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On the Decline of War

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On the decline of war

Michael Spagat & Stijn van Weezel

Abstract: For the past 70 years, there has been a downward trend in the size of wars, but the idea of an enduring 'long peace' remains controversial. Some recent contributions suggest that observed war patterns,including the long peace, could have come from a long-standing and unchanging war-generating process, an idea rooted in Lewis F Richardson's pioneering work on war. Aaron Clauset has tested the hypothesis that the war sizes after the Second World War are generated by the same mechanism that generated war sizes before the Second World War and fails to reject the 'no-change' hypothesis. In this paper, we transform the war-size data into units of battle deaths per 100,000 or world population rather than absolute battle deaths – units appropriate for investigating the probability that a random person will die in a war. This change tilts the evidence towards rejecting the no-change hypothesis. We also show that sliding the candidate break point slightly forward in time, to 1950 rather than 1945, leads us further down the path toward formal rejection of the no-change hypothesis. Finally, we expand range of wars to include not just the inter-state wars considered by Clauset (2018) but also intra-state wars. Now we do formally reject the no-change hypothesis. Finally, we show that our results do not depend on the choice between two widely used war datasets.

JEL-Classification: C10, D74, N40

Keywords: War, rare events, binomial probability

A continuing debate

Scholars have long been interested in the possibility that war might be in decline. Ongoing debate has focused on whether there might be a secular downward trend in war sizes which might herald the decline of war. For more than 70 years there has not been a truly huge war or a direct confrontation between major powers. Nevertheless, the idea of an enduring "long peace", in the coinage of Gaddis (1986), remains controversial. Some scholars have developed the decline-of-war thesis in some detail (Goldstein 2011,Pinker (2011) Hathaway and Shapiro (2017)) while others reject it (Braumoeller 2013, Cirillo and Taleb (2016b), Clauset (2018)). Here we do not attempt a broad survey of the existing literature. Rather, we focus on the recent contributions of Cirillo and Taleb

(2016b) and Clauset (2018) suggesting that observed war patterns, including the long peace, could have come from a long-standing and unchanging war-generating process. In particular, we engage with Clauset (2018) who maintains that the absence of truly huge wars since the Second World War may well have been due to good luck rather than a secular shift toward relative peace. In particular, Clauset (2018) tests the hypothesis that the war sizes after the Second World War are generated by the same mechanism that generated war sizes before the Second World War. He fails to reject what we will call the "no-change hypothesis".

Here are the main contributions of our paper. First, we give a simple exposition of the central ideas behind the new critiques of the decline-of-war thesis made by Cirillo and Taleb (2016b) and Clauset (2018). Note that these ideas hinge centrally on the original insight of Richardson (1948) into the fat-tailed size distribution of modern wars. This connection provides the relevance of our paper to the present book. Second, we transform the war-size data into units of battle death per 100,000 or world population rather than absolute battle deaths and argue that these units are appropriate for investigating the probability that a random person will die in a war. We show that this change tilts the evidence towards rejecting the no-change hypothesis; it does not on its own result in formal rejection at a standard significance level but it does move us toward a preponderance of evidence against no change. Third, we show that sliding the candidate break point slightly forward in time, to 1950 rather than 1945, leads us further down the path toward formal rejection of the no-change hypothesis. Finally, we expand range of wars to include not just the inter-state wars considered by Clauset (2018) but also intra-state wars. Now we do formally reject the no-change hypothesis.[1] Finally, we show that our results do not depend on the choice between two widely used war datasets.

Statistical patterns in war

We need to understand what might be called a war-generating mechanism before we can make a reasonable judgment on whether or not war has declined. Decades ago, Richardson (1948) introduced the idea that war sizes tend to follow what is known as a power law distribution. This means that the frequency of wars of size x is proportional to $x^{-\alpha}$ where $\alpha > 1$ is some constant. Therefore, bigger wars are less common than smaller ones with the value of α governing the rate at which war frequencies decrease as war sizes increase. This remarkable insight has fared well against more than half a century of new data and the development of more rigorous statistical methods for estimating and testing power laws (Cederman 2003; Clauset 2018,González-Val (2015)). figure 1

[1] To be clear, our findings do not refute Clauset (2018)'s. It can be true simultaneously that the per capita war size generation mechanism has changed while the absolute war size generation mechanism has not. We think that explaining these two results together presents an interesting puzzle that is a good topic for future research.

displays raw data points and power-law fits for two separate time periods, 1816-1945 and 1946-2007 using the same data used by Clauset (2018). The x axis gives war sizes measured as battle deaths. The y axis gives the fraction of wars of each size or greater. For convenience of reading both axes are rendered on logarithmic scales so that, e.g., the distance on the X axis between 10 and 100, 100 and 1,000 and 1,000 and 10,000 are all equal. We can read from the curve for 1816-1945 that between 1816 and 1945 roughly 10% of all wars had around 800,000 or more battle deaths whereas after the Second World War perhaps only 5% of wars have reached this level, at least according to the 1946 – 2007 curve which travels through a sparsity of data points at the high end.

