18
Lithuania	1.15	0.88	0.79	0.76	0.76	0.88	1.14	16
Luxembourg	0.52	0.47	0.39	0.38	0.38	0.39	0.43	24
Netherlands	1.46	1.34	1.25	1.23	1.16	1.15	1.16	15
Norway	1.49	1.52	1.51	1.47	1.48	1.51	1.47	8
Poland	1.80	1.77	1.72	1.74	1.72	1.85	2.23	3
Portugal	1.57	1.49	1.49	1.41	1.44	1.30	1.32	11
Romania	1.63	1.24	1.28	1.22	1.28	1.35	1.45	9
Slovakia	1.56	1.27	1.09	1.10	0.99	0.99	1.14	16
Slovenia	1.52	1.61	1.30	1.18	1.06	0.98	0.94	21
Spain	1.19	1.03	0.94	1.04	0.92	0.91	0.92	22
Turkey	1.99	1.93	1.76	1.76	1.75	1.70	1.67	7
U.K.	2.42	2.51	2.42	2.20	2.30	2.20	2.09	4
Europe	**1.62**	**1.47**	**1.38**	**1.35**	**1.33**	**1.30**	**1.33**	
Canada	1.28	1.16	1.23	1.10	0.99	1.02	0.98	20
U.S.	4.28	4.81	4.77	4.42	4.09	3.78	3.59	1
North America	**2.78**	**2.99**	**3.00**	**2.76**	**2.54**	**2.40**	**2.29**	

Source: NATO (2017).

Table A2: Overseas Development Assistance as a percentage of gross national income

	2005-10	*2011*	*2012*	*2013*	*2014*	*2015*	*Rank*
Albania	-	-	-	-	-	-	-
Belgium	0.49	0.54	0.48	0.45	0.46	0.42	8
Bulgaria	0.09	0.09	0.08	0.10	0.09	0.09	19
Croatia	-	-	-	-	-	-	-
Czech Republic	0.10	0.13	0.12	0.11	0.11	0.12	17
Denmark	0.87	0.85	0.83	0.85	0.86	0.85	3
Estonia	0.09	0.12	0.11	0.13	0.15	0.15	15
France	0.42	0.46	0.45	0.41	0.37	0.37	9
Germany	0.33	0.39	0.37	0.38	0.42	0.52	6
Greece	0.18	0.15	0.13	0.10	0.11	0.12	17
Hungary	0.08	0.11	0.10	0.10	0.11	0.13	16
Iceland	0.23	0.20	0.20	0.23	0.22	0.24	11
Italy	0.19	0.20	0.14	0.17	0.19	0.22	12
Latvia	0.06	0.07	0.08	0.08	0.08	0.09	19
Lithuania	0.08	0.13	0.13	0.11	0.10	0.12	17
Luxembourg	0.89	0.97	1.00	1.00	1.06	0.95	2
Netherlands	0.80	0.75	0.71	0.67	0.64	0.75	4
Norway	0.93	0.96	0.93	1.08	1.00	1.05	1
Poland	0.07	0.08	0.09	0.10	0.09	0.10	18
Portugal	0.28	0.31	0.28	0.23	0.19	0.16	14
Romania	0.08	0.09	0.09	0.07	0.11	0.09	19
Slovakia	0.07	0.09	0.09	0.09	0.09	0.10	18
Slovenia	0.13	0.13	0.13	0.13	0.13	0.15	15
Spain	0.33	0.29	0.16	0.17	0.13	0.12	17
Turkey	0.10	0.17	0.32	0.40	0.45	0.50	7
U.K.	0.42	0.56	0.56	0.71	0.70	0.70	5
Europe	**0.30**	**0.33**	**0.32**	**0.33**	**0.32**	**0.34**	
Canada	0.29	0.32	0.32	0.28	0.24	0.28	10
U.S.	0.17	0.20	0.19	0.18	0.19	0.17	13
North America	**0.23**	**0.26**	**0.26**	**0.23**	**0.22**	**0.23**	

Source: OECD (2017).

Table A3: Compliance scores on anti-money laundering and combating terror financing

	Report	*Year*	*FATF (49)*	*Rank (49)*	*FATF (40)*	*Rank (40)*	*FATF (9)*	*Rank (9)*
Albania	MER	2011	1.31	21	1.41	19	0.89	13
Belgium	MER	2015	2.12	5	2.15	4	2.00	5
Bulgaria	MER	2008	1.94	10	1.95	11	1.89	6
Croatia	MER	2008	1.09	22	1.21	22	0.56	14
Czech Republic	MER	2011	1.62	17	1.61	17	1.67	8
Denmark	FER	2010	2.00	8	2.03	7	1.89	6
Estonia	FER	2014	1.92	11	1.97	10	1.67	8
France	MER	2011	1.94	10	1.90	12	2.11	4
Germany	FER	2014	2.00	8	2.00	8	2.00	5
Greece	FER	2011	1.31	21	1.31	20	1.33	11
Hungary	FER	2013	2.40	2	2.51	2	1.89	6
Iceland	MER	2006	1.46	20	1.59	18	0.89	13
Italy	MER	2016	2.20	3	2.20	3	2.22	3
Latvia	MER	2012	2.02	7	2.08	6	1.78	7
Lithuania	MER	2012	1.85	13	1.97	10	1.33	11
Luxembourg	FER	2014	1.31	21	1.28	21	1.44	10
Netherlands	FER	2014	1.82	14	1.75	16	2.11	4
Norway	MER	2014	1.88	12	1.83	14	2.11	4
Poland	MER	2013	1.75	16	1.79	15	1.56	9
Portugal	MER	2006	1.98	9	2.03	7	1.56	9
Romania	MER	2008	1.50	19	1.59	18	1.11	12
Slovakia	MER	2011	1.54	18	1.59	18	1.33	11
Slovenia	MER	2010	2.08	6	2.08	6	2.11	4
Spain	MER	2014	2.55	1	2.60	1	2.33	2
Turkey	FER	2014	1.79	15	1.87	13	1.44	10
U.K.	MER	2009	2.18	4	2.13	5	2.44	1
Europe		**1.83**	**1.86**		**1.68**			
Canada	MER	2016	1.94	10	1.90	12	2.11	4
U.S.	MER	2016	1.98	9	1.98	9	2.00	5
North America		**1.96**	**1.94**		**2.06**			

Source: FATF (2017). FATF (49) represents the total average compliance score on all 49 FATF recommendations of a state; FATF (40) is the total average compliance score on the 40 anti-money laundering standards of a state; FATF (9) is the total average compliance score on the 9 special standards to combat terror finance of a state; for scoring criteria, see main text. MER = mutual evaluation report; FER = follow-up evaluation report.

Table A4: Reduction of carbon dioxide emissions (in metric tons of CO_2)

	2005	*2015*	*Change*	*Change (%)*	*Rank*
Albania	4,137	4,439	302	6.80	25
Belgium	116,820	97,002	-19.818	-20.43	10
Bulgaria	52,068	53,432	1,364	2.55	24
Croatia	22,695	20,538	-2,157	-10.50	16
Czech Republic	127,283	111,092	-16,191	-14.57	13
Denmark	50,856	36,908	-13,948	-37.79	5
Estonia	17,769	29,252	11,483	39.26	28
France	410,066	327,787	-82,279	-25.10	8
Germany	830,597	777,905	-52,692	-6.77	19
Greece	103,910	68,292	-35,618	-52.16	1
Hungary	59,607	48,186	-11,421	-23.70	9
Iceland	3,126	3,874	748	19.31	26
Italy	492,898	352,886	-140,012	-39.68	2
Latvia	7,981	7,973	-8	-0.10	23
Lithuania	13,616	12,478	-1,138	-9.12	17
Luxembourg	12,046	10,235	-1,811	-17.69	12
Netherlands	179,600	165,317	-14,283	-8.64	18
Norway	43,291	43,109	-182	-0.42	21
Poland	308,755	294,879	-13,876	-4.71	20
Portugal	67,215	50,792	-16,423	-32.33	6
Romania	104,206	81,247	-22,959	-28.26	7
Slovakia	42,789	36,254	-6,535	-18.03	11
Slovenia	17,738	15,610	-2,128	-13.63	15
Spain	366,314	262,683	-103,631	-39.45	3
Turkey	248,620	357,157	108,537	30.39	27
U.K.	555,007	398,524	-156,483	-39.27	4
Europe	**4,259,010**	**3,667,851**	**-591,159**	**-16.12**	
Canada	557,423	555,401	-2,022	-0.36	22
U.S.	5,886,318	5,172,338	-713,980	-13.80	14
North America	**6,443,741**	**5,727,739**	**-716,002**	**-7.08**	

Source: EU (2017).

Table A5: Average asylum applications recognition rate, 2005–15

	Average applicants (absolute)	*Rank*	*Average applicants accepted (absolute)*	*Rank*	*Recognition rate (%)*	*Rank*
Albania	125	27	10	23	8.12	23
Belgium	28,637	7	5,501	9	19.21	15
Bulgaria	4,203	16	1,513	12	36.00	4
Croatia	496	21	16	23	3.22	27
Czech Republic	2,135	17	347	16	16.26	18
Denmark	7,097	14	2,411	11	33.97	6
Estonia	64	28	14	23	22.05	13
France	82,217	2	15,043	3	18.30	17
Germany	104,587	1	24,725	1	23.64	11
Greece	19,863	10	1,196	14	6.02	25
Hungary	23,683	8	309	17	1.31	28
Iceland	128	26	15	23	11.82	21
Italy	28,898	6	11,238	4	38.89	3
Latvia	148	25	18	23	12.11	20
Lithuania	280	24	57	21	20.23	14
Luxembourg	1,471	18	269	19	18.31	16
Netherlands	17,447	12	8,954	8	51.32	1
Norway	19,268	11	4,786	10	24.84	10
Poland	9,624	13	1,462	13	15.20	19
Portugal	304	23	79	20	25.98	8
Romania	1,269	19	297	18	23.37	12
Slovakia	1,242	20	77	20	6.18	24
Slovenia	460	22	25	22	5.47	26
Spain	5,438	15	620	15	11.40	22
Turkey	32,498	5	9,412	7	28.96	7
U.K.	42,820	4	11,024	5	25.75	9
Europe	**434,404**		**99,419**		**22.89**	
Canada	22,870	9	9,856	6	43.10	2
U.S.	60,750	3	21,271	2	35.01	5
North America	**83,620**		**31,128**		**37.23**	

Source: UNHCR (2017).

Demilitarizing a small African country: Rationale, necessary conditions, and financing

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Abstract

Most efforts directed at security sector reform (SSR) in African countries have had very little impact. This includes efforts aimed at a more rational allocation of tasks and resources in the sector. This article is concerned with the strongest form of SSR, the total disbanding of military forces. The best example of effective demilitarization is Costa Rica, which has flourished since it disbanded its military some 70 years ago. The strategic situation, the negative behavior of its defense force since its formation, and the opportunity costs of military expenditure provide a strong case for the demilitarization of Lesotho, a small country in southern Africa. Five necessary conditions for a successful demilitarization can be identified, namely its acceptance by a country's citizens, a willing government, a detailed demilitarization plan, an implementing agency, and adequate finances. While these are interrelated, the article focuses on financial aspects, including the need for foreign assistance to finance the initial investment required. The peace dividend resulting from demilitarization could be used to provide a basic income grant to all adult citizens. We estimate that this would raise average incomes of the poorest 95 percent of households by around 20 percent per annum.

The concept of security sector reform (SSR) was introduced some 20 years ago by Clare Short, then U.K. Secretary of State for International Development. A decade later, a review of the experience of security sector reform efforts in Africa concluded that little reform had in fact taken place.[1] Another decade on, there still is little evidence of resource reallocations within the security sector in response to a more rational allocation of functions among the various components of the sector. The military has generally proven adept at maintaining, and often increasing, their share of national budgets and the question whether a country needs a military of a particular size, let alone whether it needs one at all, is almost never asked.

As shown in Table 1 there are, however, some 21 sovereign states without armed forces—15 with no official military forces and six without standing army but some form of limited military forces. Some of these states have never had military forces while others made a decision to demilitarize. Most are small island states with populations of less than 200,000 people although some are much larger. It has been argued that most small countries have virtually no capacity to maintain armed forces of any military usefulness and should therefore make alternative arrangements to meet their national security needs.[2] In consequence, the argument presented in this article has most relevance to small countries.

Demilitarization may be total or partial and will very likely have two aspects. At the least, it will involve a significant and sustained reduction in the power and influence of the military indicated by reductions in military expenditure, military personnel, and the effectiveness or capacity of a military; the last is often termed force projection. Demilitarization may well go together with efforts to move toward a "culture of peace," whereby a society emphasizes the nonviolent resolution of conflict and of personal and social justice. An alternative way of expressing this is via the concept of positive peace, which

Table 1: Countries without armed forces

No official military forces
Andorra, Dominica, Grenada, Kiribati, Liechtenstein, Marshall Islands, Federated States of Micronesia, Nauru, Palau, Saint Lucia, Saint Vincent and the Grenadines, Samoa, Solomon Islands, Tuvalu, Vatican State

No standing army but limited military capacity
Costa Rica, Iceland, Mauritius, Monaco, Panama, Vanuatu

Source: Wikipedia (2018).

can result from conscious and sustained efforts to address the underlying causes of conflict, be they interpersonal, intergroup, or international. Countries on such a path have the motive and opportunity to divert government expenditures from the military to expenditure categories such as health, education, and welfare, which further reinforces positive peace. A critical component of a commitment to a culture of peace and/or positive peace, as discussed later, is a constitutional change which bans the existence of a standing army.

