

Pinker versus Taleb:
A Non-deadly Quarrel over the Decline of Violence

Professor Michael Spagat
Department of Economics, RHUL
m.spagat@rhul.ac.uk

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This is the first paragraph in the preface to Steven Pinker's 2011 book *The Better Angels of our Nature*:

“This book is about what may be the most important thing that has ever happened in human history. Believe it or not – and I know that most people do not – violence has declined over large stretches of time, and today we may be living in the most peaceable era in our species' existence. The decline, to be sure, has not been smooth; it has not brought violence down to zero; and it is not guaranteed to continue. But it is an unmistakable development, visible on scales from millennia to years, from the waging of wars to the spanking of children.”

Today I just want to focus on two points from the quote.

1. The book is about the decline of ***violence in general*** – war is only part of this story, albeit a very important part.

2. Pinker does ***not*** claim that the decline in violence is ***inexorable***, although he certainly argues that it has momentum behind it.

Today I'll talk mainly about the decline (or nondecline) of war but it's important to remain aware of the bigger violence picture,

This is not just because much of the Pinker book would still stand even if the decline-of-war thesis were knocked down.

It is also because the dynamics of ***different violence types are probably related to one another*** – they might have common causes or they might cause each other.

Pasquale Cirillo and Nicholas Nassim Taleb disagree with Pinker about war:

“All the statistical pictures obtained are at variance with the prevailing claims about “long peace”, namely that violence has been declining over time.”

(Note 1 – by “violence” C & T mean “*war* violence”)

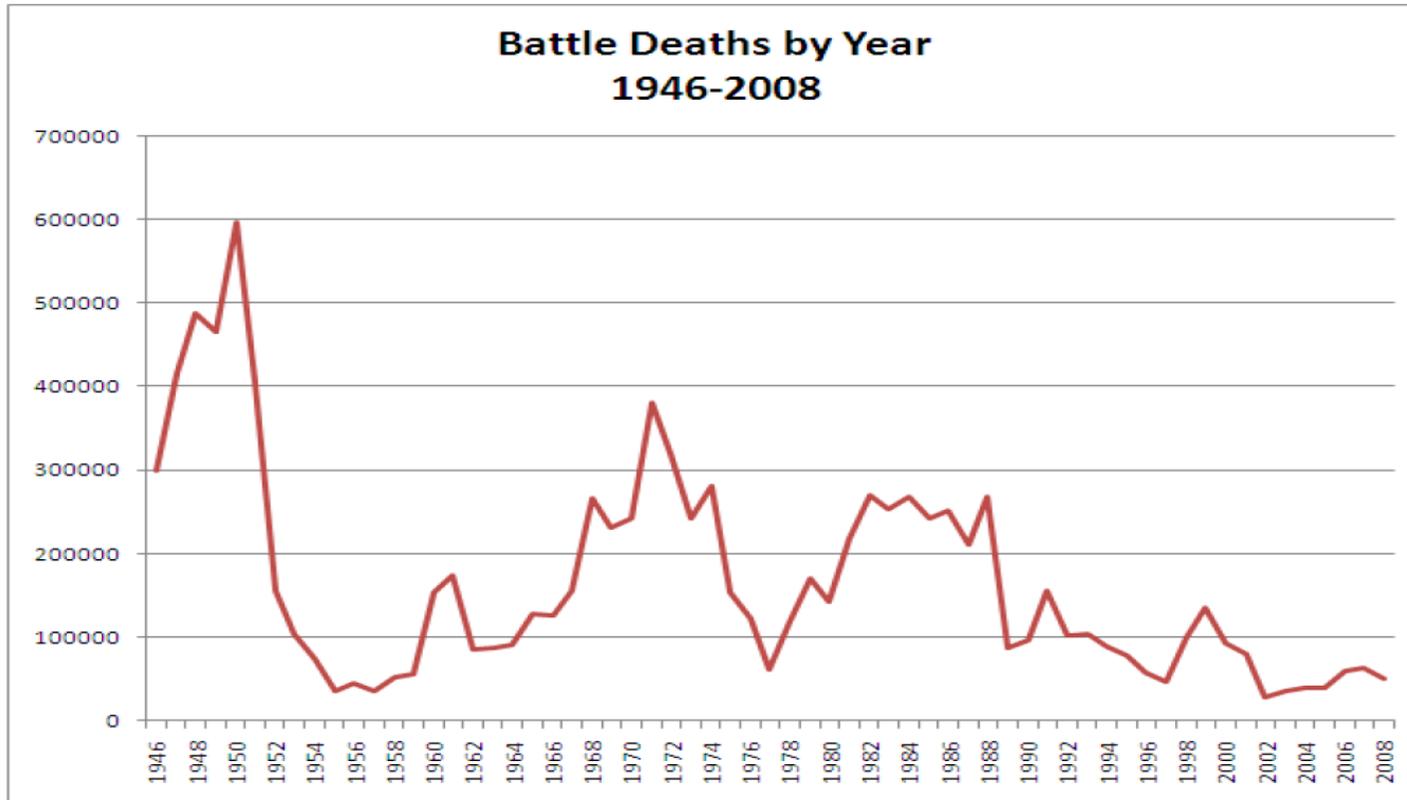
(Note 2 – the term “long peace” originally referred to the absence of war between great powers after WWII but C & T use the term as a broad shorthand for the decline of war generally.)