Notice that figure 1 follows Clauset (2018) in using the Second World War to demarcate the before and after comparison. The α values of the two curves are, essentially, identical, 1.54 versus 1.55, although the curves differ slightly from each other. Indeed, Richardson (1960) thought that the distribution of war severity was stable over time, i.e. not subject to structural changes such as a decline in war, and 1 plus Clauset (2018) support this view. Moreover, the very fact that war sizes seem to obey a power law suggests that there is some underlying regularity in the characteristics of the system generating the wars (Cederman 2003).

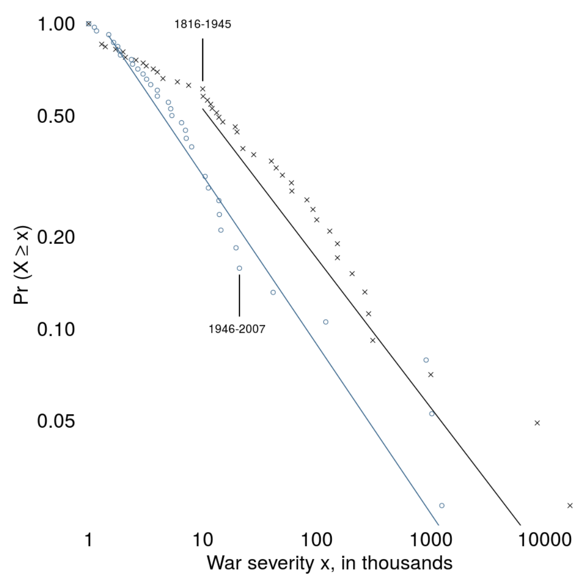

Figure 1: Power law patterns in war sizes for two periods. Data: Sarkees and Wayman (2010), inter-state war

For our purpose, the important characteristic of distributions that follow a power law is that they have what are known as "fat upper tails" governing the inverse relation between war sizes and their frequencies. This property entails that, although bigger wars are less common than smaller ones, the gradient along which war frequencies decline with war sizes is not nearly as steep as it would be if war sizes followed, e.g., a normal distribution (bell curve). Most people are conditioned to think in terms of normal distributions so it requires some mental effort to account for fat tails. For the purposes of this paper the most salient point could be stated as follows; huge wars are really rare but not really really really, rare.

To illustrate this point, let's consider the following numerical example. Suppose that every time the world experiences a new war w, the probability that the size of this war will grow to at least the size of the First World War $\overline{w}$ - hereafter a 'truly huge war' - is 0.006.[2] We now make the important assumption that war size realizations are statistically independent of each other, which implies the size of war w tells us nothing about the magnitude of previous or future wars. Under these conditions, the chance that there is at least one truly huge war after 200 war-size realizations is roughly 2/3 (1 − 0.006 raised to the power 200 is about equal to 0.3). If we lower the probability that each new war will turn out to be a truly huge one down to $P(w \geq \overline{w}) = 0.0001$, then the chance of at least one truly huge war in 200 draws drops to around 1 in 50. Decreasing the probability of a truly huge war on each draw further down to $P(w \geq \overline{w}) = 10^{-7}$, this chance decreases all the way down to about 1 in 50,000. Thus, it makes a big practical difference whether truly huge wars are really rare, $P(w \geq \overline{w}) = 0.006$; really really rare, $P(w \geq \overline{w}) = 0.0001$; or really really really rare, $P(w \geq \overline{w}) = 10^{-7}$. This fat-tail property of the war-size distribution potentially places it into what we might call a "bad Goldilocks" range. On the one hand, 0.006 is a large enough number that we might expect to suffer a truly huge war once every few generations, far too often for such a calamity. On the other hand, 0.006 is a small enough number that the risk of a truly huge war can easily lurk below the surface for a long time without being exposed as a major threat. This is evident from our example which illustrates that the world has about a 1/3 chance of experiencing 200 wars without suffering a truly huge one. If this happens then we could easily last another 200 wars without suffering a truly huge one.

Thus, we arrive at an important insight flowing from the pioneering work of Richardson (1948) and developed by Clauset (2018); the threat of a truly huge future war can be quite serious while remaining, simultaneously, well-hidden for a long time. In other words, we should not dismiss the possibility of a truly huge future war just because such an event would be dramatically out of line with our average range of experience over the last 70 years or so. At the same time, we must be careful not imprison ourselves in our own assumptions as these are largely ahistorical. In particular, we have relied on the artifice of independent draws with fixed probabilities. These calculations are useful

[2] This probability is not entirely fictitious. The First and Second World Wars are two out of 362 wars that occurred between the beginning of the 19th century and 1945, in the dataset compiled by Gleditsch (2004): $2/362 \approx 0.006$.

for understanding important concepts and, perhaps, for beginning to establish baseline expectations. But there is no particular reason to believe that they possess any special powers to describe the world we currently live in or predict its future.