Table 2: Costa Rica and its neighbors

	Ranking			*Share in GDP (%)*		
	HDI	*GPI*	*WHI*	*Health*	*Education*	*Military*
Costa Rica	65	40	13	6.8	7	0
El Salvador	117	116	40	4.5	3.4	0.9
Guatemala	125	111	41	2.3	2.8	0.4
Honduras	130	118	30	2.5	5.9	1.6
Nicaragua	124	68	72	5.1	4.5	0.6
Panama	60	50	27	5.9	3.3	0

Sources: HDI (United Nations Development Program, 2018); GPI (Institute for Economics and Peace, 2017); WHI (Helliwell, Layard, and Sachs, 2017); Health and Education (United Nations Development Program, 2018); Military (Stockholm International Peace Research Institute, 2018).

Costa Rica's demilitarization

Following civil war, Costa Rica's decision to disband its military was formalized in Article 12 of its 1949 Constitution:

> The Army as a permanent institution is abolished. There shall be the necessary police forces for surveillance and the preservation of the public order. Military forces may only be organized under a continental agreement or for the national defense; in either case, they shall always be subordinate to the civil power: they may not deliberate or make statements or representations individually or collectively.[3]

Internal security in Costa Rica is maintained by a range of uniformed forces, including the Public Forces, Judicial Police (investigative), Transit Police (transport), Municipal Police, DIS (intelligence), and the Coast Guard. For the purpose of national security, Costa Rica has opted for collective security under the Organization of the American States (OAS) and it is a signatory to the Inter-American Reciprocal Defence Treaty (TIAR). Costa Rica has invoked TIAR against invasion threats from neighboring states, particularly Nicaragua, in 1948, 1955, and 1978. In each instance, OAS intervention facilitated the signing of peace pacts between Costa Rica and Nicaragua which have proven reasonably durable. In 2018, the United Nations Court of Justice granted Costa Rica sovereignty over a coastal border area claimed by both countries. In recent years, the country has allowed U.S. military forces into its waters and ports to assist in the fight against drug trafficking, but this does not in any formal sense mean that the United States is a guarantor of Costa Rica's security.

For decades, the country has been known as a haven of peace, democracy, and prosperity in a region bedeviled by violence and insecurity. It operates a strictly neutral foreign policy. From time to time, it has acted as a mediator in the conflicts of its neighbors and has helped to build democracy in the region and in the wider world. It hosts the Inter-American Court of Human Rights and the United Nations University for Peace. Table 2 summarizes the country's ranking on some well-known indicators and expenditures on public health, public education, and the military, each as a proportion of its GDP, as compared to its neighbors. The only one of its neighbors which comes close to Costa Rica in terms of its ranking in the Human Development Index (HDI), the Global Peace Index (GPI), and the World Happiness Index (WHI) is Panama, which itself demilitarized in 1990, and Costa Rica's public expenditure on health and education as proportions of GDP is far higher.

Acknowledging the positives of his country's demilitarization, former president Oscar Sanchez noted that

> [i]nternational development agencies recognize that Costa Rica today has a standard of living comparable to that of industrialised countries. It is universally accepted that the extraordinary advances of my country in the fields of education, health, housing and social welfare are basically due to the fact that we do not dedicate our resources to the purchase of arms. The absence of the army has strengthened the Costa Rica democracy system, making it one of the most consolidated democracies of Latin America. To us, these are the dividends that would be within the grasp of all third world countries if they did not dedicate a very important part of their resources to the purchasing of arms.[4]

To understand Costa Rica's socioeconomic success only in terms of tradeoffs between military and other expenditures may

be too simple. Costa Rica has had a long commitment to human rights and social democracy (for example, it abolished the death penalty as early as 1877 and banned the corporal punishment of children in 2008) which suggests that a combination of a mindset inclined to demilitarization along with expenditure (re)allocations is essential for a successful demilitarization. We will return to the importance of such a mindset later on.[5]

Thus far, we have shown that countries can survive—and some can thrive—without the presence of a military force. We now turn to considering whether demilitarization is appropriate and feasible for Lesotho.

Not quite two dozens countries in the world are demilitarized. This article examines the case for demilitarization for Lesotho, a small country in southern Africa. Five necessary conditions for demilitarization are identified. The discussion suggests the implementation of a basic income grant to all adult citizens of Lesotho to best recycle the resources from demilitarization and advance the country's economic development prospects.

The case for demilitarizing Lesotho

Lesotho is a small, landlocked country in southern Africa with a population of some 2.2 million people. Ranking 159th of the 189 countries reported on in the UNDP's Human Development Index, it has per capita gross national income, in 2018, of PPP$3,255 (i.e., in purchasing power parity or international dollars). The most recent poverty data indicates that 59.6 percent of its population earns less than the international poverty line minimum of PPP1.90/day. Life expectancy is a scant 54.6 years and mean years of schooling are but 6.3.[6]

Three main reasons for the demilitarization of Lesotho can be identified, namely the lack of any significant need in terms of national security, a continual history of military interference with democratic processes, and—given the high levels of poverty—the high opportunity cost of the resources allocated to the military.[7] First, the country is completely surrounded by South Africa, one of Africa's military superpowers. Assuming that it had reason to invade, with military expenditure in 2017 almost 70 times that of Lesotho, South Africa would easily overwhelm the Lesotho Defence Force (LDF) of some 3,000 soldiers. Lesotho's army must not be viewed as an end in itself. If it serves no significant security function, then its very existence needs to be reconsidered.[8]

Second, from its inception, the LDF has interfered with the democratic process and engaged in human rights abuse. Having survived without a military for 13 years following independence, the Lesotho Para-Military Force was forged out of the Police Mobile Unit (the riot squad) in 1979 and was used by the ruling party to thwart political opposition and strengthen its hold on power. In 1986, the military took over political power via a coup. Even after the return of the country to civilian rule in 1993, it continued to interfere in political processes, prompting the then-Prime Minister to include the military among "the five enemies of democracy" in the country. In April 1994, a group of soldiers took four government ministers hostage, resulting in the death of the deputy prime minister. The prime minister called for assistance from the Commonwealth and South Africa to disarm the LDF but this only occurred (and even then, only partially) following military intervention by South Africa and Botswana under the auspices of the Southern African Development Community (SADC) in 1998.[9]

In August 2014, members of the LDF stormed then-Prime Minister Thabane's official residence in what local and international commentators labeled an attempted coup. Thabane fled to South Africa but returned some days later under the guard of South African security personnel, against his own armed forces, until he vacated office following the 2015 elections. The LDF has also been racked by internal divisions and intrigues. In addition to many incidences of mutiny, two of its commanders were murdered by subordinates, in June 2015 and September 2017.

In response to the events of late 2014, SADC intervened to help build political and security sector stability in Lesotho. A standby force of 269 personnel was sent to the country in December 2017 to facilitate an environment conducive for the implementation of SADC decisions, including security sector reform. Most recently, SADC established the Lesotho National Dialogue and Stabilization Project to help develop a "roadmap" for constitutional and security sector reforms, scheduled for completion by May 2019.

Third, given the extent of poverty in the country, military expenditure comes with high opportunity costs. In 2017, Lesotho's military expenditure was USD52.6 million (in local currency, 698 million maloti), 70 percent of which consisted of personnel costs. In constant prices and exchange rates (2016 USD), the country's military expenditure has doubled in the 10 years from 2008 to 2017 and now represents 2.2 percent of GDP and 5.1 percent of government expenditure.[10]

Requirements for effective demilitarization

Demilitarization can occur in several ways. It might be forcefully imposed on a country in the hope that in due course its net benefits will become apparent to its citizens; or it might come about more gradually as a logical consequence of movement toward a culture of peace over time. In either case, the actual and perceived security needs of the country will need

to be taken into account. Five requirements, often interrelated, seem necessary for a successful demilitarization, namely widespread acceptance by the citizenry, a government which is willing to make the decision (preferably with the support of other political parties), a detailed plan for implementation, an implementing agency, and adequate finance. We briefly discuss each of these.[11]

Widespread acceptance by citizens

A well-known UNESCO statement asserts that "[s]ince war begins in the minds of men, it is in the minds of men that the defences of peace must be constructed." Fundamentally, demilitarization needs a strong and widespread aspiration for a culture of peace as opposed to a culture of violence. Such a change may occur relatively quickly as the result of a crisis, as with Panama (which demilitarized in 1990 following the U.S. invasion) and Haiti (which disbanded its military in 1994 when a civilian government replaced the military government), or it may be more gradual and be built on careful planning. It may begin as a groundswell among the masses and/or it may be inspired by political leadership. It may be entirely indigenous or it may be supported by international agencies, NGOs, or governments.[12]

This change in mindset will involve a population feeling more secure. It is possible that this may occur simply by disbanding the military, as may have been the case in Haiti, but they may need to be convinced that other arrangements are being made. Of course, security has a much wider meaning than protection against the threat of invasion and the UNDP's concept of human security includes economic security, food security, health security, environmental security, personal security, community security, and political security. If the narrow aspect of security is regarded as relatively unimportant by society—by comparison, say, with economic security—it may be possible to use resources released by demilitarization to build economic security. More generally, as alternative ways of achieving security are developed, feelings of security, widely defined, likely will increase. Neighbors can support a country's decision to demilitarize, and help nurture the new mindset, by appropriate confidence-building measures, e.g., assurances of peaceful intent and perhaps statements about their own plans to demilitarize.

A willing government

The government of the day needs to be willing and able to make the decision to demilitarize. It might come to the decision largely on its own and then encourage its acceptance by citizens or it might act in response to pressure from below. It is highly desirable that the decision not be seen as belonging to one particular political party but is promoted by most if not all parties. The importance of a constitutional change which does away with a standing army needs to be emphasized. In its absence, there is every possibility that a military will reappear in the future.

After decades of military interference in politics and many military coups, Haiti's military was disbanded in 1995, following a referendum in which 62 percent of the population voted in favor of demilitarization. However, no change was made to the country's constitution and, in 2017, remilitarization began with the recruitment of armed forces to assist with development projects, disaster relief, and border security.[13]

A detailed plan

Whether demilitarization is rapid or more gradual, it needs to be based on a detailed plan which meets the needs of society and uses appropriate means. That is, the process of demilitarization itself must reflect the new mindset, and specifically the need to manage or resolve conflict nonviolently and democratically. The benefit of a gradual approach is that a careful plan can be worked out; the danger is that countervailing forces may gain strength and the opportunity to demilitarize may be lost. In any case, the aforementioned constitutional change must form part of the plan.

An implementing agency

A plan will achieve nothing if it is not effectively implemented. The range of the tasks involved in demilitarization, and their complexity, means that they cannot be left to various government departments, each influenced by their own ethos and agendas. An organizational structure needs to be established, perhaps a National Ministry of Peacebuilding, to plan and implement the tasks of demilitarization. Six countries have such a ministry—Costa Rica, the Solomon Islands, Nepal, Timor Leste, South Sudan and, since late 2018, Ethiopia—each set up under different circumstances and performing different functions. Of necessity, this would need to be a senior ministry, staffed by personnel committed to a demilitarized society.[14]

Financing demilitarization

It is tempting to think that demilitarization will result in an immediate "peace dividend" which is available for other government purposes. However, the experience of reduced military expenditure during the 1990s, when much of the saved expenditures went to reduce government budget deficits, shows that this is by no means certain. In addition, disarmament, demobilization, and reintegration (DDR) efforts that accompany demilitarization are complex and expensive

processes and need to be well planned and funded in order to avoid the real possibility that ex-combatants will recommence fighting or turn to crime and banditry. Where demobilization follows the end of a war, some sources of wartime finance, including war taxes and the cavalier exploitation of natural resources, may no longer be available. Almost certainly, foreign financial aid will need to be tapped. Cost categories include DDR, compensation of former soldiers (e.g., financial payouts, reintegration training), providing for alternative ways to achieve national security, and meeting secondary tasks previously performed by the military. These apply whether demilitarization follows the end of a war or during a time of peace. The main potential sources of finance include, on the domestic front, saved expenditures (the peace dividend), taxation, selling government bonds (borrowing from the public), and the sale of military assets, and, for the foreign side, development assistance (NGOs and bi- and multilateral aid) and as well borrowing from commercial banks (with private and/or public guarantees).

The first two cost categories can be considered as necessary investments in successful demilitarization. Disarming and demobilizing need to be well-organized (there are numerous examples of failures around the world) but are the lesser of the two in terms of cost and time. We focus here on the costs of compensating former soldiers for their loss of employment, which would have to at least meet the requirements of Lesotho's Labour Code Order, 1992. The Code allows for dismissals, among other reasons, "... based on the operational requirements of the undertaking, establishment or service" and stipulates a severance payment equivalent to two week's wages for each completed year of continuous service with the employer (see Table 3). A more generous approach would be to pay each of the LDF personnel the equivalent of, say, two years of their present gross income. The approximate total costs of these two approaches are M152 million (USD12.1 million) and M789 million (USD59.3 million), respectively. A combination of the two, to meet the Labour Code Order requirements, and pay one year of present gross income, would cost M546.2 million (USD41.1 million). Such amounts would probably need to come from foreign assistance.[15]

Table 3: Payouts to LDF personnel following disbandment

Scenario 1 (following Lesotho's Labour Code Order, 1992)
Assuming wages makes up 56.5 percent of annual military expenditure and that LDF personnel have an average length of service of 10 years, the total cost of a one-off payout would be M151.8 million (USD11.4 million).

Scenario 2 (paying two years' of present gross income to each LDF member)
Assuming wages makes up 56.5 percent of annual military expenditure, the total cost of a one-off payout would be M789 million (USD59.3 million).

Scenario 3 (following the Labour Code Order plus one year present gross income for each LDF member)
M151.8 million plus M394.4 million or M546.2 million total (USD41.1 million).

