“Collateral Damage: Trade Disruption and the Economic Impact of War,”
by Reuven Glick and Alan Taylor

comments
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In brief...

► A major study
  - on an important topic,
  - well-executed.

► Of great interest even to those who don’t care about century-old history,
  - as work by Alan & other economic historians,
  - because it brings to bear a long enough data series (1870-1997) to be able to answer questions that post WWII statistical studies are simply unable to answer.

► And Reuven is a fellow member of the gravity club, where large amounts of data are available on a cross-section basis.
The question: the economic costs wars inflict via lost international trade.

► Dramatic conclusion: this indirect cost = or > the direct costs that we usually associate with wars.

► I will raise a few doubts about that comparison.

► Even if the comparison stands, it is bound to strike any political scientist (let alone any member of the military) as an implausible exercise in “me-tooism” by economists: How could the trade costs possibly compare with the destruction of human life and property?

► => a suggestion for presentation.
Message would be less jarring if the authors included a survey of some studies of other indirect costs of war.

- Wars, especially the two World Wars, generally claim more lives through associated famine, disease, & dislocation than through combat.

- A topical case: the costs of the Iraq war. Three studies:
  - Nordhaus (ex ante) and Bilmes-Stiglitz and Orszag (ex post).
  - All three are detailed and explicit about which costs they consider and which they choose to leave out.
  - All three go well beyond short-term budget costs (e.g., they include the costs of long-term medical care for disabled vets), yet exclude costs such as loss of lives among Iraqi civilians (Glick-Taylor do count civilians --Table 8),
  - let alone less direct costs.
The paper is to be commended for including the cost of impaired trade with non-belligerents.

US efforts to remain neutral in the Napoleonic Wars and in World War I ended in failure both times, in part because of the effects of these conflicts on American shipping, which suggests that we took trade pretty seriously.
Possible reverse causality from trade to war

► which has received more attention by others than the direction of a causality that is the subject of this paper (from war to trade).
► Since the presumed effect of trade on the likelihood of war is negative, the same sign as the observed correlation, the authors know they have to deal with reverse causality as a possible explanation for their results.
► They indeed get an apparent negative effect of trade on war, but it disappears with country-pair fixed effects.
► Conclusion, “…the level of trade interdependence may help to answer the question of which countries engage in conflict, rather than when countries engage in conflict.”
Questions about this elimination of the reverse causality problem

► The other variables in the probability of conflict equation:
  ▪ contiguity, alliance, preceding years of peace, & great power status.
  ▪ I am a little unclear whether to think of these as instrumental variables for the war variable; if so we have a problem because all are also logical determinants of trade.

► I understand that there is apparently no tendency for a pair of countries that experiences an increase in trade over time to be less likely to go to war. (The correlation is only across country pairs.)

► But then where does this leave the central finding, that fighting wars is negatively correlated with trading?

► (Does it all depends on lags?)
In any case, could liven up the citations by mentioning:

- The increase in trade between France & Germany in the decades before WWI -- famously proclaimed to have rendered war obsolete in *The Grand Illusion*, published in 1913 (by Norman Angell);

- The current upward trend in trade between China (PRC) and Taiwan (PoC) -- normally presumed to reduce the probability of conflict.
Central conclusions

► War reduces trade
  ▪ among belligerents by about 80-90%, and
  ▪ with non-belligerents by somewhat less.

► Altogether, in the case of World War I, global trade is estimated to have been cut
  ▪ by something like 30% during the war years (p.25)
  ▪ and by a declining amount after the war.
I can’t quarrel with the way they chose to translate lost trade into lost output: an estimate in F & Romer, *AER*, 1999

►► It is always great to have one’s own work used in such a way.
►► In truth, my preferred estimate is .4, not the 1.98 that they understandably take (p.28) from the 1999 publication.
►► To realize that I prefer .4, the authors would have had to go by the original 1996 NBER Working Papers with Romer, or subsequent papers, e.g., an update with Rose in the *QJE* (2002).
►► The *AER* paper focused on estimates that did not control for saving, human capital, or lagged income, in the Hall-Jones tradition; whereas the other papers reported estimates that did control for all these variables, in the tradition of Mankiw, Romer & Weil.
►► I worry about misattributing roles of saving & education to trade.
►► It seems adjustment would put trade effects below direct costs of war.
►► Incidentally, when I use the .4 estimate to calculate effects of multilateral trade liberalization, I get welfare effects double those in CGE models.
The authors treat the major direct cost of war as the loss of human capital

- I am fully prepared to put a dollar cost on human life (or, rather, to let someone like Viscusi do it), but I am not prepared to limit it to the present discounted value of future earnings.

- The authors follow Goldin in Lewis in valuing a life at the wage. By this logic, does the life of a retired person have no value?

- Admittedly this is much better than the way Bogart (1920) counts human capital, which is the investment of child-rearing, in which case an infant’s life has no value.
Loss of human life, continued

► Don’t economists who value human life now go beyond human capital?
► Don’t governments standardly adopt measures – decisions about, for example, the safety of highways they build -- that entail higher figures?
► Or, if workers in high-risk occupations like construction require wages in excess of other blue-collar jobs to compensate for a small increased probability of death, then
  ▪ the wage going with a high probability of death should be higher,
  ▪ and the value of certain death should be much higher still.
What is the counterfactual to war?

If one were to think of it as a policy question — in the face of some provocation do we go to war or not — the option that might appear at the top of the list of alternative policy options would be economic sanctions.

- Examples in US history run from the 1808 Embargo against England, preceding the War of 1812, to the sanctions against Saddam Hussein, preceding the 2003 invasion of Iraq.
- Whatever one thinks of those two wars, one might consider that the alternative also involved a sharp disruption of trade.
Thus there are three grounds on which I fear that the trade effects of war are less likely than the authors believe to equal or exceed the direct effects:

- (1) I would prefer to value human wealth at more than the value of the human capita;
- (2) in an actual decision setting trade might be lost even without war; and
- (3) I fear that their estimate of the effect of trade on welfare may be too high, notwithstanding that they got it from me.
A technical issue, regarding the proper interpretation of gravity models

- Van Wincoop criticizes others’ application of gravity estimates, specifically in the context of the way that unions promote bilateral trade among the member countries or regions.

- The criticism: in the theory from which the gravity model is most frequently derived (Helpman-Krugman trade in imperfect substitutes), a reduction in trade costs among regions of a union diverts trade away from non-members, so that a general equilibrium analysis gives different answers.

- I hadn’t been aware of the Baier & Bergstrand (2006) technique, which sounds a tractable way of implementing van Wincoop.

- It’s a welcome innovation. Glick & Taylor do the right thing by reporting results both with and without this.

- Van Wincoop and Baldwin say that any analysis that does not fully incorporate the theoretical trade-diversion is at best worthless.

- But trade diversion does not seem to take place empirically.

- Can the authors can shed any light in the current context, in the way of testing the van Wincoop constraint rather than imposing it?