We now delve deeper into the notion of a war-size generation mechanisms which, we assume, exists. This means that the actual historical time series of war sizes is just one realization among many possible realizations generated by this mechanism. If so, then two things can be true simultaneously;

1. Actual war sizes, i.e., the realizations of the mechanism, are declining
2. The underlying threat of war violence, i.e., the mechanism itself, is constant (or not changing much).

The assumption of an underlying war-size generating mechanism underlies much of the new critique the decline-of-war thesis. There is a diversity of opinion among the proponents of the decline-of-war thesis. So we now do some reconnaissance over its main tendencies without providing a review of the whole literature. First, there is usually a claim that actual war violence has declined over time, albeit unevenly (Lacina and Gleditsch 2005,Human Security Report Project (2011)). Different scholars emphasize different time periods, although most view the Second World War as an important turning point. Second, sometimes the main claim is about per capita war violence rather than about total war violence (Pinker 2011). A third tendency is that no one we are aware of argues that truly huge wars have become impossible or that a major reversal cannot occur. To be sure, a sense of optimism pervades this literature with scholars generally providing reasons why war violence is decreasing and why this trend might reasonably be expected to continue. However, invariably there is also a note of caution about the future: we must not be complacent.

In the present paper we concern ourselves with the recent critique of the decline-of-war thesis that was instigated by Cirillo and Taleb (2016b), who collected data on wars going all the way back to Boudicca's rebellion against the Romans in the first century common era (CE). Using extreme value theory to fit the fat-tailed data, they find that they cannot reject their model and conclude from this non-rejection that the data do not support a decline-of-war thesis.^[Cirillo and Taleb (2016b) also offer a separate analysis of war arrival times, also in the spirit of Richardson. But we will leave this discussion aside and broadly accept the idea that there has not been a reduction in arrival rates for new wars. In a companion paper they write that *"there is no scientific basis for narratives about change in risk"* (Cirillo and Taleb 2016a).

Cirillo and Taleb (2016b) helped to prompt renewed focus on the importance of fat tails in war sizes for the decline-of-war debate; however, they left several important issues unresolved. First, although a main contribution of their work is the data collection effort, their dataset is not publicly available and they have refused to allow other researchers to examine it (Spagat 2017). This stance takes their work outside the scientific universe, at least for now, as it is impossible to scrutinize their results. Second, non-rejection of a model fitting two thousand years of data does not rule out the possibility of scientifically grounded discussions about possible changes in war risks during subsets of these two thousand years. For example, given the 565 wars in their database, it is possible that there was a big change after war number 500, but the last 65 draws did not disturb the fit of the first 500 data points sufficiently to lead to rejection of the whole model. Imagine flipping a coin that has a 0.5 probability of landing heads for the first 500 flips and a 0.3 probability of landing heads for the last 65. You would probably not reject a hypothesis that all the flips had a chance pretty close to 0.5 of landing heads. More importantly, if you confine your analysis to the 565 flips as a whole then you will get no hint that there was a dramatic change after flip number 500. A more appropriate approach would have been to test for a break in the data at a potential change point, such as the end of the Second World War: they do not provide such a test. Third, there is an overarching assumption in this approach that the only evidence scientifically admissible to our discussion is a list of war sizes and timings. Cirillo and Taleb (2016b) seem to think that historical events such as peace treaties, formation of international institutions or social trends such as improving human rights are, simply, outside the bounds of a scientific discussion: this restrictive view makes little sense.

Clauset (2018) addresses the first two of the unresolved issues. First, he uses the open-source Correlates of War (COW) dataset that covers interstate wars from the beginning of the 19th century to the present (2007). Second, his whole analysis focuses on testing for a trend break starting at the end of the Second World War. The essence of his approach on war sizes is to fit a power law to the data up through the Second World War and then test the hypothesis that the data after 1945 was generated by this distribution, i.e., he tests what we refer to as a no-change hypothesis. Clauset (2018) concludes that he cannot reject the no-change hypothesis. This finding is intuitive in light of our above discussion although it still may seem surprising at first glance. We know that since the Second World War the world has not suffered another truly huge war. Superficially, this fact may seem to be at odds with non-rejection of the no-change hypothesis. However, if we think in terms of the numerical examples provided earlier we see that this fact does not contradict the no-change hypothesis although it is in tension with it. We could, plausibly, be in a bad Goldilocks zone where the risk of a truly huge war remains substantial yet hidden.