Linking demilitarization to poverty reduction

One of the arguments for demilitarizing Lesotho concerns the opportunity cost of military expenditure. It is a very poor country; almost 60 percent of its population live below the international poverty line of PPP$1.90/day. What follows is a proposal for a basic income grant (BIG) payable to all adult citizens and financed by the savings from demilitarization. The detail involved in applying a BIG to Lesotho is the subject of another article in preparation. Here we sketch out the broad picture.[16]

A BIG is "a modest amount of money paid unconditionally to individuals on a regular basis (for example, monthly). It is often called a universal basic income (UBI) because it is intended to be paid to all." Not fundamentally aimed at poverty reduction *per se*, but providing psychological security in a way that a means-tested, behavior-tested, or non-universal system of benefits cannot, BIG is about economic security for a population at large. It also promotes worthy objectives like social justice, freedom, and equality. Other potential benefits include its emancipatory value and the development of agency.[17]

Opponents of BIGs have four main concerns—that they are unaffordable, reduce incentives to work, result in wasteful expenditures, and reduce social cohesion and reciprocity—but recent reviews of the experience of BIG-type programs worldwide report very positive outcomes. In the words of one review, "[c]ash transfers have arguably the strongest existing evidence base among anti-poverty tools, with dozens of high-quality evaluations of cash transfer programs spanning Africa, Asia and Latin America." Its summary of this evidence is that cash transfers result in improved health and education, lead to higher incomes in the long term, and are not used on wasteful "temptation" expenditures.[18]

Two significant African BIG experiments (one completed, one ongoing) are worth noting. The first basic income pilot in a developing country was in a Namibian village of around 1,000 people in 2008–9. All village members, including children but not those over 60 already receiving a pension, received a monthly payment of some USD12, around a third of the poverty line. Before versus after comparisons indicated better nutrition and health, especially among children, higher

school attendance, a substantial decrease in petty economic crime (e.g., food theft), increased economic activity, lower expenditure on alcohol, and an enhanced status for women. Village members, on their own initiative, set up a committee to advise people on spending and saving matters.[19]

In November 2017, a long-term randomized control experiment began in Kenya. Villages were randomly assigned to a control group (whose residents received no cash transfers) or to one of three treatment groups. The first treatment group villages are receiving an amount equivalent to half the average income in rural Kenya each month for 12 years; the second are receiving the same amount for two years; and the third have received a single lump-sum payment equal to the two year basic income.[20]

An estimate of a possible BIG for all adults in Lesotho—using the expenditure currently allocated to the LDF—is summarized in Table 4. This is based on data from the 2010/11 Household Budget Survey, the most recent available, which estimated average household income for the poorest 94.3 percent of the population at M4,853 (USD346) per year. If the M698 million (USD53 million) currently spent on the military were allocated instead to all adults, assuming two adults per household, then the average household income of the poorest households would increase by M954 (USD68) per year, i.e., a gain of almost 20 percent.[21]

A considerable proportion of payouts to the LDF and to the BIG would flow back to government in the form of income- and value-added taxes. A rough estimate of the former, assuming a Scenario 3 payout (Table 3), is M137 million (USD10.3 million) from income taxes, plus M43 million (USD3.2 million) from VAT as one-off recoupments. Those individuals who pay income tax will also pay tax on their BIG receipts (M17.5 million; USD1.3 million), and all recipients will pay VAT to the extent that they spend their BIG payments (M71.4 million; USD5.4 million); both BIG-related taxes will be recouped each year.

Table 4: Financing a basic income grant for Lesotho

Military expenditure, 2017	M698 mn (USD53 mn)
Population, 2018	2.2 mn (1.46mn adults)
Households, 2010/11	426,000 (typically 2 adults, 3 children)
Household income, 2010/11	Overall average household income was M12,827/year (USD964), with 94.3% earning less than M36,000 (USD2,707)/year. For these households, average income was M4,853/year (USD365).
Basic income grant	M477/adult; M954/household (USD36/72), a 19.7% increase in average household income per year for 94.3% of households.

Conclusion

Two of the major challenges facing Lesotho are high levels of poverty and the negative involvement of the LDF in political matters. This article has argued the case for both, demilitarization and a basic income grant for all adult citizens and has shown how the latter can be financed from the savings made by demilitarizing. Both suggestions are subject to critiques, addressed earlier in the article.

Some important questions remain, particularly with respect to timing. Should demilitarization be imposed with the hope that its net benefits would become apparent to the population in the future or should a long-term process of building a culture of peace be undertaken in the hope that it would result in a future consensus to demilitarize? More specifically, would a 20 percent increase in average household income for 94 percent of households be enough to persuade them to support demilitarization? Would LDF personnel be happy with one or the other of the payout scenarios outlined in Table 3? And is government willing to make the decision to disband the LDF? However these questions are answered, the case for the demilitarization of Lesotho is clear to us.

Notes

1. Bendix and Stanley (2008).
2. Hill (2000); Harris (2004a, pp. 193–196).
3. Quoted in Peters (2013, p. 185).
4. Quoted in Harris (2008, pp.82–83).
5. On Costa Rica' story, see Harris (2004a).
6. UNDP (2018).
7. Letsie (2018).
8. Easily overwhelm: SIPRI (2018). LDF: The LDF does perform some functions unrelated to national security, e.g., disaster relief, assisting with medical emergencies, rural engineering works, and some policing functions. However, these occur to a very limited extent and almost certainly could be performed more cost effectively by more specialized government entities.
9. Quote from: Mothibe (1999, p. 48).
10. SIPRI (2018).
11. Taken into account: Harris (2004b).

12. UNESCO quote: Opening statement of the UNESCO Constitution. http://portal.unesco.org/en/ev.php-URL_ID=15244%26URL_DO=DO_TOPIC%26URL_SECTION=201.html [accessed 7 March 2019].

13. Haiti-FLASH (2017a; 2017b).

14. Ministry of Peacebuilding: See Harris (2019).

15. Labor code: See http://www.ilo.org/dyn/natlex/docs/WEBTEXT/31536/64865/E92LSO01.htm [accessed 7 March 2019].

16. International poverty line: UNDP (2016, p. 218).

17. See Standing (2017, pp. 65, 314; the quote is from p. 3).

18. Positive outcomes: McFarland (2017); Standing (2017). Quote: GiveDirectly (2018).

19. Haarmann, *et al.* (2009).

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The impact of terror attacks on global sectoral capital markets: An empirical study

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Abstract

This article examines the effects of fifteen major terror attacks perpetrated in the U.S. and Europe between 2001 and 2017 on a general global stock market index as well as on industry-specific indices, namely (1) airlines, (2) global hotels, restaurants, and leisure (hospitality), and (3) global utilities. Using an event-study method, we show that attacks tend to result in significant negative abnormal returns on the day of attack which, on occasion, persist for a few days. As expected, adverse market effects appear more pronounced, in terms of magnitude and persistence, for the global airline and hospitality industries than for the global utilities industry. Attacks in Europe since 2015 show no adverse global market effects, with two late exceptions (the London Bridge and Barcelona attacks, both in 2017). This might suggest that just when investors and markets seemed to have learned to cope with attacks, these two latter events caused some concern again. Implications of our findings for short- and long-term global investor strategy are discussed.

Economic costs of acts of terror can be grouped into three categories (Krugman, 2004). First is the direct damage caused on buildings, infrastructure, and on productive lives ended. Second are the budgetary cost of public sector responses to terror, such as increased amounts of monies spent on national defense and homeland security. And third is the cost imposed on the private sector by the way people and firms respond to the fear of future terror attacks. The empirical literature largely focuses on effects suffered within the attacked countries, even if some consideration is given to external effects. For example, Becker and Murphy (2001) document falling investment in the United States due to terror threats as approximately 0.2 percent of GDP, and they suggest that this then likely affects other economies through lower U.S. demand for imports. Blomberg, Hess, and Weerapana (2004) find that terror attacks reduce domestic economic growth to a smaller degree than when compared to the effects of internal conflict, external war, or natural disasters.

Given their prominence in the functioning of the economy, capital markets in the western world are likely to be prime recipients of adverse effects of terror. Campbell, Lo, and Craig (1997) argue that share prices and the evolution of market indices can be a good source of information regarding the economic impact of terror as they reflect both, companies' profit expectations and the likelihood, as seen by investors, of these expectations being fulfilled. Frey, Luechinger, and Stutzer (2004) make a similar point. Profit expectations may be revised downward due to the destruction of physical and intangible capital and also due to demand-reducing consumer fears. Relatedly, market risk premia increase when terror involves greater uncertainty about firms' prospects. Market assessment thus likely dictates subsequent actions of market agents, such as investors and analysts, and will eventually determine the way markets react to any specific event, including the time required for market recovery ("bounce back").

In this article we study the effects of terror attacks on international equity markets. Employing an event-study method, we assess whether fifteen major terror attacks that took place between 2001 and 2017 in the United States and Western Europe carried adverse effects on *global* stock markets. We pay particular attention to sub-indices in (1) the global hotel, restaurant, and leisure industry (hospitality), (2) the global airline industry, and (3) the global utilities industry. We look at global rather than domestic stock markets for two reasons. Following deregulation policies introduced in the late 1990s, capital markets became more globalized than before, with investors now holding internationally diversified portfolios to reduce nonsystematic risk as much as possible. But since that time, terror attacks also became more global in nature, including in OECD countries (IEP, 2016).

We provide answers to the following research questions. First, the "big picture": To what extent did specific attacks affect global stock markets (i.e., the global market index)? And was the effect, if any, permanent or transitory? Second, how did attacks affect specific industries at the global level?

Toward this, we dissect the global market index and examine two industries likely to be adversely affected (the hospitality and airline industries) and one unlikely to be negatively affected (utilities), or affected to a lesser degree. Again, for all three industries, we ask whether the effect, if any, was permanent or transitory. Third, in line with the extant literature, we ask how stock market responses to terror attacks may have changed over time.

Answers to these questions broaden the literature along two dimensions. First, to our knowledge, there has been no research on capital market effects related to relatively recent sets of terror attacks, including those in Western Europe between 2015–2017. Second, even though several papers have addressed the effect of terror attacks at the level of national capital markets, including at industry levels, none has looked at it from the point of view of international stock markets. Our work thus is useful to further update, assess, and measure the economic costs of terror generally and for international investors, specifically, who may be concerned about the negative effects that acts of terror may have on their investment portfolios and strategies.

The remainder of the article is organized as follows. The next section reviews existing literature and sets out the research framework. Following that, the data used is described along with the event-study methodology. The results section follows. The article concludes with a discussion of the main findings.

This article studies the magnitude and persistence of fifteen major terror attacks in the U.S. and Europe between 2001 and 2017 on global equity markets, specifically on the airline, hospitality, and utilities industries globally. As expected, the research finds more pronounced, if transitory, effects on the airline and hospitality industries and less on utilities and an overall diversified global equities index. It also finds that a wave of attacks in Europe since 2015 at first led to few adverse market reactions until two major attacks (in London and Barcelona) in 2017, suggesting that attacks can still rattle the markets. Implications for short- and long-term global investor strategies are discussed.

Literature review

We focus on two layers of literature related to the research theme of this article. The first is broad and considers the general reaction of stock markets to terror attacks; the second is focused and concentrates on the impact of terror attacks on specific industries.

Abadie and Gardeazabal (2003) used an event-study method to assess the firm-level impact of terror attacks in the Basque region of Spain, finding that shares of firms with a significant part of their business in that region showed positive relative performance when conditions of truce prevailed and a negative one when they did not. Chen and Siems (2004) studied the effect of 14 major terror/military attacks on U.S. capital and global markets for the period 1915–2001, paying particular attention to the effect of Iraq's invasion of Kuwait (1990) and to the 9/11 attack (2001). They found that, as compared to global markets, the U.S. capital markets became more resilient over time and recovered sooner. Johnston and Nedelescu (2005) examined market reactions to 9/11 and to the attack in Madrid in March 2004. Their main finding was that financial markets faced major disruptions as well as high levels of uncertainty, especially for the case of the 9/11 attacks in New York.

Nikkinen, *et al*. (2008) also focused on 9/11, examining its effect on 53 stock markets across the world. Their findings show increased volatility as well as short-run negative effects, which were eliminated quickly. Kollias, Papadamou, and Stagiannis (2011) investigated two major terror attacks in Europe—in March 2004 in Madrid and in July 2005 in London—on the stocks of different sectors, finding that whereas the Spanish market experienced significant negative returns across most sectors this was not the case for London.

More recently, Baumert, Buesa, and Lynch (2013) studied the effect of the Boston marathon bombing in 2013 on financial markets in Frankfurt, London, Madrid, Paris, Milan, New York, and Tokyo, comparing the effects with those of prior prominent attacks. The results show that the markets exhibited statistically significant negative abnormal returns on the day of the Boston attack but that the magnitude of these abnormal returns was lower than when compared to previous events.

Moving now to the second layer of literature, the tourism and airline industries have naturally received special attention due to their vulnerability to terror attacks. For example, Enders, Sandler, and Parise (1992) quantified the value of losses in tourism revenues for European countries and found that continental Europe lost USD16.145 billion due to such attacks for the period 1974–1988. Fleischer and Buccola (2002) examined hotel revenues in Israel, finding that from 1992 to 1998 annualized averages of monthly revenue losses from terror events in its foreign and local tourism markets were approximately USD48.6 and USD0.3 million, respectively.

Raby (2003) investigated sectoral effects more broadly and concludes that the airline, travel, tourism, accommodation, restaurant, postal, and insurance industries are particularly sensitive to increased terror risks. Madanoglu, Olsen, and Kwansa (2010) focused on the market value of hospitality and tourism firms as a result of three attacks, namely bombings in Bali (2002), Istanbul (2003), and Madrid (2004), in each case finding adverse market reactions. Gallego, Rossell, and Fourie

(2016) looked at the effect of terror, crime, and corruption on tourist arrivals for 171 countries for the period 1995–2013 and found that terror and crime exert a negative effect on tourist arrivals, but corruption did not.