Bibliographic Aside

Steven Pinker is very much the main target of Cirillo and Taleb but other people have contributed to the decline-of-war thesis, including Nils Petter Gleditsch, Bethany Lacina, John Mueller, Joshua Goldstein and Andrew Mack (through the Human Security Report).

I apologize to people I've left out and would be happy to take suggestions for modifying the list.

Have a look at this picture:



Source: PRIO Battle Deaths Dataset

Battle deaths decline (pretty unevenly) after World War 2.

The Peace Research Institute of Oslo (PRIO) time series only goes up to 2008 – if updated it would increase a bit after 2008 because of the Syrian conflict but this Syria-driven movement would show as a minor uptick in the overall picture.

Also, notice that the series starts *after* the two world wars – the downward trend would be much more pronounced than the picture shows if you extended it to include these major wars.

So how can Cirillo and Taleb say that “all statistical pictures are at variance” with the decline-of-war idea?

Actually, C & T do have a point - ***perhaps the forces that generate wars didn't actually change much after World War 2 but we've just been enjoying a 70-year run of good luck.***

Analogously, imagine your favourite sports team is having a rotten year but then suddenly wins four games in row – this might, unfortunately, be only a temporary random blip.

And C & T's point is better than the sport analogy suggests because really big wars (like World War 2) are rare events.