Clauset (2018) provides a useful contribution to our thinking but, at the same time, we must be cautious about this result for several reasons. First, other information besides the time series of war sizes is potentially relevant. Second, we should not think exclusively in terms of any one particular hypothesis such as the no-change one. There are other hypotheses, more in line with a decline-of-war thesis, that would also not be rejected by the data. For example, suppose we modify the no-change hypothesis by stipulating that wars with more than 5 million battle deaths became very very rare after the Second World War. That is, we virtually eliminate the fat tail from the hypothesized war generation mechanism. This restriction will not bind in the post-war period since no war after 1945 even comes close to such a size. Thus, this modified hypothesis is both well in line with decline-of-war ideas and will also not get rejected. Third, we must not fall into the trap of accepting the null hypothesis based on its non-rejection. Clauset (2018) finds that we would finally reject his no-change hypothesis ($p < 0.05$) after about 100-140 more years without a truly huge war. Even then we still would not be able to entirely rule out the no-change hypothesis, let alone dismiss the possibility of a truly huge war. However, if the data became excessively embarrassing to the no-change hypothesis after 100 sufficiently peaceful years then the data would already be fairly embarrassing to this hypothesis after 50 sufficiently peaceful years. Going back to our earlier calculations, the Gleditsch2004 dataset contains 212 wars for the period after the Second World War. If wars continue arriving at the same rate as they have for the last 70 years this would entail that the world would suffer another 212 wars over the next 70 years. If each of these wars has a probability of 0.006 of being truly huge then the probability of experiencing 424 wars without a truly huge war is about 0.1. Thus, if we make it that far we could reject this version of the no-change hypothesis at a 10% level. In other words, the 0.05 threshold is arbitrary and excessively binary; non-rejection of the no-change hypothesis does not mean that the decline-of-war thesis is false until it suddenly switches to true after 100 years without a truly huge war.

At this stage it is valuable to return to the final point of the overview of the decline-of-war thesis outlined at the beginning of this section: nobody actually argues either that a truly huge war is impossible or that one is likely to break out soon. Therefore, the present debate is about the exact size of the probability of a truly huge war, a probability that everybody agrees is very small. The answer to this question matters, but the precise estimation of very small probabilities is hard and it is unlikely that some new and sophisticated statistical method will settle it for once and for all.

Measuring war

Our empirical analysis relies on two datasets that cover war sizes and dates; the commonly used Correlates of War (COW) dataset (Sarkees and Wayman 2010), which was also used by Clauset (2018), and the dataset compiled by Gleditsch (2004). The two datasets overlap quite substantially and both cover the period 1816-2007. Indeed, the Gleditsch (2004) dataset is based on the COW dataset. However, there are important distinctions that are worth understanding even though it turns out that our results do not depend materially on the choice of dataset. For COW there is a big change in the inclusion criteria in 1920 with the founding of the League of Nations. The fundamental test for COW is always membership in the international system for both states, in the case of inter-state war, and for the state in intra-state war. Between 1816 and 1920 this test consists of two parts; i) a population greater than 500,000 and ii) being "sufficiently unencumbered by legal, military, economic, or political constraints to exercise a fair degree of sovereignty and independence". After 1920 the test is switched to membership in the League of Nations (or United Nations) and receiving diplomatic missions from any two major powers (Singer and Small 1972). Gleditsch and Ward. (1999) note that in practice the pre-1920 test boils down to having formal diplomatic relations with Britain and France. This rule excludes many countries and their wars, including the three Anglo-Afghan wars that took place between 1839 and 1919 and some intrastate wars such as the 1831-1845 civil war in Central America.

It would be unfair to label the COW dataset as simply incorrect yet we believe that its British-French emphasis excludes many wars that are relevant to the decline-of-war debate. The revised data by Gleditsch (2004), which corrects these systematic problems, contains 574 wars between 1816 and 2007, 136 of which are interstate wars. During the same period COW contains only 474 wars, 95 of which are interstate. Thus, the difference in war counts is substantial. Moreover, 1920 is close enough to the Second World War so that the 1920 switch could potentially affect the results of Clauset (2018). Thus, we prefer Gleditsch (2004) but run our calculations on both datasets.[3]

A second contrast with Clauset (2018) is that we include both inter and intrastate wars in our analysis. We think that there is no *a priori* theoretical justification to separate the two and agree with Small and Singer (1982) who argued that "an understanding of international war cannot rest on interstate wars alone". The common focus on wars involving major powers or other interstate wars seems to be driven by data availability rather than theoretical considerations (Cunningham and Lemke 2013). Indeed, some of the worst atrocities since the end of the Second World War took place during civil wars such as the ethnic cleansing in Yugoslavia and the Rwandan Genocide. Thus, combining all wars is best practice in our view although we also run our calculations on interstate wars alone and intrastate wars alone.