As far as the airline industry is concerned, Drakos (2004) examined the effects of 9/11 on a set of airline stocks listed on various international stock markets and documented an increase in volatility following the attack. Additionally, he found that in the six months period prior to the attack, the markets considered airline stocks as defensive (low risk) while in the six months after the attack, they became aggressive stocks (high risk). Carter and Simkins (2004) found statistically significant abnormal price movements in the stocks of U.S and international airline carriers following 9/11. These, however, were not permanent (that is, mitigated in the days following the attack). Relatedly, Ito and Lee (2004) assessed the impact of 9/11 on U.S. airline demand and found that it resulted in both a negative transitory shock, exceeding 30 percent, and an ongoing negative demand shock amounting to approximately 7.4 percent of pre-9/11 demand volume. Brauer and Dunne (2012) studied the effects of large-scale natural and man-made catastrophes, such as epidemics, terror, and war on global air traffic for the world's largest 20 airlines. Their results suggest that global air traffic was not greatly affected by the general level of terror attacks worldwide as global airlines could change routes to fly to substitute tourist destinations. It took an exceptional event, such as 9/11, to cause a measurable impact on air traffic demand and, even then, the effect turned out to be relatively small in magnitude.

Data and methodology

Compiling a list of terror events deemed "significant," and hence likely to affect global markets, is somewhat arbitrary. According to the U.S. government's Incident Review Panel Criteria, a terror incident is considered significant "if it results in loss of life or serious injury of persons, major property damage, and/or is an act or attempted act that could reasonably be expected to create the conditions noted" (USDOS, 2003).

Authors who have addressed questions similar to ours have built their sample of events in a way that facilitates the examination of their research questions. For example, Johnston and Nedelescu (2005) studied two significant events, Chen and Siems (2004) studied 14 such events, and Baumert, Buesa, and Lynch (2013) focused on four significant events. We select fifteen events, providing a "platform" upon which to discuss the effect of terror attacks on international capital markets. In contrast to most other studies, ours uses the large number of 15 events and, even though some prior and non-European attacks are included, focuses on 11 recent attacks occurring in Europe over the period 2015–2017. The reason for our selection is that the research questions we address focus on possible effects at the international capital markets level where there is clearly a high level of integration between the European and U.S. markets. According to Nikkinen, *et al*. (2008), the impact of terror attacks varies across geographic regions, depending on the degree of their integration within the global economy; less integrated regions are less exposed. The events we use, along with some background information, are presented in Table A1 in the Appendix.

The hypotheses tested in this article refer to the effects, if any, that the listed terror attacks may have had on the MSCI World Index and three global sectoral indices, namely, MSCI World Hotel, Restaurant, and Leisure Index, MSCI World Airline Index, and MSCI Utilities Index.[1] The first index is a broad global equity index, representing large and mid-cap stocks across 23 countries. The sectoral indices are comprised of global stocks of firms within these sectors. All data are in daily frequency and were collected from DataStream. The indices were transformed into daily returns using the continuous compounding equation $R_{it} = ln\,(P_{it}/P_{it-1})$, where R_{it} is the daily return of index i, and P_{it} and P_{it-1} are the daily prices of index i at time t and $t–1$.

Methodologically, event studies examine the possible effects of one or more event(s) on the value of assets, such as stocks and bonds, commodities, and exchange rates. The method is based on the efficient-market hypothesis put forward by Fama (1970). It asserts that as new information arrives at the market, investors and analysts immediately and accurately assess its current and—more importantly—future impact. This (re)assessment results in prices changing to reflect the effect of this new information on the value of the future performance of the asset under consideration, in our case the four world market indices. Consequently, price changes can be attributed to specific events resulting from the release of this new information.

The event study method has been widely used to assess the impact of a wide range of events, such as earnings (Ball and Brown, 1968), announcements of mergers and acquisitions (Brown and Warner, 1980), regulatory changes (William, 1981), the effect of macroeconomic announcements on foreign exchange markets (Evans and Lyons, 2008), and actions related to corporate social responsibility (Katsikides, Markoulis, and Papaminas, 2016). Regarding terror attacks, papers with a scope like ours, such as Chen and Siems (2004), Johnston and Nedelescu (2005), Madanoglu, Olsen, and Kwansa (2010), and Baumert, Buesa, and Lynch (2013) all employ the event-study method to study the effect of such attacks on stock markets.

A general framework to carry out meaningful event analysis is provided by MacKinlay (1997) and Kothari and Warner (2007). The first step is to determine the event date. For the purposes of this article, this is defined as the day on which a specific terror attack took place. Following that, the estimation period and the event period need to be defined. According to MacKinlay (1997), the estimation period is the time period used to calculate the estimated return predicted by the market around the "announcement date" of the event. Here, we use a period of 90 trading days before the event date.

The event period is usually defined to be longer than the event date, or period of interest, so as to accommodate the examination of periods around the event and to capture possible effects of insider trading (if any) as well as the longer-term effects of the specific event. In the case of terror attacks, unfortunately they cannot be foreseen and, as such, we begin the analysis on the date of the event. Apart from the event date, we also use event windows of 5, 10, and 15 days thereafter. The reason for employing event windows is to assess how quickly the market absorbed (or failed to absorb) the event news. On the one hand, a possibility exists that on some occasions initial worries might persist (e.g., if not all event perpetrators were apprehended or killed, fears of further attacks may linger), hence keeping the market index down. On the other hand, it is possible that uncertainties might be quickly alleviated through the release of new information (e.g., government taking specific actions so that people feel safe) and thus causing market recovery.

To measure market reaction to the announcement of a terror attack, a normalized or expected return for each of the market indices we use needs to be estimated during the various event windows. This expected return must then be subtracted from the actual market return observed on the day of the event, and on subsequent days, in order to determine whether any abnormal return could be attributed to the event. Thus, $AR_{it} = R_{it} - E(R_{it})$, where the left-hand side term is the abnormal return of market index i at time t, R_{it} is as defined before, and $E(R_{it})$ is the expected return of market index i at time t.

An important issue concerns the estimation of $E(R_{it})$. We follow Chen and Siems (2004) and Baumert, Buease, and Lynch (2013) and compute it as $\frac{1}{90}\sum_{-90}^{-1} R_{it}$. The event date is set as $t = 0$, so that the expected return of market index i is estimated over 90 days, i.e., from $t = -90$ to $t = -1$, the last full trading day prior to the event.

Event day abnormal returns can be used to examine the *immediate* market reaction to an event. *Cumulative* abnormal returns (CAR) over the next few days or weeks can, however, provide a stronger and potentially more useful measure of the market's resilience and, importantly, its ability to "bounce back" from an attack. Therefore, once a time series of abnormal returns has been established, it would be important to test whether CAR are different from zero over the event windows that span after the event day. They can be estimated as $CAR_{t1,t2} = \sum_{t=t1}^{t2} AR_{it}$, where *t1* and *t2* denote the start and end of the event window, respectively. The null and alternative hypotheses then are H_0: CAR = 0 versus H_1: CAR ≠ 0.

We examine each of the fifteen events separately and therefore carry out standard *t*-tests for each event as well as for each event window within the specific event. The relevant *t*-statistic is $CAR_{t1,t2}/\left(\sigma^2_{t1,t2}\right)^{1/2}$, where the sigma term in parentheses equals $L\sigma^2(AR_t)$ and captures the variance of the one-period average abnormal return over the estimation window, and L is the number of days corresponding to each event window. Thus, the CAR will have a higher variance, the longer is L (i.e., the longer the event window).

The question we ask is whether the CAR of each of the four global market indices is statistically significantly different from zero on the day of the event ($t = 0$) and during the three subsequent event windows.

Results

In the main narrative here we present some results visually. Tables A2 to A5, in the Appendix, present the full numeric results, including the *t*-statistics needed to assess the statistical significance of the estimates.

Looking at the "big picture" first—the effect of the fifteen terror events on the MSCI Global Index—we make four observations. First, of the fifteen events only five affected the MSCI Global Index on the day of the event. They are: 9/11 (2001), Madrid (2004), Boston (2013), Orlando (2016), and Barcelona (2017). Second, when this is the case, no event—and that includes 9/11—negatively affected the index for more than 10 days. Effects are wholly transitory and not permanent. Actually, 9/11 and the Boston marathon bombing caused negative CAR up to the 5-day window (although of a very different magnitude, 10.5 percent versus 2.9 percent), and the Madrid train bombing caused a negative CAR in the 10-day window (–3.8 percent). The other two events (Orlando and Barcelona) caused abnormal returns only on the day of the event. Third, the magnitude of CAR diminished over time (see Figure 1). For example, the CAR caused by earlier events, except for the London bombing of 2005, were much higher in comparison to those caused by more recent events (if any). This leads to the fourth observation, which is that the relatively recent wave of terror attacks in Western Europe, which started with the Charlie Hebdo attack in Paris in January 2015 has not

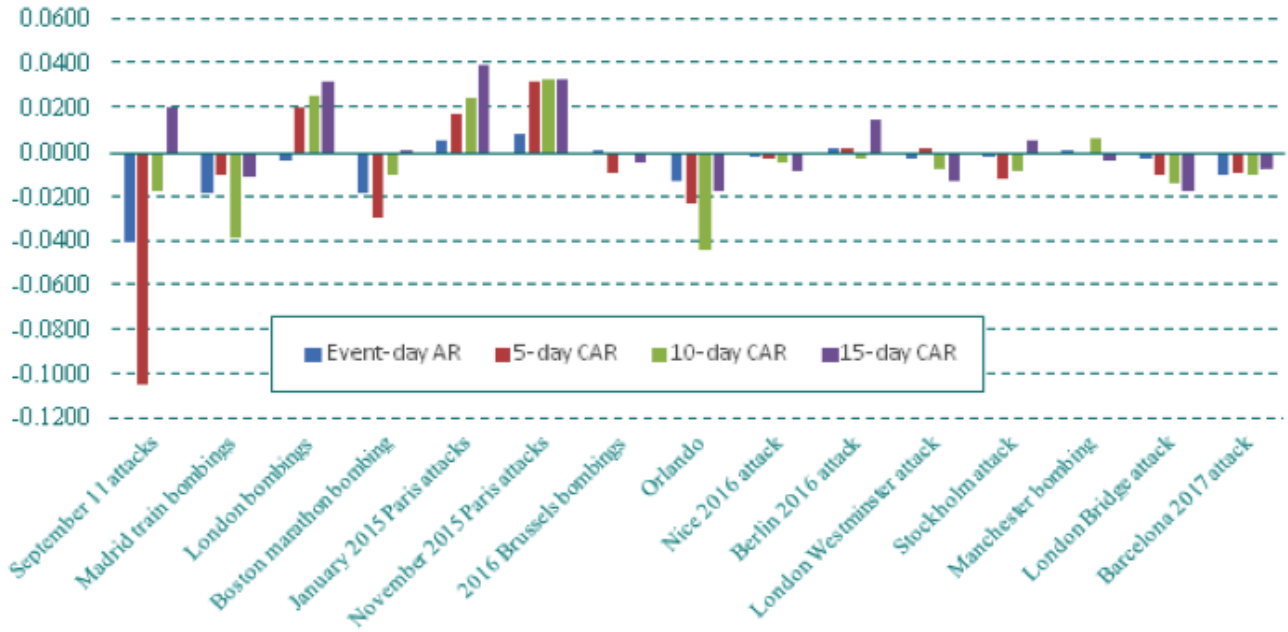


Figure 1: Event-day AR and 5,10, and 15-day event window CARs for MSCI Global Index for all 15 events.

resulted in any negative CAR for the global index, except for the August 2017 attack in Barcelona.

Our findings partly align with existing literature. For example, Chen and Siems (2004) documented significant negative short-term abnormal returns for U.S. and other markets for a number of significant terror events, Nikkinen, *et al*. (2008) found transitory negative effects on several stock markets due to 9/11, and Baumert, Buesa, and Lynch (2013) found adverse abnormal returns for major international markets on the day of the Boston bombings in 2013. Yet our findings also differ in some respects. For instance, not all of the 15 events we examined affect the global stock market index. More specifically, we found that almost all of the attacks since 2015 do not result in negative abnormal global market returns, not even on the day of the attack.

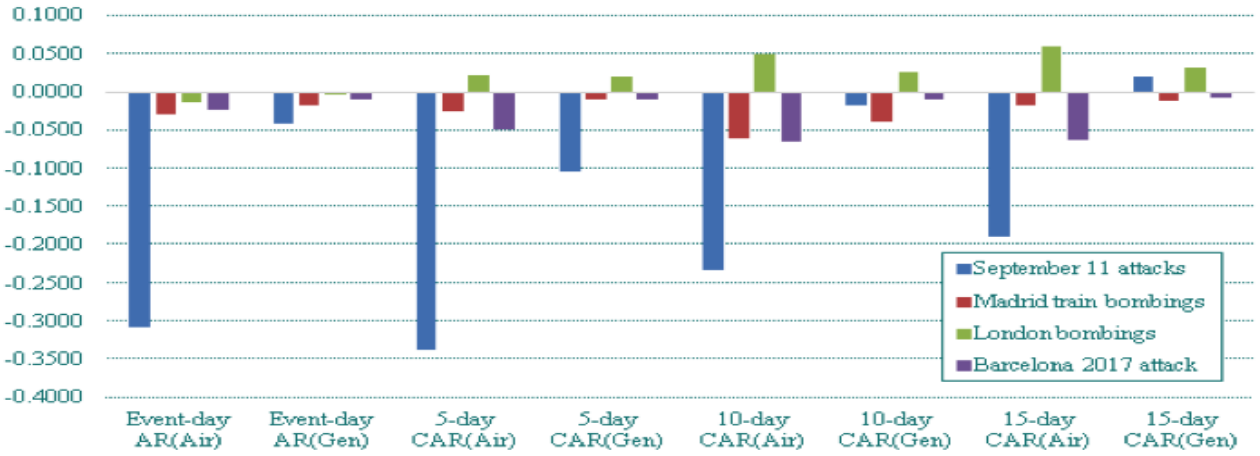
We next turn to the effect of the fifteen terror attacks on our sectoral global MSCI indices. The first industry we focus on is the airline industry. Our findings indicate that only four of the 15 events examined affected the MSCI Global Airline index: The 9/11 (2001), Madrid (2014), London (2005), and Barcelona (2017) attacks. When an adverse effect is noted, its magnitude was larger than that observed for the MSCI Global index (see Figure 2, Panel A).