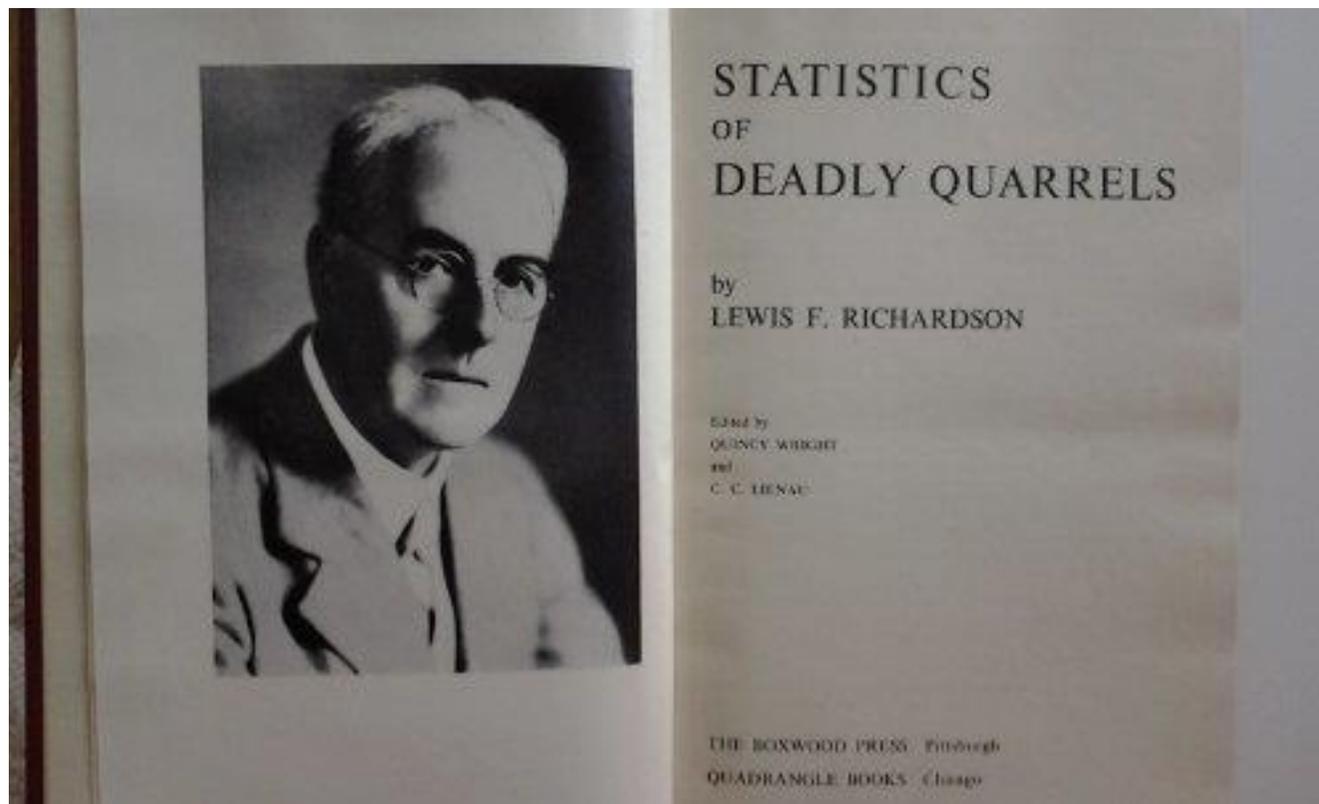
If, say, huge wars only happen once every 80 years on average then we could easily have a run of 150 years without one, even while the underlying risks of big wars hold steady.

Taleb can be confusing on this point, sometimes erupting vitriolically against the simple claim that war violence *has* declined as if it is unscientific to even say this.

It's as if you delight in the above-mentioned four-game win streak but your friend insists you are unscientific to even think this because it might be just a random blip.

Really, it should be OK to acknowledge the existence of both the winning streak as well as the possibility that it is not a new trend.

Let's now go on a brief digression.



The great Lewis Fry Richardson found that ***war sizes (1820 – 1945) can be well modelled by a “power law distribution”***.

This means that the number of wars of size S is proportional to $1/S$ raised to some power.

In plain language we might say that huge wars, like World War II, are really rare but not really, really, really, rare as they would be if war sizes could be well captured by a conventional Bell Curve rather than by a ***fat-tailed distribution*** like a power law.

In other words, there is a relevant distinction between events that are really rare and events that are virtually impossible.

I *would* take a bet to lose all my wealth if a student taller than 3 metres walks into my classroom next year and to win £100 otherwise – the 3-metre student event is virtually impossible.

I *would not* take a bet of all my wealth for £100 hinging on the event that a war killing 10 million people does not break out over the next 10 years – such a war is very unlikely but not impossible.

Not content with his remarkable contribution on war sizes, Richardson also discovered something very interesting about the timing of wars (1820 – 1945): ***War onsets are well captured by a “stationary Poisson distribution”.***

That is, the (random) number of wars breaking out during a fixed time period will depend only on the *length* of the period and not on the *history* of war onsets prior to the beginning of the time period.