Our third data departure Clauset (2018) is that we divide all war sizes by world population estimates. These are applied to the start year of each war and taken from Fink-Jensen (2015), Klein Goldewijk, Beusen, and Janssen (2010), and U.N. Department of Economic and Social Affairs (2013) with some interpolations before 1950. The probability that an average person will be killed in war is of particular interest to the decline-of-war discussion and population adjustment is appropriate to assess this probability. At the same time we see the point of Braumoeller (2013) who argues that examination of unadjusted war sizes is of great relevance to understanding human war-proneness.[4]

[3]Note that the Gleditsch (2004) dataset covers about two centuries of war yet contains roughly the same number of wars as the Cirillo and Taleb (2016b) dataset which covers two millennia of wars. The inclusion criteria for the two datasets seem to be similar.

[4]A war that kills one million people is an unmitigated disaster both in a world of 5 billion people and in a world of 9 billion people and lower probabilities of an average person getting killed by a war might be more a consequence of population growth than of a declining human

We stress one further point about the data before moving on to the analysis. First, war sizes are meant to include just battle deaths but both of our datasets work from available sources that might often mix in other kinds of deaths. This issue creates two separate problems. First, ideally we would have data on the full human cost of war but often we only have data on the battle-death component of this cost. For example, both datasets record 910,084 deaths for the Korean War but a full figure would include famine deaths that could push the number up to 5 or 6 million (Lacina, Gleditsch, and Russett 2006). Second, there is inconsistency across wars since some figures hue close to a battle deaths only concept whereas other figures are more comprehensive. Yet it is virtually impossible to verify or modify the fatality numbers, given the poor state of documentation of the COW dataset.

Insights from the data

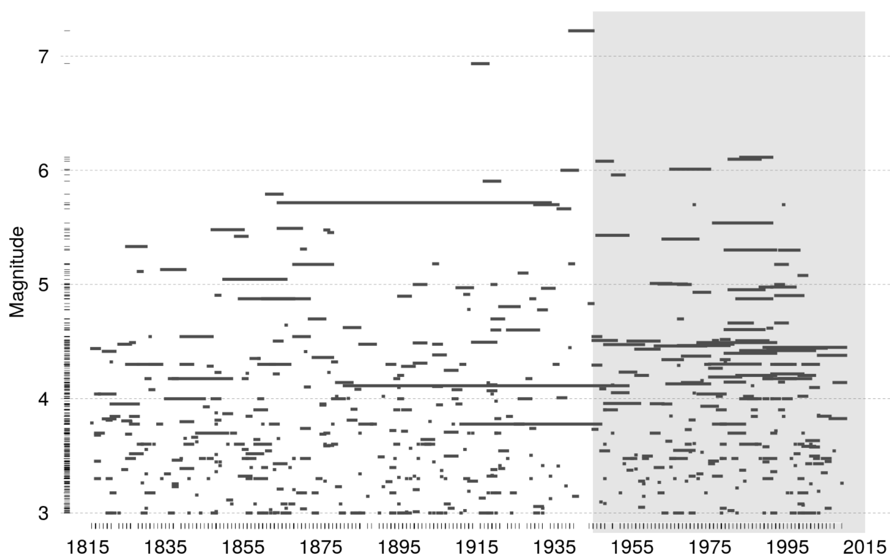

Figure 2: Wars over time, ranked by order of magnitude. Data: Gleditsch (2004)

Inspection of figure 2 does not reveal any clear pattern in war sizes or trends. It does, however, highlight the sheer enormity of the First and Second World Wars compared to all other wars. After adjusting for global population sizes we find that these two wars killed, respectively, 499 and 781 people per 100,000.

tendency towards violence.

The next largest war is the American Civil War with 52 fatalities per 100,000 people.

A particular feature of our approach is the large number of no-change hypotheses that we test. All our hypotheses have two separate cut-off points: one is based on time periods and the other is based on per capita war sizes. Our time periods pivot either around the Second World War or the Korean War. Future work should consider more potential time period cut-offs but here we look just at these two. For war sizes we consider all possible cut-offs and examine the fraction of all wars above each war-size cut-off for both the early period and the late period. In short, we examine many fat right-hand tails and evaluate whether the tails for the later periods are thinner than the tails for the earlier periods.
Each evaluation is a formal test of a no-change hypothesis. These tests require just simple calculations using binomial distributions.

Here is how these calculations work when the time cut-off point is 1945. According to the Gleditsch (2004) data there were 362 wars between 1816 and 1945 with the Second World War being the largest of these by far. Our first no-change hypothesis for the post-1945 period is that the probability that a random war after 1945 will kill at least 781 people per 100,000 is given by the fraction of all wars before 1945 that reached this violence level. This is $p_0 = \frac{1}{362} \approx 0.003$.