In terms of persistence, the event that stands out is 9/11, where the CARs are highly statistically significant during all event windows. According to our calculations, CARs relating to the global airline index continued to be negative and statistically significant 37 days after the event. Apart from 9/11, the effect of all other events appears to be rather transitory. For example, the Madrid train bombings of 2004 resulted in negative CARs up to the 10-day event window, while the Barcelona attacks in 2017 produced negative CARs up to the 15-day event window. The London bombings of 2005 also generated negative event day AR, followed by positive CAR up to the 15-day event window. This suggests that, in this case, the market exhibited a rebound after the event.

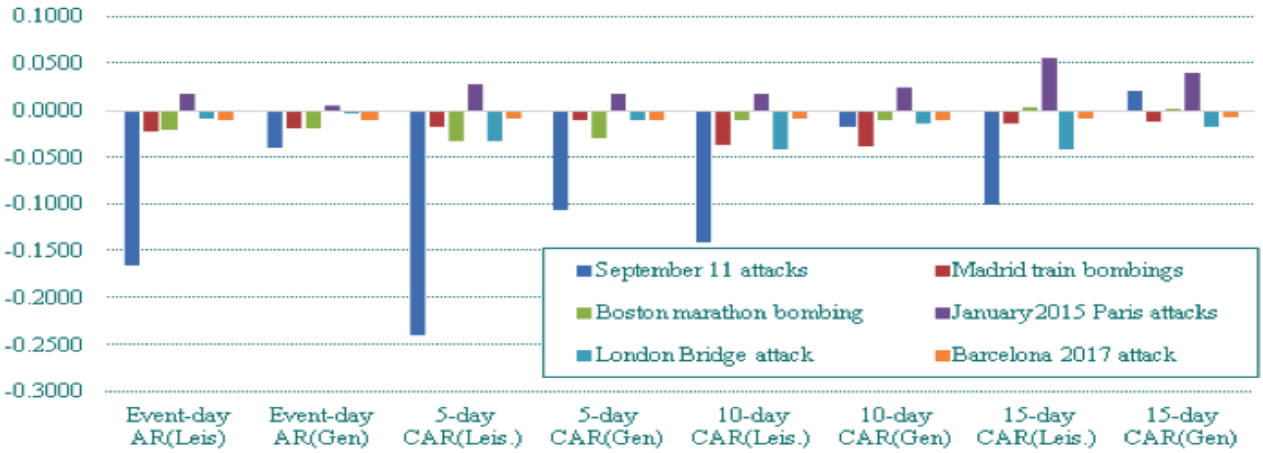
As for the case of the global index, the pre-2015 attacks affected the airline industry index substantially more than when compared to the post-2015 attacks. Along the same lines, the attacks that occurred in Western Europe between 2015 and 2017 have not affected the global airline index, again except for the Barcelona (2017) attack.

Existing literature on the effect of terror attacks on the airline industry mostly focuses on airline demand. For example, Ito and Lee (2004) documented a large negative transitory effect followed by a smaller permanent one and Brauer and Dunne (2012) documented that, with the exception of 9/11, global air traffic for the top-20 air carriers was not greatly affected by the general level of terror attacks worldwide. Our results augment these findings from the angle of global stock markets in the spirit of Brauer and Dunne (2012), in the sense that most of the events did not appear to cause significant negative abnormal returns and even when they did, the only one that exhibited permanent characteristics

Panel (A): Event day and 5-,10-, and 15-day event window CARs for MSCI global airline index.



Panel (B): Event day and 5-,10-, and 15-day event window CARs for MSCI hotels, restaurant, and leisure index and MSCI global index



Panel (C): Event day and 5- and 10-day event window CARs for MSCI utilities, airlines and hotels, restaurant, and leisure indices for 9/11 and 2004 Madrid bombings

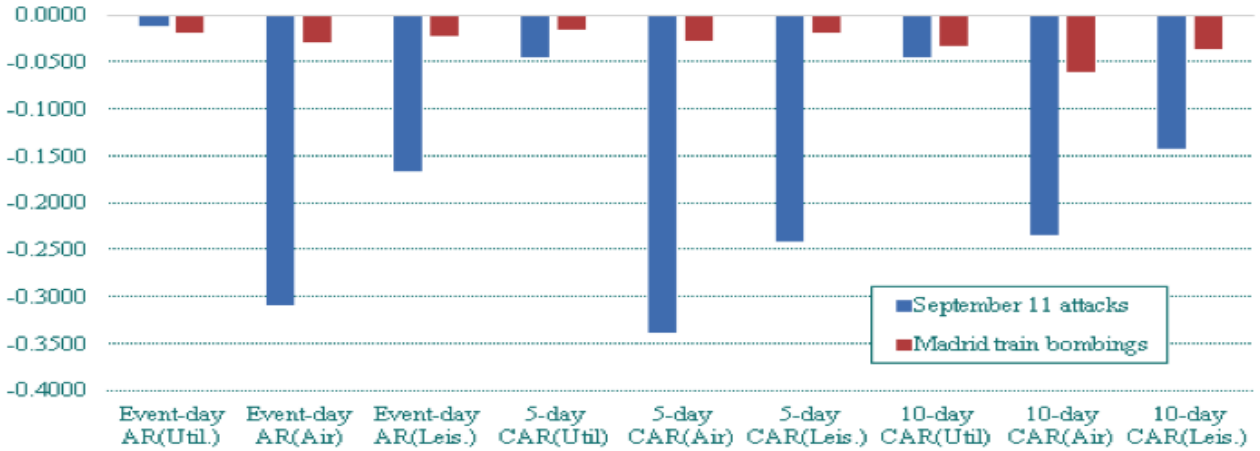


Figure 2: Event day and CARs windows.

was 9/11. We should also note that our results enrich those of Carter and Simkins (2004) who found rather transitory effects on stocks of airline companies as a result of 9/11.

Now turn to the effect of the attacks on the MSCI World Hotel, Restaurants, and Leisure index. Our results indicate that of our fifteen events, five had a negative effect on the industry (9/11, Madrid 2004, Boston 2013, London Bridge 2017, Barcelona 2017) and one, the Charlie Hebdo attack in Paris in 2015, a positive one. Once more, when there was an effect on the industry, it appeared to be larger in magnitude in comparison to the MSCI Global Index (see Figure 2, Panel B).

As far as the persistence of the market effect of the events is concerned, two events stand out, 9/11 and the London Bridge attack, both producing significant negative CAR up to and including the 15-day event window. Apart from these two, the Madrid bombing of 2004 produced negative CAR up to the 10-day event window, the Boston marathon bombing of 2013 up to the 5-day window, and the Barcelona attack produced abnormal returns on the day of the event only. The behavior of the hospitality index after the 2015 Charlie Hebdo attack in Paris also merits a comment since it might be reflecting the "we are not changing our way of life" mood prevalent at the time.

As mentioned, existing work has focused on the effect of terror attacks on tourist arrivals (e.g., Gallego, Rossell, and Fourie, 2016, among others). Once more, our work enriches current findings as it offers evidence that, from an international equity market perspective, only a few attacks result in negative abnormal returns and, of those, only two exhibit persistence.

Regarding global utilities index—low risk equities from the perspective of global investors—our results indicate that only two of the fifteen events caused any effect, namely 9/11 and the 2004 Madrid train bombing, and both were substantially weaker than for the airline and hospitality industries. Figure 2, Panel C graphs the effects for the two relevant events. In terms of persistence, both resulted in modest negative CAR up to the 10-day event window, after which the effect disappeared.

Discussion and conclusion

The analysis provides useful insights regarding the research questions addressed in this article. In terms of the MSCI global index, most of the pre-2015 attacks studied (plus the Barcelona 2017 attack) resulted in but transitory adverse effects on the index, and none beyond the 10-day event window. An investor with a global index buy-and-hold strategy is not likely to suffer financially from terror attacks of the nature studied here.

Sector-specific investors, however, may need to draw more differentiated conclusions as the global airline and hospitality indices reacted differently to terror attacks—in magnitude and in persistence—than did the overall global index. (Utility stocks, as we saw, hardly reacted at all.) Even here, of the fifteen attacks only four adversely affected the airline index and only five the hospitality index. Moreover, it takes an event as dramatic as that of 9/11 to cause some degree of persistency.

For general global investors, a diversified portfolio across industries makes sense, offering some investment protection in the face of terror attacks. Other investors, however, may take positions depending on the apparent effects different attacks exert in terms of magnitude and persistence across global industries. For example, it may make sense to take short positions in industries likely to be negatively affected in the short-term and long positions in those likely to be financially "immune" to terror attacks. An investor might also try to time the market after an attack, waiting for prices first to fall and then recover, knowing that index declines are unlikely to persist. Given that today's markets move much faster than in the past, it would also be interesting to observe intraday price movements (as, e.g., in Baumert, 2009), especially in relation to the post-2015 attacks.

Our results indicate that, for all equity indices examined, pre-2015 terror attacks, such as 9/11 or the Madrid or Boston bombings, generated negative returns. Nonetheless, the effects differed depending on the magnitude of the event and the specific industry considered. For example, 9/11, the biggest attack in magnitude (i.e., many more casualties, higher direct and indirect costs), naturally had a more profound effect on all indices examined. But attack "bigness" *per se* does not always correlate to the effects on the markets. For example, the Paris (November 2015) and Nice (July 2016) attacks caused many more deaths than the Boston marathon attack (2003), yet the global market reaction was distinctly different.

Why do markets react differently? One possible explanation is that, over time, international investors have learned to more quickly assess the "true" economic and financial consequences of terror attacks. They know that markets do tend to "bounce back" relatively quickly, and this holds even for attacks as big as 9/11. A second possible explanation is related to a theory advanced by Abadie and Gardeazabal (2003) and Eldor and Melnick (2004) who argue that market effects depend on investor perceptions regarding the persistence of the terrorist phenomenon *per se* and not on single attacks by themselves, whatever their magnitude may be. Our findings suggest that international investors may view single attacks as "one-off" events (at least up to the Manchester bombing) and not likely to recur. This is perhaps the reason that the vast majority of the wave of attacks on Western Europe between 2015–2017 did not seem to cause any significant negative market effects. On a cautionary note, however, note

that our findings show that the attack in Barcelona in 2017 had a negative effect on the MSCI global index, as well as on the airline and hospitality indices, and that the London Bridge attack negatively affected the hospitality index which, moreover, was relatively persistent. In the wake of an increasing string of attacks, it is also possible that market participants may be reevaluating their reaction to such events. All in all, the relation of financial markets to terror attacks is certainly an interesting field for future research even as we hope, of course, that the number of attacks will decline in time.

Notes

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1. MSCI (Morgan Stanley Capital International) is a provider of financial markets information. It compiles influential indices tracked by fund managers worldwide. These cover thousands of stocks under various categories and are used as benchmarks to measure the performance of investment portfolios.

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TableA1: Terror events and background information

Event no.	*Country*	*Event name or location/s*	*Event date dd/mm/yy*	*Period of interest dd/mm/yy*	*Perpetrator group*	*Casualties (deaths)*
1	USA	43353	37203	08/05/01–05/10/01	Al-Qaida	7,365 (2,997)
2	Spain	Madrid train bombing	11 Mar 2004	06/11/03–31/03/04	Abu Hafs al Masri Brigades	>1,800 (191)
3	UK	London "7/7" bombing	7 Jul 2005	03/03/05–27/07/05	Al-Qaida	784 (56)
4	USA	Boston marathon	15 Apr 2013	10/12/12–03/05/13	[Individual/s]	264 (3)
5	France	Charlie Hebdo, Paris	7 Jan 2015	03/09/14–27/01/15	Al-Qaida	23 (12)
6	France	Stade de France, Paris	13 Nov 2015	13/07/14–04/12/15	ISIS	423 (137)
7	Belgium	Brussels airport and metro station bombing	22 Mar 2016	17/11/15–11/04/16	ISIS	270 (35)
8	USA	Orlando night club shooting	12 Jun 2016	08/02/16–01/07/16	Jihadi-inspired	107 (49)
9	France	Nice truck attack	14 Jul 2016	11/03/16–04/08/16	ISIS (claimed)	433 (87)
10	Germany	Berlin Christmas market truck attack	19 Dec 2016	15/08/16–06/01/17	ISIS	48 (13)
11	UK	Westminster car attack	22 Mar 2017	16/11/16–11/04/17	[Individual/s]	50 (6)
12	Sweden	Stockholm truck attack	7 Apr 2017	02/12/16–27/4/17	ISIS-inspired	20 (5)
13	UK	Manchester concert bombing	42877	17/01/17–12/06/17	[Individual/s]	512 (23)
14	UK	London Bridge vehicle ramming and stabbing	3 Jun 2017	30/01/17–23/06/17	ISIS	56 (8)
15	Spain	Barcelona van attack	17 Aug 2017	13/04/17–06/09/17	ISIS (claimed)	>100 (16+8)

Sources: Data for the first seven events taken from Global Terrorism Database. For the other events, data collected from various media sources.