[End of the Richardson digression.]

Suppose we try to ***extrapolate the future of war mechanically from the history of war sizes and timings.***

We will, necessarily, predict that our future will contain some really big wars – if the future is like the past then it's just a matter of time before disaster strikes.

This is a truism but ignores the central question – ***has the world become more peaceful so that extrapolation from the past now overestimates the chances of a huge war?***

The Contribution of Cirillo and Taleb

1. They ***assemble a dataset*** on what they hope are all armed conflicts with more than 3,000 “casualties” between 1 AD and 2015 AD – a total of 565 such conflicts.

(Note – I would reserve the word “casualties” for killings plus injuries but I’m pretty sure that C & T use it to mean killings only.)

Assembling this data set is, potentially, a big contribution to the conflict field but this potential may not be realized because, as of now, C & T [refuse to share their data.](#)

This data secrecy is important because such a dataset can hardly be pristine – C & T's methods appear to be fairly robust to data inaccuracies but we still need to see the data to have good confidence in their findings.

2. They fit a fat-tailed distribution (more general than a power law) to the war-size data.

3. They fit a stationary Poisson distribution to the war-timing data.

Points 2 and 3 together say that ***Richardson's two big insights hold, but on a 2,000 year run of data, not only for Richardson's 1820 – 1945 dataset.***

This would be a very nice contribution to our knowledge of war if it holds up but C & T don't stop there.

They claim, further, that

“....there is no basis to discuss any ‘trend’, and no scientific basis for narratives about change in risk.”

Reminder

It's important to distinguish between a decline in violence itself versus a decline in the *risk* of violence.

This distinction is about what has actually happened versus what may well have happened.

There are several problems with Cirillo and Taleb's approach.

1. There is a bold underlying assumption that ***the future will be just like the past.***

Some such extrapolation assumption is always both necessary and risky in any forecasting exercise but here it is unusually strong since C & T go all the way back to Queen Boudica's rebellion against the Romans in the 1st century AD – ***is there really some fixed war-generating process that applied two thousand years ago and that continued to apply though the centuries and down to the present day?***

2. Cirillo and Taleb's approach is *assume* there is no trend, fit a model with no trend since Boudica, find that this trendless model doesn't get rejected in hypothesis tests and conclude from this non-rejection that there actually is no trend.

Essentially, they accept **a hypothesis in response to non-rejection of that hypothesis with the test predicated on assuming the hypothesis is true.**

This is a well-known statistical fallacy.

Still, I would grant that non-rejection of a trendless model doesn't hurt the case that there really is no trend.

3. C & T *never explicitly entertain hypotheses about whether the post-World-War 2 period may or may not differ from the pre-World-War 2 period.*

Without doing this they can't really address the question of whether the risk of war violence has declined since World War 2.

In other words, you need to do an explicit before and after analysis to convincingly conclude that there has been no downward trend.

Consider the following example - we flip a coin 550 times and want to know whether it became more likely to land “heads” on the last 50 tosses compared to the first 500 tosses.

Assume that in reality it was a fair coin on the first 500 tosses that produced 250 heads and 250 tails and then it turned into an unfair coin that produced 35 heads and 15 tails on the last 50.

(Perhaps there is reason to believe that something happened to the coin after flip 500 that changed it physically.)

Suppose we do a standard test of the hypothesis that the coin was fair for all 550 flips.

We will not reject this hypothesis at a standard significance level. (You can play with such calculations on [this site](#))

We know by construction that the coin becomes unfair for the last fifty flips but ***the data from the first 500 flips overwhelm the data from the last fifty*** and so, incorrectly, we don't reject the fairness hypothesis over the full 550 flips.

If we want to test the hypothesis that the coin became unfair for the last 50 flips then we should address this hypothesis directly.

Specifically, we can estimate the probability of heads based on the first 500 flips (which will be $\frac{1}{2}$) and then test whether the last 50 flips seem to be governed by this fairness parameter.