It turns out that between 1946 and 2007 0 wars out of 212 in the Gleditsch (2004) data attained this size. If war sizes are drawn randomly and independently of each other and if the no-change hypothesis is true then the probability of this happening is $1 - \frac{1}{362})^{212} = 0.56$. This probability can be interpreted as a p-value on one particular no-change hypothesis at the most extreme end of the distribution of war sizes.[5]

Next we calculate exactly the same p-values for lower and lower war sizes. For war sizes beginning at 781 per 100,000 and moving down towards 499 per 100,000, the size of the First World War, the p-values stay constant. At 499 battle deaths per 100,000 the p-value drops to $1 - \frac{2}{362})^{212} = 0.31$. It then stays constant all the way down to 52 battle deaths per 100,000, which is the size of the American Civil War (1861-1865) at which point the p-value drops down to 0.17 $((359/362)^{212})$. In short, the three biggest wars were all before World War 2 inclusive and together they yield a preponderance of evidence against a no-change hypothesis but not a formal rejection since p stays above 0.05. The next largest war is the second phase of the Chinese Civil War which pitted the communists under Mao Zedong and against the nationalists under Chiang Kai-shek and caused 51 battle deaths per 100,000 people. The no-change hypothesis assigns probability $\frac{3}{362}$ to the probability that each war size after World War 2 will exceed 51. Since this happens once in 212 draws the p-value on the no-change hypothesis adds together the probability of 0 wars above size 51 and the probability of 1 war above size 51, leading to a p-value of 0.47..

We calculate p-values similarly as we move to smaller and smaller war sizes.

[5] Braumoeller (2013) offers a similar calculation.

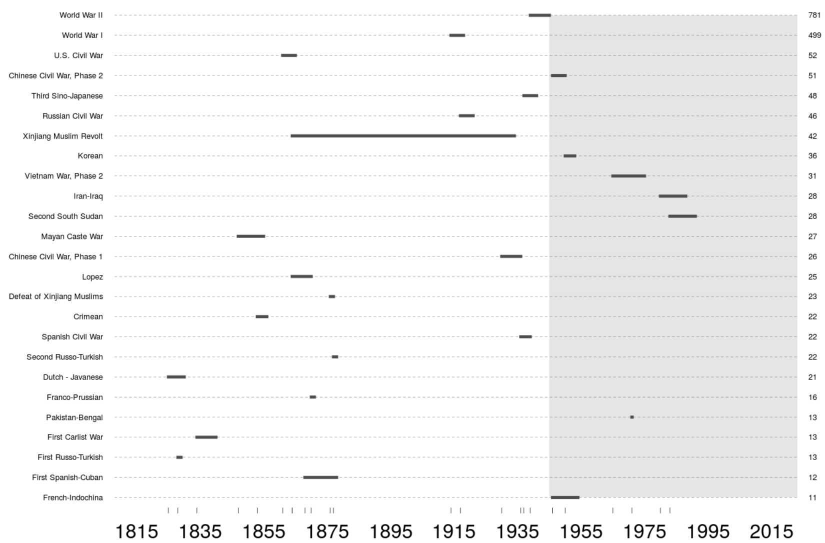

Figure 3: Overview of the largest wars. The number on the right y axis shows number of battle deaths per 100,000 people. Data: Gleditsch (2004)

When, for example, there are 6 wars before 1945 of size s and above then the no-change hypothesis fixes a probability of $\frac{6}{362}$ on the event that a new post-1945 war will be of size s or above. If, for example, 3 out of these 212 wars after 1945 are then the p-value on the no-change hypothesis is the probability of 3 or fewer wars of size s or greater after 212 independent draws, each with probability $\frac{6}{362}$ of reaching this size. We use the binomial formula to make this calculation.

Panel a in figure 4 displays the p-values for the tests of all no-change hypotheses tests with cut-offs for war sizes below 50 battle deaths per 100,000 people and with a time break point of 1945. The curve jumps at war sizes that actually occur and is flat everywhere else. The p-value curve jumps down at the sizes that occurred between 1816 and 1945 and jumps up at war sizes that occurred between 1946 and 2007. Reading from right to left the curve dips down below 0.2 as we move through the Third Sino-Japanese War which began in 1937[6], the Russian Civil War (1918-1920)following the Russian Revolution of 1917, and the 1864 Muslim revolt in Xinjiang, China. The p-values then rise above 0.8 as the next four largest wars all occurred after the Second World War. These are the Korean War (1950-1953), the second phase of the Vietnam War which started in 1965, the Iran-Iraq war between 1980 and 1988, and finally the Second South

[6]This war is often known as the Second Sino-Japanese War. The data counts three wars between China and Japan: the first starting in 1894, the second in 1931, and the third in 1937

Sudan War (1983-2005). Next, continuing to read from right to left, the next 9 wars all took place before the Second World War, bringing the *p*-values back down to around 0.2.[7]