Table A2: Event day AR and Event window CAR for the MSCI Global Index

Country	*Event*	*Event day AR*	*5-day CAR*	*10-day CAR*	*15-day CAR*
USA	September 11 attacks	-0.0402	-0.1050	-0.0175	0.0203
	SE	0.0078	0.0174	0.0245	0.0301
	t-statistic	-5.1816***	-6.0524***	-0.7121	0.6769
Spain	Madrid train bombings	-0.0182	-0.0103	-0.0381	-0.0113
	SE	0.0053	0.0119	0.0168	0.0206
	t-statistic	-3.4138***	-0.8620	-2.2654**	-0.5474
UK	London bombings	-0.0039	0.0198	0.0253	0.0326
	SE	0.0049	0.0110	0.0155	0.0190
	t-statistic	-0.8016	1.8004*	1.6302	1.7119
USA	Boston marathon bombing	-0.0187	-0.0293	-0.0100	0.0013
	SE	0.0055	0.0124	0.0175	0.0215
	t-statistic	-3.3751***	-2.3603**	-0.5709	0.0619
France	Jan. 2015 Paris attacks	0.0053	0.0173	0.0246	0.0396
	SE	0.0068	0.0152	0.0216	0.0264
	t-statistic	0.7831	1.1368	1.1398	1.4997
France	Nov. 2015 Paris attacks	0.0085	0.0318	0.0332	0.0329
	SE	0.0102	0.0227	0.0321	0.0393
	t-statistic	0.8374	1.4022	1.0358	0.8372
Belgium	2016 Brussels bombings	0.0001	-0.0090	-0.0002	-0.0047
	SE	0.0097	0.0217	0.0306	0.0375
	t-statistic	0.0102	-0.4163	-0.0063	-0.1240
USA	Orlando	-0.0128	-0.0228	-0.0443	-0.0178
	SE	0.0074	0.0165	0.0233	0.0285
	t-statistic	-1.7418*	-1.3840	-1.9054	-0.6235
France	Nice 2016 attack	-0.0023	-0.0023	-0.0046	-0.0081
	SE	0.0094	0.0210	0.0297	0.0364
	t-statistic	-0.2421	-0.1093	-0.1545	-0.2227
Germany	Berlin 2016 attack	0.0022	0.0019	-0.0032	0.0143
	SE	0.0053	0.0119	0.0168	0.0206
	t-statistic	0.4136	0.1611	-0.1899	0.6915
UK	London Westminster attack	-0.0029	0.0022	-0.0071	-0.0128
	SE	0.0038	0.0086	0.0121	0.0149
	t-statistic	-0.7663	0.2573	-0.5840	-0.8592
Sweden	Stockholm attack	-0.0015	-0.0116	-0.0086	0.0056
	SE	0.0037	0.0083	0.0117	0.0144
	t-statistic	-0.3966	-1.4011	-0.7319	0.3893
UK	Manchester bombing	0.0002	-0.0001	0.0067	-0.0037
	SE	0.0041	0.0091	0.0128	0.0157
	t-statistic	0.0500	-0.0108	0.5204	-0.2381
UK	London Bridge attack	-0.0026	-0.0104	-0.0140	-0.0170
	SE	0.0040	0.0089	0.0126	0.0154
	t-statistic	-0.6514	-1.1670	-1.1188	-1.1083
Spain	Barcelona 2017 attack	-0.0098	-0.0094	-0.0098	-0.0071
	SE	0.0043	0.0095	0.0135	0.0165
	t-statistic	-2.2945**	-0.9833	-0.7312	-0.4334

Note 1: ***: significance at the 1%; **: significance at the 5% level; *: significance at the 10% level.
Note 2: Table presents *t*-tests for the event day and event windows of 5, 10, and 15 days. (H0: CAR=0; H1: CAR≠0); estimates of the standard error of AR and CAR and *p*-values in italics.

Table A3: Event day AR and Event window CAR for the MSCI Global Airline Index

Country	*Event*	*Event day AR*	*5-day CAR*	*10-day CAR*	*15-day CAR*
USA	September 11 attacks	-0.3083	-0.3375	-0.2335	-0.1895
	SE	0.0096	0.0215	0.0304	0.0372
	t-statistic	-32.0789***	-15.7058***	-7.6828***	-5.0908***
Spain	Madrid train bombings	-0.0292	-0.0258	-0.0605	-0.0166
	SE	0.0114	0.0255	0.0361	0.0442
	t-statistic	-2.5581***	-1.0108	-1.6759*	-0.3762
UK	London bombings	-0.0135	0.0219	0.0494	0.0598
	SE	0.0064	0.0143	0.0203	0.0248
	t-statistic	-2.1044**	1.5282	2.4390**	2.4090**
USA	Boston marathon bombing	-0.0122	0.0003	0.0373	0.0612
	SE	0.0080	0.0180	0.0254	0.0311
	t-statistic	-1.5156	0.0172	1.4696	1.9655*
France	Jan. 2015 Paris attacks	-0.0018	-0.0013	0.0050	0.0427
	SE	0.0134	0.0299	0.0423	0.0518
	t-statistic	-0.1382	-0.0432	0.1176	0.8235
France	Nov. 2015 Paris attacks	-0.0190	-0.0128	-0.0488	-0.0402
	SE	0.0117	0.0262	0.0370	0.0454
	t-statistic	-1.6183	-0.4888	-1.3171	-0.8869
Belgium	2016 Brussels bombings	-0.0118	-0.0282	-0.0349	-0.0494
	SE	0.0127	0.0283	0.0401	0.0491
	t-statistic	-0.9281	-0.9937	-0.8708	-1.0062
USA	Orlando	-0.0054	-0.0050	-0.0357	-0.0266
	SE	0.0082	0.0183	0.0259	0.0317
	t-statistic	-0.6568	-0.2741	-1.3780	-0.8375
France	Nice 2016 attack	-0.0056	-0.0455	-0.0313	-0.0620
	SE	0.0192	0.0428	0.0606	0.0742
	t-statistic	-0.2933	-1.0630	-0.5164	-0.8364
Germany	Berlin 2016 attack	-0.0046	-0.0062	-0.0152	-0.0042
	SE	0.0061	0.0136	0.0192	0.0236
	t-statistic	-0.7598	-0.4566	-0.7917	-0.1786
UK	London Westminster attack	-0.0033	0.0182	-0.0046	0.0106
	SE	0.0089	0.0199	0.0282	0.0345
	t-statistic	-0.3654	0.9159	-0.1635	0.3078
Sweden	Stockholm attack	-0.0020	-0.0014	0.0220	0.0263
	SE	0.0088	0.0197	0.0279	0.0342
	t-statistic	-0.2230	-0.0713	0.7887	0.7687
UK	Manchester bombing	0.0013	0.0190	0.0297	0.0178
	SE	0.0092	0.0205	0.0290	0.0355
	t-statistic	0.1396	0.9268	1.0223	0.5012
UK	London Bridge attack	-0.0027	-0.0079	-0.0141	-0.0143
	SE	0.0091	0.0203	0.0288	0.0352
	t-statistic	-0.3002	-0.3865	-0.4890	-0.4069
Spain	Barcelona 2017 attack	-0.0236	-0.0494	-0.0656	-0.0634
	SE	0.0087	0.0194	0.0275	0.0337
	t-statistic	-2.7105***	-2.5380**	-2.3852**	-1.8823*

Note 1: ***: significance at the 1%; **: significance at the 5% level; *: significance at the 10% level.
Note 2: Table presents *t*-tests for the event day and event windows of 5, 10, and 15 days. (H0: CAR=0; H1: CAR≠0); estimates of the standard error of AR and CAR and *p*-values in italics.

Table A4: Event day AR and Event window CAR for the MSCI Global Hotels, Restaurants, and Leisure Index

Country	*Event*	*Event day AR*	*5-day CAR*	*10-day CAR*	*15-day CAR*
USA	September 11 attacks	-0.165340	-0.240642	-0.141443	-0.101086
	SE	0.007844	0.017540	0.024805	0.030380
	t-statistic	-21.078631***	-13.719860***	-5.702242***	-3.327434***
Spain	Madrid train bombings	-0.021602	-0.017879	-0.036001	-0.014273
	SE	0.006459	0.014442	0.020424	0.025014
	t-statistic	-3.344722***	-1.237994	-1.762716*	-0.570588
UK	London bombings	-0.005848	0.021748	0.030513	0.022527
	SE	0.006332	0.014159	0.020024	0.024525
	t-statistic	-0.923552	1.535962	1.523789	0.918547
USA	Boston marathon bombing	-0.021093	-0.033071	-0.009397	0.004257
	SE	0.005949	0.013303	0.018813	0.023042
	t-statistic	-3.545507***	-2.485964**	-0.499474	0.184757
France	Jan. 2015 Paris attacks	0.018378	0.028429	0.017407	0.056698
	SE	0.007593	0.016979	0.024011	0.029408
	t-statistic	2.420315***	1.674400*	0.724937	1.927986**
France	Nov. 2015 Paris attacks	-0.001955	0.018827	0.020133	0.032155
	SE	0.011571	0.025873	0.036590	0.044814
	t-statistic	-0.168981	0.727681	0.550215	0.717518
Belgium	2016 Brussels bombings	-0.005460	-0.012351	0.007665	0.001657
	SE	0.011199	0.025042	0.035415	0.043375
	t-statistic	-0.487531	-0.493206	0.216441	0.038191
USA	Orlando	-0.005383	-0.005024	-0.035718	-0.026586
	SE	0.008196	0.018328	0.025919	0.031745
	t-statistic	-0.656790	-0.274106	-1.378030	-0.837504
France	Nice 2016 attack	-0.007409	0.006806	0.006954	-0.007995
	SE	0.010086	0.022553	0.031895	0.039063
	t-statistic	-0.734611	0.301761	0.218042	-0.204672
Germany	Berlin 2016 attack	-0.004621	-0.006211	-0.015227	-0.004208
	SE	0.006082	0.013601	0.019235	0.023557
	t-statistic	-0.759763	-0.456629	-0.791665	-0.178648
UK	London Westminster attack	0.000964	0.017266	0.012635	0.011805
	SE	0.004462	0.009976	0.014109	0.017280
	t-statistic	0.215964	1.730727*	0.895510	0.683196
Sweden	Stockholm attack	-0.003588	-0.003592	0.010015	0.037200
	SE	0.004329	0.009679	0.013688	0.016764
	t-statistic	-0.829026	-0.371148	0.731654	2.219009**
UK	Manchester bombing	-0.002710	0.006566	0.019756	-0.013927
	SE	0.004297	0.009608	0.013588	0.016641
	t-statistic	-0.630670	0.683426	1.453951	-0.836912
UK	London Bridge attack	-0.007929	-0.032767	-0.036420	-0.041727
	SE	0.004376	0.009785	0.013837	0.016947
	t-statistic	-1.812073*	-3.348858***	-2.631972**	-2.462174**
Spain	Barcelona 2017 attack	-0.009669	-0.008574	-0.008628	-0.008538
	SE	0.005789	0.012944	0.018305	0.022419
	t-statistic	-1.670337*	-0.662366	-0.471315	-0.380830

Note 1: ***: significance at the 1%; **: significance at the 5% level; *: significance at the 10% level.
Note 2: Table presents *t*-tests for the event day and event windows of 5, 10, and 15 days. (H0: CAR=0; H1: CAR≠0); estimates of the standard error of AR and CAR and *p*-values in italics.

Table A5: Event day AR and Event window CAR for the MSCI Global Utilities Index

Country	*Event*	*Event day AR*	*5-day CAR*	*10-day CAR*	*15-day CAR*
USA	September 11 attacks	-0.010903	-0.044444	-0.043904	0.008101
	SE	0.006894	0.015415	0.021800	0.026699
	t-statistic	-1.581535	-2.883246***	-2.013985**	0.303429
Spain	Madrid train bombings	-0.018298	-0.014475	-0.032453	-0.016221
	SE	0.005108	0.011423	0.016154	0.019785
	t-statistic	-3.581896***	-1.267248	-2.008921**	-0.819874
UK	London bombings	-0.003208	0.014720	0.004837	0.011175
	SE	0.005391	0.012054	0.017047	0.020879
	t-statistic	-0.595080	1.221169	0.283744	0.535216
USA	Boston marathon bombing	-0.004828	-0.011341	-0.003765	0.001451
	SE	0.005794	0.012956	0.018323	0.022441
	t-statistic	-0.833285	-0.875334	-0.205480	0.064669
France	Jan. 2015 Paris attacks	0.003070	0.009295	0.030325	0.047180
	SE	0.007430	0.016615	0.023496	0.028777
	t-statistic	0.413186	0.559454	1.290613	1.639493
France	Nov. 2015 Paris attacks	0.010465	0.023101	0.011387	0.008734
	SE	0.009190	0.020549	0.029061	0.035592
	t-statistic	1.138774	1.124169	0.391830	0.245385
Belgium	2016 Brussels bombings	-0.004624	-0.003378	0.006057	-0.006637
	SE	0.008221	0.018382	0.025996	0.031839
	t-statistic	-0.562488	-0.183787	0.232995	-0.208469
USA	Orlando	-0.008175	-0.008611	-0.030863	0.019142
	SE	0.007040	0.015741	0.022262	0.027265
	t-statistic	-1.161204	-0.547034	-1.386380	0.702086
France	Nice 2016 attack	0.000358	0.000617	-0.006877	-0.018645
	SE	0.007804	0.017450	0.024678	0.030224
	t-statistic	0.045933	0.035359	-0.278687	-0.616894
Germany	Berlin 2016 attack	0.005843	0.010371	0.015893	0.022689
	SE	0.009318	0.020835	0.029465	0.036088
	t-statistic	0.627092	0.497762	0.539389	0.628721
UK	London Westminster attack	0.001342	0.003999	-0.006186	-0.008999
	SE	0.006629	0.014822	0.020961	0.025672
	t-statistic	0.202443	0.269780	-0.295120	-0.350551
Sweden	Stockholm attack	-0.004026	-0.005290	-0.019900	-0.015154
	SE	0.005572	0.012460	0.017621	0.021581
	t-statistic	-0.722556	-0.424557	-1.129350	-0.702205
UK	Manchester bombing	0.003370	0.005228	0.010948	-0.003878
	SE	0.005098	0.011399	0.016120	0.019743
	t-statistic	0.661091	0.458609	0.679132	-0.196414
UK	London Bridge attack	-0.004248	-0.014436	-0.010612	-0.031095
	SE	0.004926	0.011014	0.015577	0.019077
	t-statistic	-0.862415	-1.310685	-0.681305	-1.629959
Spain	Barcelona 2017 attack	-0.003161	0.000240	-0.002341	-0.004681
	SE	0.004561	0.010198	0.014422	0.017663
	t-statistic	-0.693137	0.023539	-0.162354	-0.265026

Note 1: ***: significance at the 1%; **: significance at the 5% level; *: significance at the 10% level.
Note 2: Table presents *t*-tests for the event day and event windows of 5, 10, and 15 days. (H0: CAR=0; H1: CAR≠0); estimates of the standard error of AR and CAR and *p*-values in italics.