A classical hypothesis test *would* then reject the fairness hypothesis for the last 50 flips.

An important new paper by [Aaron Clauset](#) (at the moment circulating only by email) does exactly this sort of thing for the decline of war hypothesis.

Clauset uses the Correlates of War dataset to model the sizes and timing of wars between 1823 and 2003.

Clauset then restricts his modelling to wars between 1823 and 1945 and tests whether the post-World War 2 data were generated by the same process that generated the pre-World War 2 data.

Clauset cannot reject the hypothesis that the pre-World War 2 war-generating process (assuming it exists) continued to generate wars after World War 2.

Indeed, Clauset writes that the post-World War 2:

“...trend would need to continue for 100-150 more years in order to reliably conclude that it was not a long transient under a stationary process.”

This is good news for Cirillo and Taleb.

However, note that Clauset's exercise is stacked in favour of C & T.

He starts with the hypothesis that the pre-World-War 2 process generates the post-World-War 2 data and fails to reject it – again, failure to reject a hypothesis is not proof that the hypothesis is true (Clauset does not claim that non-rejection is such proof).

Clauset also finds the following in the [Correlates of War](#) data:

“However, we also find that the postwar period exhibited a statistically unusual reduction in the frequency of large conflict, which is balanced by an increased likelihood of small conflicts.”

That is, ***war violence did actually decline*** (it is not out of bounds to state this fact).

Suppose we now test the hypothesis that post-World War 2 war violence has been declining at a rate that roughly fits the war data we have.

Will we reject that hypothesis?

I don't think so – ***if we can't reject a hypothesis of steady war violence with data displaying decreasing violence then we certainly will not reject a hypothesis of declining war violence with the same data.***

Does this prove that the risk of war declined throughout the post-war period?

No

The real point here is that the classical hypothesis testing framework is pretty much powerless in this fat-tailed situation – ***it will take years to settle this debate if we only look at data on war sizes and timing.***

Do we have to just throw up our hands and wait another 150 years?

No, thankfully.

We can look at other evidence – there is nothing forcing us to focus exclusively on the time series for war sizes and timings.

Let's resort one last time to the analogy of the losing sports team that suddenly starts winning.

Suppose that the winning started immediately after the team brought in a heralded new coach and a bunch of good players.

It would be, frankly, stupid to ignore these personnel changes in forecasting the direction of the team – there is simply no justification for ignoring everything except the time series of wins and losses.

Recall that Pinker's book is about the decline of violence, generally, not just about the decline of war violence.

Violent crime has declined, slavery nearly abolished, capital and corporal punishment greatly curtailed and much more – you need to read the book to appreciate the full scope of the phenomenon.

The varieties of violence must reinforce each other, e.g., we were probably more warlike back when we publicly tortured people than we are now that we don't engage in this appalling practice.

Moreover, there are some notable historical developments that are strong candidates to explain the decline we see in the data (you can find details in Pinker's book). These include:

1. Declines in [military spending](#) as a proportion of GNP (see also [here](#)) and in the practice of military conscription.
2. The formation and development of the European Union which has steered its member countries away from their sad history of widespread war.

3. International agreements such as the [Kellogg-Briand Pact](#) and the [UN Charter](#) which outlaw war except in cases of self-defence and Security Council approval.

4. The very sharp decline of interstate war, the most violent kind of war, as would be suggested by points 2 and 3.

5. The widespread rejection of militarism in intellectual and artistic discourse.

The Bottom Line

Classical hypothesis testing based only on the historical timings and sizes of wars leaves us in more or less a dead heat over the decline of war thesis.

That said, if I had to make a call based on historical timing and size data alone I would tend toward the decline of war since this is the actual tendency of the realized data, although I do recognize that this tendency is consistent with no change in the underlying risk of war.

However, when you take a broad historical perspective on the full range of violence forms then the balance tips strongly toward the decline of war thesis.

This tendency is not inexorable but I believe that it does exist.

Thank you for listening.