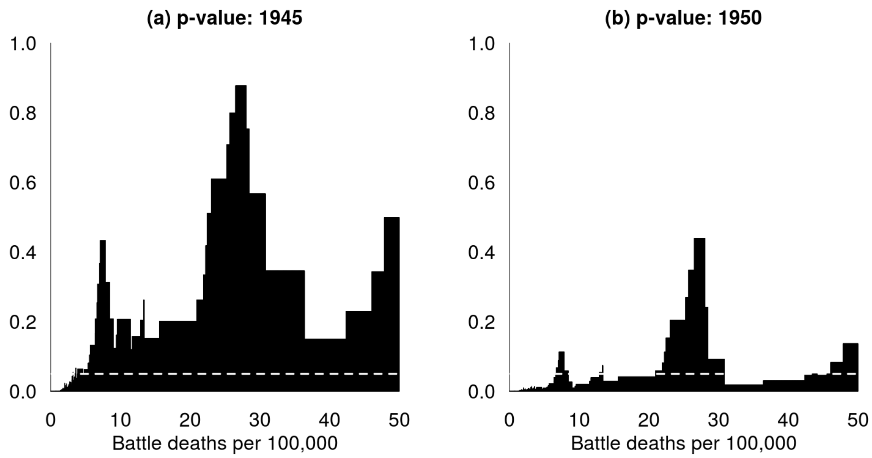

Figure 4: Results of tests of no-change hypotheses for all wars based on Gleditsch (2004), using 1945 (a) and 1950 (b) as break point.

In summary, we find the tail evidence to be unfavourable to the no-change hypothesis ($p < 0.5$) except for in a narrow range of tails for war sizes between about 25 and 28 per 100,000, i.e. starting very far from truly huge wars. At the same time, we never get close to rejecting the no-change hypothesis at the standard 0.05 level. So although we think that this evidence leans towards the decline of war idea, it is far from definitive. The American phase of the Vietnam War, the Iran-Iraq War, and the war in South Sudan provide reasons to doubt this conclusion, although they all stopped before exceeding 30 battle deaths per 100,000 people. When we use 1950, rather than 1945, as a break point the results are much more favourable to the decline-of-war thesis. Now the eight largest wars in per capita terms occur before the break point. Panel b in figure 4 displays the new *p*-values. No-change hypotheses are often rejected at the 5%, and even the 1% level for a wide range of tails.

Of course, panel b in figure 4 displays exactly the same data as panel a does but with the break point pushed forward to 1950. Two of the very biggest wars (the Chinese Civil War and the Korean War) broke out within the 1945 to 1950 time

[7] The remaining sizes below 20 battle deaths per 100,000 take us beyond our core subject

window.[8] The p-value curve is now drops much lower than it did when 1945 was break point. It is well below the standard significance level of 0.05 until the wars in Vietnam, Iran-Iraq and South Sudan drag it back up to around 0.5. Subsequently it moves down back below 0.05 again.

We have made four separate data changes compared to Clauset (2018): measuring war sizes in per capita terms, using Gleditsch data rather than COW data, considering 1950 as a break point and including intrastate as well as interstate wars. To isolate the importance of each particular change we now consider each in turn. First, adjusting for world population levels is essential to get anything resembling the results in this paper. This is so much true that we do not even bother showing pictures unadjusted for population. Second, the choice of COW or Gleditsch does not matter, as one can clearly see from figure 5. Third, both figure 4 and figure 5 show that the choice of break point matters; evidence against the no-change hypothesis much stronger when the break is at 1950 than it is when the break is 1945. Finally, figure 6 and figure 7 show that our decision to include intrastate wars does matter. We think this is simply due to sample size; excluding intrastate wars decreases the number of wars, making it harder to reject the no-change hypothesis.

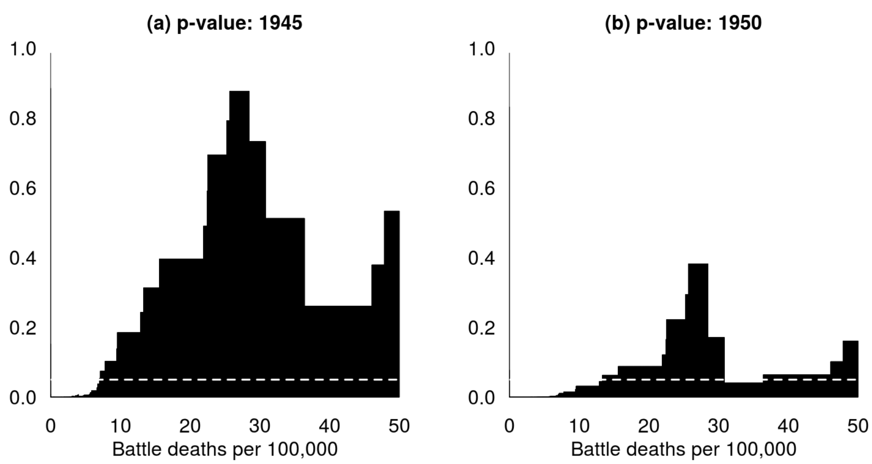

Figure 5: Results of tests of no-change hypotheses for all wars based on Sarkees and Wayman (2010), using 1945 (a) and 1950 (b) as break point.