Participation in the international coalition against Daesh and the rise of foreign fighters

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Abstract

The flow of foreign fighters leaving for Iraq and the Syrian Arab Republic has slowed, but they often still pose a serious threat, either by encouraging others toward violence or by directly assisting themselves in a terrorist attack after their return. This article studies the effect of a country's active involvement in a conflict zone on the flow of foreign fighters. Specifically, we test whether a nation's participation in the international coalition against Daesh influences its number of foreign fighters. Despite the small sample size resulting from limited official data on foreign fighters, we report several interesting insights for cautious interpretation and only regarding the countries included. Findings from a negative binomial model suggest that a country's active international role against Daesh also increases the foreign fighters coming from that country. Hence, it is important to keep in mind that the cost of a military intervention can be higher than the cost of the operation itself. Policymakers should also account for the cost of the increased number of foreign fighters and the resulting threat.

In the wake of the Arab Spring of 2011, the European Counter-Terrorism Coordinator noted that "a significant number of radicalized people travel from the EU to conflict areas ... and pose a clear threat to internal security." By the end of 2015, the number of foreign fighters in Iraq and Syria was estimated at between 27,000 and 31,000 people.[1]

Since then the number of foreign fighters leaving for Iraq and the Syrian Arab Republic has slowed, but they still pose a serious threat in at least two ways. First, while abroad they can assist and encourage others to execute attacks in their country of residence. Second, they can be directly involved in terrorist attacks when they return. The attack on the Jewish Museum in Brussels in 2014, but also the attacks in Paris in 2015 and those in Brussels in 2016 constitute painful anecdotic evidence of this. Recent estimates suggest that about 30 percent of 5,000 European foreign fighters have returned home. Moreover, returning foreign fighters are more effective terrorists than non-veterans. While abroad, they have often built a social network and gained experience on the battlefield. As Braithwaite and Chu note, foreign fighters present an important form of trained human capital to perform attacks at home. Their study shows that having a significant number of foreign fighters abroad increases the likelihood of terrorism in the home country, at least when the conflict is won by the rebels.[2]

To respond appropriately to this threat, a thorough understanding of the triggers for foreign fighters to leave their country of residence is necessary. Official data on the topic is limited but clearly shows that the number of foreign fighters is not simply related to a country's population (nor to the composition thereof). Recent literature identifies multiple variables that play a role in explaining the flow of foreign fighters. Among others, a country's economic prosperity and population are believed to significantly affect the number of foreign fighters, as are the percentage of Muslims and the distance to Syria.[3]

In this article we test whether military counter-terrorism policy, expressed by a partnership in the international coalition against Daesh, has an impact on the number of foreign fighters leaving the country. While data availability limits the scope of our tests, we are able to include from 24 to 49 countries, depending on model specifications. Including the usual set of control variables in our negative binomial model shows that, among the countries included, coalition members have substantially higher numbers of foreign fighters leaving to fight in Iraq and Syria as compared to noncoalition countries. This finding has important policy implications since it suggests that military interventions in Iraq and Syria have backlash effects in the home country. The additional costs need to be accounted for when deciding whether to intervene in the conflict area. When a decision to intervene is made, it needs to be complemented with considerable preventive policies as well.[4]

The article is organized as follows. First we review the relevant literature on radicalization as the literature on foreign fighters *per se* is rather scarce. In addition, the section develops

the research question. Subsequently, the data collection process and the descriptive statistics are presented. This is followed by a description of the model to be estimated and a discussion of the main results. The final section concludes.

This article reports that countries that have joined the *Global Coalition Against Daesh* (also known as IS, ISIS, or ISIL) experience statistically significant increases of their citizens joining as foreign fighters for Daesh, and also posing a risk of backlash terror attacks for the sending country. The policy implication is that in addition to explicit budgetary costs, policymakers in sending countries should take into account the eventual cost of possible backlash terror attacks in their home countries.

Theoretical framework and research question

The literature on radicalization[5]

A review of the literature on terrorism reveals that the answer to how and why an individual engages in terrorism often boils down to finding the reasons how and why someone radicalizes. Radicalization can indeed be a pathway to terrorism but is neither a necessary nor a sufficient condition. Many individuals with radical ideas never turn to terrorism and foreign fighter groups in Iraq or Syria also contain terrorists who are not driven by a radical belief, but are rather motivated by the opportunity to escape a life seen as meaningless. A universal terrorist profile does not exist: Terrorists' motives and roles are heterogeneous.[6]

To understand the flow of foreign fighters, then, we need to draw on the literature on radicalization. This literature shows that the factors causing someone to evolve toward violent terrorism are not only inherent to the individual (e.g., perceived deprivation or personal grievance) or related to the group or the direct environment of the person (meso-level determinants) but that macro-level determinants stemming from society also play an important role in the radicalization process.[7]

Globalization and modernization as well as foreign policy of some (Western) countries constitute typical examples of macro-level determinants which can initiate or advance radicalization. Globalization can threaten group identity and reinforce an us-versus-them way of thinking. A black and white view of the world is easy and offers a feeling of security. Globalization also results in higher mobility of people, leading to ever-increasing numbers of refugees and international migrants worldwide. Migration politics also has a significant effect on terrorism events in the home country.[8]

The geopolitical policy of a country likewise can affect the likelihood of terrorism. Some Muslim groups experience the Western way of life as a threat to their personal lifestyle and some interpret Western geopolitical policy as a threat to the Muslim community at large. One of the defenders of this thesis is the French Professor François Burgat who explains that the vulnerability of France to terror attacks partly stems from its colonial past and its geopolitical policy. Others posit that suicide bombers against Western targets often are driven by nationalist motives. If we extrapolate this reasoning to the number of foreign fighters, we expect a positive relationship between the number of foreign fighters leaving from a country to fight in Iraq and Syria and the foreign policy of this country with respect to the conflicts in Iraq and Syria. If active involvement in the Syrian conflict is interpreted by Muslim society as a threat to their lifestyle, for instance, this could motivate more people to leave and join the fight on the other side, i.e., on the side of Daesh.[9]

Empirical evidence documenting the relationship between military deployment and terror attacks already exists. Foreign military interventions which support and help the government and which involve a large number of ground troops increase the incidence of suicide attacks performed by regime challengers. This is partly explained by the phenomenon that military interventions strengthen the power of the local government and increase the defense of the targets. Thus, insurgents resort to nonconventional, more lethal tactics such as suicide attacks. The military presence of a foreign country also significantly increases the probability of a suicide terrorist targeting the police. Military interventions do not only seem to affect the tactics used in the conflict zone but also in the country which deploys the troops. For instance, U.S. military involvement in different conflict zones has made it more attractive for international terror plots, a finding later confirmed for all NATO countries (over the period 1998–2007): Military deployment to conflict areas results in a significant increase in the probability of a terror attack in the deploying country. And although other researchers initially found that the deployment of U.S. troops decreases the number of terror attacks affecting the host country, the effect fades after controlling for strategic goals (*in casu*, oil).[10]

Clearly, a range of evidence suggests that the total cost of military deployment to a conflict area surpasses the explicit, budgetary cost of sending troops. Among the implicit costs are the reaction to military interventions in terms of increased terror attacks. We study whether military interventions also lead to an increased number of foreign fighters leaving from the troop-deploying country. If this is the case, then an increase in the flow of foreign fighters is an implicit cost of military counter-terrorism policy. Hence our research question: Does participation in the *Global Coalition Against Daesh* in Iraq and Syria lead to a larger flow of foreign fighters?

Data and descriptive statistics

The dependent variable comes from a Soufan Center report and counts the number of foreign fighters going to Iraq and Syria. It captures official, and for some countries also unofficial, data. An update of this report was issued in 2017. As the update did not revise the numbers for certain countries, and left out others, we opt for the 2015 edition. The official data stems from government estimates regarding foreign fighters. Other measures are usually derived from UN reports or academic sources. We opt for the official data which leads to a sample of 49 countries (30 of which are coalition members). This limits the dataset by excluding countries for which official numbers are not available, such as Afghanistan, Kuwait, and Libya. While official data are more reliable than the unofficial numbers, we point out that even for the official statistics different countries use different measures. Hence, cautious interpretation of the results is warranted.[11]

Table 1: Number of foreign fighters by country

Country	*Number of foreign fighters*	*per 100,000 people*	*per 100,000 Muslims*	*Country*	*Number of foreign fighters*	*per 100,000 people*	*per 100,000 Muslims*
Coalition countries							
Australia	120	0.50	21.02	Malaysia	100	0.33	0.51
Austria	300	3.47	64.35	Moldova	1	0.03	4.69
Belgium	470	4.17	70.66	Morocco	1,200	3.45	3.45
Bosnia	330	9.33	20.65	Netherlands	220	1.30	21.65
Canada	130	0.36	17.27	New Zealand	10	0.22	18.13
Denmark	125	2.20	53.64	Norway	81	1.56	42.19
Egypt	600	0.64	0.67	Romania	1	0.01	1.68
Finland	70	1.28	159.69	Saudi Arabia	2,500	7.92	8.52
France	1,700	2.55	34.02	Singapore	2	0.04	0.25
Germany	760	0.93	16.04	Spain	133	0.29	13.64
Ireland	30	0.64	58.31	Sweden	300	3.06	66.55
Italy	87	0.14	3.87	Tunisia	6,000	53.22	53.49
Jordan	2,000	21.84	22.46	Turkey	2,200	2.81	2.87
Kosovo	232	12.88	13.73	U.K.	760	1.17	24.31
Macedonia	146	7.02	75.50	U.S	150	0.05	5.19
Noncoalition countries							
Algeria	90	0.23	0.23	Madagascar	3	0.01	0.41
Azerbaijan	104	1.08	1.11	Maldives	200	48.88	49.68
Brazil	3	0.00	1.46	Pakistan	70	0.04	0.04
Cambodia	1	0.01	0.32	Philippines	100	0.10	1.79
China	300	0.02	1.22	Russia	2,400	1.67	16.66
India	23	0.00	0.01	South Africa	1	0.00	0.11
Indonesia	700	0.27	0.31	Sudan	70	0.18	0.20
Israel	50	0.60	3.21	Switzerland	57	0.69	14.05
Kazakhstan	300	1.71	2.43	Tajikistan	386	4.52	4.67
Lebanon	900	15.38	25.09				

Source: Soufan Center (2015). *Note*: Only countries for which we have data on foreign fighters are included. Coalition membership as of 2015.

Our independent variable of interest is country participation in the *Global Coalition Against Daesh*. Established on 17 October 2014 "to formalize and combine ongoing military actions against the threat posed by IS in Iraq and Syria," the coalition is led by the United States and at the time of writing consists of 79 countries. Contributions to the coalition can take the form of military support but also of human and/or financial support. While the types of contributions differ, all types are "visible" and can induce radicalization in the contributing country. A dummy variable indicates country involvement in the coalition, irrespective of the type of contribution. Table 1 lists the number of foreign fighters in 2015, grouped by coalition and noncoalition countries. We only report countries for which we have official data. This does not mean that other countries have no foreign fighters but merely points to the absence of official statistics.[12]

Table 1 shows no direct relation between the number of foreign fighters and the size of sending countries but several smaller countries have relatively high numbers of foreign fighters, especially when they are coalition members against Daesh. For noncoalition countries, the range is between 1 and 2,400 foreign fighters, and for coalition countries between 1 and 6,000. Russia is the source of by far the highest absolute number of foreign fighters among the noncoalition countries (2,400). The active role of the government of Russia in the conflict, albeit outside the U.S.-led coalition, potentially offers at least part of the explanation. Tunisia has most foreign fighters among coalition members (6,000), followed by Saudi Arabia (2,500), and Turkey (2,200). The median, and mean, for foreign fighters of coalition members is about double the value of that of noncoalition members.

Table 2: Descriptive statistics

Variable type	*Variable name*	*Obs*	*Mean*	*StDev*	*Max*	*Min*
Dependent	# of foreign fighters	49	541	1029	6000	1
Policy	Coalition member	49	(of which 30 are in the coalition)			
Controls	GDP/capita	49	24442	24829	93293	402
	Gini coeff.	25	33.6	6.66	52.7	25.9
	Population	49	1.0e+08	2.7e+08	1.4e+09	409163
	Distance	49	4256	3541	16304	0
	GRR Index	49	4.36	2.42	8.7	0.2
	Polity Index	47	5.38	5.95	10	–10
	Muslim percentage	49	35.3	41.7	99.9	0.1
	Ethnic fraction (%)	46	0.36	0.23	0.87	0.03
	Linguistic fraction (%)	46	0.33	0.25	0.86	0.01
	Religious fraction (%)	46	0.42	0.26	0.86	0.003

On a per capita basis (in terms of the total population as well as in terms of the Muslim population), the Maldives and Lebanon show the highest relative numbers of foreign fighters with, respectively, 49 and 15 foreign fighters per 100,000 people. India and South Africa report the lowest relative numbers. Among coalition members, Tunisia has by far most foreign fighters, not only in absolute (6,000) but also in relative terms (53 per 100,000). Tunisia is followed by Jordan (22 per 100,000) and Kosovo (13 per 100,000). Finland has most foreign fighters relative to the size of its Muslim population.

Apart from coalition membership, our models include a set of control variables inspired by the literature. Countries' economic, social, and political characteristics are reported to affect the number of people that leave to fight for Daesh. We include GDP per capita as a measure of economic prosperity and development. Poor economic conditions can nourish feelings of economic deprivation and marginalization. People living in poorer areas have fewer possibilities to develop a prosperous future and could hence have a higher propensity of developing radical behavior. GDP per capita captures averages and thus hides information on the distribution of economic wealth. But wealth distribution can also play an important role as a poor individual in a poor country may be relatively happier than a poor individual in a rich country. To account for the potential effect of inequality we include countries' Gini coefficient which take a value between 0 and 100. (A value of 0 represents total equality; the higher the coefficient, the more unequal the distribution.) In addition, our models control for population size as it is reasonable to assume that more populous countries host a larger pool of potential foreign fighters. We also include the distance between Damascus and the capital of the sending countries, as being closer to Iraq or Syria presumably makes it easier to leave as a foreign fighter.[13]

Further, we draw on the radicalization literature for indicators regarding feelings of injustice and deprivation. The perception of unfairness can be provoked by the fact that Muslim groups often represent a minority in Western countries. While the lifestyle is already different, every restriction posed can be perceived as a threat, providing a cognitive opening for radicalization. Daesh enlarges the differences between the two lifestyles and offers a radical rhetoric against the Western one. In addition, the group offers identity and a sense of belonging. Hence, the more restrictions a country imposes on the practice of religion, the more prone members of a minority religion may become for radical ideas. We thus include a Government Restriction on Religion Index (GRR), ranging from 0 (very low level of restrictions) to 10 (very restrictive). The potential for democratic participation needs to be accounted for as well. The more people can participate in public debate, the lower political frustration will be. As a measure for democracy, we opt for a Polity Index, ranging from –10 (strongly autocratic) to +10 (strongly democratic). Finally, to include a measure capturing the degree of homogeneity of the society in a country, we include the percentage of Muslims in the total population. As Daesh constitutes an Islamic organization, we expect a positive relationship between the size of the Muslim population and the number of foreign fighters. In addition, we account for ethnic, linguistic, and religious fractionalization, where higher levels

for the respective indices represent more fractionalized societies. Table 2 provides descriptive statistics.[14]

We again remark that the sample is limited, consisting of 49 countries (the countries for which we have official data on foreign fighters in 2015). And as we do not have Gini coefficients for a number of countries, including it further reduces the sample size. We thus conduct all tests with and without it.

Model and results

We opt for a negative binomial regression to study whether a country's coalition participation influences the number of foreign fighters that left it (in 2015), while controlling for other potential effects. In all, we have 11 variables in the dataset, namely coalition member, GDP per capita, Gini coefficient, population size, distance to Damascus, the GRR Index, Polity Index, Muslim percentage, and three fractionalization indices, ethnic, linguistic, and religious Our model choice results from the count nature of the dependent variable. A Poisson model is not appropriate because the assumption of equal mean and variance is rejected in our data.

Table 3: Negative binomial model (IRR)

# of foreign fighters	*Full model*	*Stepwise elimination*	*Without Gini*	*Stepwise elimination*
Coalition member	45.1482**	2.3882**	1.8384	2.6249**
GDP/capita	1.0001***	1.0001**	1.0001***	1.0001***
Gini coeff.	1.0616	–	–	–
Population	1	–	1	1.0000**
Distance	1.0010***	0.9998*	0.9999	0.9999**
GRR	1.8247**	1.1585*	1.0739	–
Polity	1.164	–	1.0593	–
Muslim (%)	1.0729***	1.0206***	1.0335***	1.0298***
Ethnic (%)	0.277	–	0.5227	–
Linguistic (%)	12.4384	–	0.3653	–
Religious (%)	9.0399*	–	1.0152	–
Observations	24	49	45	49
Pseudo-R2	0.0965***	0.0447***	0.0591***	0.0501***

Note: Statistically significant at the ***1% level; **5% level; *10% level.

Results are shown in Table 3. To allow for a more direct and intuitive interpretation, we report incidence rate ratios (IRR) rather than coefficients. As mentioned, we have a limited number of observations for the Gini coefficient. Hence, we run the test with and without this variable. For each of these options, we proceed with a backward step-down selection, that is, a stepwise elimination of the least significant variables. This entails that we start by including all variables for which we find support in the literature and then rerun the estimation, each time dropping the least significant variable. Such a procedure does not only limit the number of independent variables (increasing the degrees of freedom) but also allows us to check, to the extent possible, the robustness of the results for our variable of interest. Backward selection is not without limitations but nevertheless is among the most widely used techniques when it comes to model selection and verification. We thus remind readers once more that results should be interpreted with caution considering the limited official data on foreign fighters and the resulting small sample of countries included in the estimations.

Regarding the variable of interest—participation in the coalition against Daesh—Table 3 mostly reports a substantial influence. We find an extremely high impact when reviewing the test results, including the Gini coefficient on the side of the independent variables, with a reported IRR for coalition membership of 45.15. This would mean that participation in the coalition against Daesh increases the number of foreign fighters by a factor of 45.15 as compared to the average country that does not participate. However, the small sample size combined with the inclusion of the full set of independent variables substantially decreases the statistical reliability of these results. We thus focus on the outcome of both stepwise elimination processes (one starting from the full model, the other one directly leaving out the Gini coefficient) and notice a smaller, yet still substantial, impact with IRRs of 2.39 and 2.62 for coalition members. From these results, we conclude that being a member of the coalition against Daesh increases the foreign fighters in coalition countries by a factor of about 2.5, compared to the noncoalition countries in the dataset. It thus appears that coalition membership has a substantial influence on the number of foreign fighters leaving from a coalition country. We directly add that these results must be interpreted keeping in mind the small sample size. Due to the limited amount of official data available, the estimations include a cross-section of 49 countries. Hence, the results cannot be generalized to other countries. In addition, even though data on foreign fighters is drawn from official sources, we cannot fully exclude differences in measurement techniques, nor measurement errors.

We further find that richer countries have a slightly higher number of foreign fighters. As for population, only one out of four negative binomial tests reveals that a larger population also results in more foreign fighters. However, a substantial

positive size effect is found regarding the variable measuring the Muslim population in a country. Interestingly, we find seemingly conflicting results for distance. The first model reports a positive relationship, meaning that countries further away from Iraq and Syria have more foreign fighters. This is somewhat counterintuitive as we expected that being closer to the Iraq and Syria would facilitate the flow of foreign fighters. The results from both stepwise eliminations, however, indeed report this negative relationship between distance and the number of foreign fighters. This is possibly explained by the set of countries included in the different models. By including the Gini coefficient, a very specific group of countries drops out. Several of these countries are among the ones that are closest to the conflict.[15]

Often it is argued that the United States constitutes an influential statistical observation in the data. As the coalition against Daesh was initiated and lead by the U.S., the results could be driven by its presence in the dataset. Therefore, we also ran our regressions excluding U.S. data. This did not lead to substantially different conclusions. For the most part, the results are similar to the stepwise elimination exercise after initially excluding the Gini coefficient. The incidence rate ratio for coalition membership is 2.69, still pointing toward a substantial influence.

Conclusion

Keeping in mind the small sample size due to limited official data on foreign fighters, we document a positive relationship between membership in the *Global Coalition Against Daesh* and the number of foreign fighters. For the average of the 30 countries included in the estimation, coalition membership results in about 2.5 times more foreign fighters as compared to the average of the 19 countries not in this coalition. If we can interpret the number of foreign fighters leaving from a country to fight at the side of Daesh as an indication of radicalization in a country, this study hence shows that coercive military counter-terrorism policy affects the level of radicalization. In other words, participation in the anti-Daesh coalition increases the support for Daesh in the home country, at least as expressed by the number of individuals leaving to fight.

This study is not free of limitations. Several questions remain, creating opportunities for further research. The conclusions apply, of course, only to the data used in this study and results should be interpreted with caution. First, official data regarding the number of foreign fighters is available only for a limited number of countries. In addition to a small sample size, we point out that our estimations concern only a cross-section. Even though we work with official data only, potential measurement errors cannot be excluded. A more extensive (number of countries as well as a longer period of time) and coherent dataset would thus be very valuable to have at hand for future research. Second, we have used a dummy variable to measure involvement in the coalition against Daesh. Clearly, there are different types of support a country can deliver to the coalition and hence potentially different effects may result from that. Future studies could examine whether there is a difference in the specific contribution a country delivers to the coalition, e.g., the effect of military support versus humanitarian support. Furthermore, it would be interesting to study the effect on the number of foreign fighters of different types of military intervention as well as of past grievances resulting from a country's involvement in other regions.

Despite these limitations, the main results for our sample document a strong effect and hold during robustness tests. The size of the effect certainly offers food for thought. Since security is a public good which surpasses country borders, we need to act as an international society. We cannot merely rely on other countries to go and fight terrorism while we only focus on our own protection. Hence, this study should certainly not be interpreted as a plea against military action. It does, however, argue that the costs related to military interventions surpass the direct budgetary costs. Since military operations increase the flow of foreign fighters from a country, one also needs to account for the societal cost of this increase.

Most countries use a broad spectrum of policies regarding counter-terrorism and counter-radicalization. Diverse policies should be seen as complements, not substitutes. Especially if military interventions increase radicalization, this study suggests that complementing this policy with alternatives focusing on the prevention of radicalization is of crucial importance. These policies aim to increase the opportunity cost of going to Syria (or Iraq) to fight. If these opportunity costs are sufficiently high, this can lead potential candidates to refrain from leaving. The focus should thus probably be on increasing the benefits of not leaving, having more to give up, answering the need to belong in an alternative way. In short, to provide a long-run response to the danger posed by both foreign fighters as well as by homegrown fighters, repression by use of military action is only one part of the answer and it needs to be complemented with preventive policies.[16]

Notes

Suggestions made by participants at the International Conference on Economics and Security (27–30 June 2018 at Middle Eastern Technical University, Northern Cyprus) are gratefully acknowledged as are comments from anonymous reviewers.

1. Quote: Council of the European Union (2011). Number of fighters: Soufan Center (2015).

2. Two ways: van Tigchelt (2017). Recent evidence: RAN (2017). More effective: Hegghammer (2015). Their study shows: Braithwaite and Chu (2017).

3. Not simply related: Soufan Center (2017). Recent literature: Benmelech and Klor (2016).

4. Daesh: Multiple abbreviations and acronyms are used in the media and the literature such as IS, ISIS, ISIL, and Daesh. We opt for Daesh as it features in the official name: "The Global Coalition against Daesh." See http://theglobalcoalition.org/.

5. Partly derived from (and for further information, see) Du Bois (2016; 2017).

6. Radicalizes: Silke and Brown (2016). Neither necessary nor sufficient: Borum (2011a; 2011b). Opportunity to escape: Coolsaet (2016). Heterogeneous: Victoroff (2005).

7. Doosje, *et al*. (2016).

8. Black and white view: van Dongen (2017). Migration politics: Bove and Böhmelt (2016).

9. Professor Burgat: Colloqium in Brussels, 3 March 2017. Some posit: Pape (2006).

10. Incidence of suicide attacks: Choi and Piazza (2017). Partly explained: Choi and Piazza (2017). Targeting of police: Gibbs (2017). U.S. more attractive: Neumayer and Plumper (2011). All NATO countries: Du Bois and Buts (2016). U.S. deployment: Azam and Thelen (2008; 2010).

11. Number of foreign fighters: Soufan Center (2015).

12. Quote and number of coalition members: Global Coalition (2014; 2018).

13. Inspired by the literature: Benmelech and Klor (2016). GDP/capita, Gini coefficient, and population size: World Bank data. Distance: When a country shares a border with Iraq or Syria, the distance variable takes the value of zero.

14. Injustice and deprivation indicators: Borum (2003); Moghaddam (2005). GRR Index: PEW Research Center (2017). The Index is built up from 20 restriction indicators. Polity Index: Marshall and Jaggers (2011). Fractionalization: Dahlberg, *et al*. (2017).

15. Whereas these estimations study the absolute number of foreign fighters and report a substantial influence of participation in the coalition against Daesh, it is also interesting to study which determinants influence the relative numbers of foreign fighters (compared to the total population of a country and compared to a country's Muslim population). While these estimations necessitate a different econometric approach resulting from the changing nature of the dependent variable, they provide very similar results. To avoid undue repetition they are not reported here but are available upon request. Participation in the coalition against Daesh thus substantially increases the number of foreign fighters relative to a country's population as well as the number of foreign fighters relative to a country's Muslim population. The variable GDP/capita, however, loses significance in one test, namely when estimating the effect of coalition participation on the number of foreign fighters relative to a country's population.

16. Broad spectrum: Trivalent (2017). Increase opportunity costs: Frey (2017).

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