[8]Note that throughout our analysis we date wars by when they start.

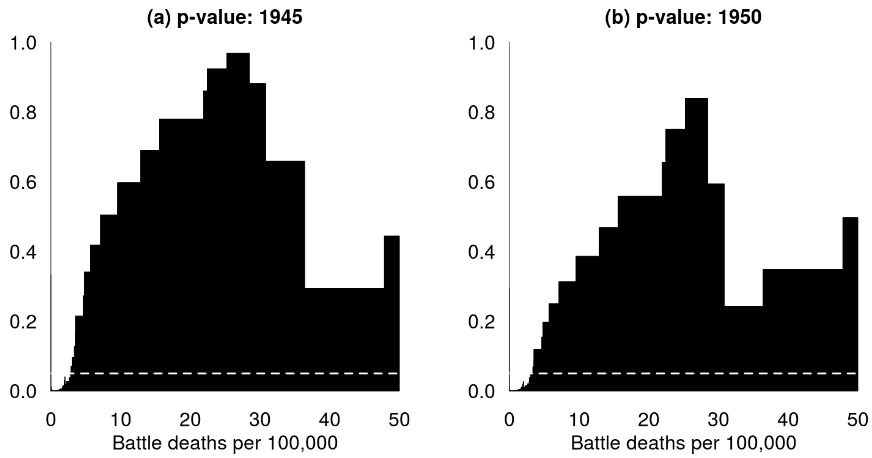

Figure 6: Results of tests of no-change hypotheses for interstate wars only based on Gleditsch (2004), using 1945 (a) and 1950 (b) as break point.

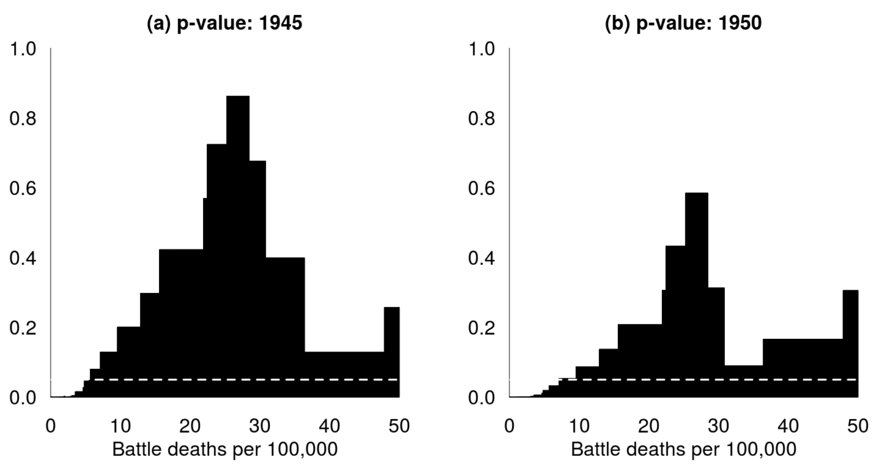

Figure 7: Results of tests of no-change hypotheses for interstate wars only based on Sarkees and Wayman (2010), using 1945 (a) and 1950 (b) as break point.

A more peaceful world since 1950

There will continue to be debate on the probability of another truly huge war. If we limit our attention to the probability of a future war at least as large as the First World War then, consistent with Clauset (2018), our analysis suggests that there is presently not enough data to draw a strong conclusion. At the same time, our analysis also suggests that the chances of drawing a truly huge now are probably lower than they were in the 19th century and the first half of the 20th century. When we widen our scope to include, e.g., wars killing more than 40 per 100,000 of world population then there is substantial evidence that the world has become significantly more peaceful since the 1950's.
Until recently scholars have tended to assume that the Second World War is the obvious candidate for a break point into a more peaceful world.

However, recent papers of Fagan et al. (2018) and Cunen, Hjort, and Nygard (2018) start from an agnostic position potential break points and use statistical methods to detect convincing ones. Both papers find substantial evidence for a change at 1950 although they identify other strong, and perhaps preferred, candidate break points including 1912 (Fagan et al. 2018) and 1965 (Cunen, Hjort, and Nygard 2018): These results complement ours nicely.

There is certainly room to improve on our analysis. First, we repeat our caution that a full treatment of these issues should consider more than just the time series of war sizes (and population numbers). Second, it would be helpful to go beyond battle deaths to include more complete numbers on war deaths.
Unfortunately, it is unlikely that this will ever be fully possible. That said, we can think of no reason to expect that fuller data on war deaths would have a big impact on our findings. Third, the new research into defining change points is an important development that will, hopefully, continue. Nevertheless, despite the potential for improvement we believe that our paper should shift the debate in favour of the decline-of-war thesis.

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