Towards a Scientific Approach to Identifying and Understanding Indicators of Radicalization and Terrorist Intent: Eight Key Problems[1]

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Collectively the articles contained within this issue offer a lot of interesting and insightful material on radicalization/violence indicators and the validity of the TRAP-18. They do this through testing the tool against a medium-n sample of 22 cases (Reid, Rohsi, Glaz-Ocik and Hoffman), and individual case studies ranging from the very famous (Breivik in Meloy, Habermey & Guldimann), to the not so famous ('U' in Bockler, Hoffman and Zick) to the barely remembered (Lucheni in Van Der Meer). They make important methodological contributions (discussions surrounding inter-coder reliability) and substantive contributions in terms of new data generation and providing granular level detail on a couple of largely overlooked and unstudied case studies. The results illustrate that time and again, various distal and proximal indicators built into the TRAP-18 were apparent. So, has the problem been solved or are there further avenues that need exploring?

To my mind, the study of indicators of radicalization and terrorist intent suffers from eight key problems that need surmounting before we can become more assured in such tools. The papers within this issue offer some key clues in how these can be overcome and I refer to them as I work through the eight problems.

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[1] This is an amended version of a talk originally given at a conference on 'Radicalization and Violent Extremism: Lessons Learned from Canada, the UK and the US Conference', Arlington, VA. (July, 2015).

It is not my intention to denigrate existing approaches, but rather to try set out a roadmap to where we go from here.

The first problem is the sheer number of supposed radicalization indicators that abound within the literature. At times, it seems there are more indicators than actual domestic terrorists. Previous work carried out on lone-actor terrorists identified over 100 indicators of radicalization or terrorist intent within the larger literature (which itself has grown since that study) (Gill et al., 2014). If researchers were to standardize codebooks it may lead to a crowd-sourced answer as to which indicators are most prevalent across a wide range of data collection processes and research endeavors.

The second problem, which should be obvious to anyone with even minor social science training is that of base rates. Quite simply, we have no grasp on what the societal prevalence of the vast majority of radicalization indicators. In some cases, like issues surrounding mental disorders, it is easier because of epidemiological studies (see Corner, Gill and Mason, *forthcoming*), In other cases, like leakage, this is a really difficult task to quantify. Without a sense of base rate, we can't measure with any certainty how reliable any one indicator is, either in isolation or in combination with other indicators. Instead, we can only sample on the dependent variable which is not great practice. Criminology largely overcame such problems through longitudinal cohort studies and trace aspects related to the individual's development from an early age, personality factors, their interactions with the criminal justice system and so on. These are

obviously long-term projects that do not suit the immediacy of the problem of radicalization but their potential benefits to the literature are incalculable.

On a related note, the study of radicalization indicators is often misunderstood as ultimately producing predictive utility. This is not the case and is neatly outlined by Meloy et al. in this issue: "it will never be a predictive measure of targeted violence risk due to the very low base rate for such behaviors, even in a population of concern". Instead, these studies are meant as a tool to help triage amongst a number of cases displaying worrying signs. Not every lead or every case can be assigned full priority, so these measures, tools and protocols are developed with the intention of assigning low-, medium- or high-risk to the cases/leads based on our experience of previous cases. They are not meant to look at a random sample of the population and pick out specific individuals but rather to help triage between a sample of people who have already been deemed by someone else (be it an intelligence analyst, a member of a partner agency like mental health or a member of the public) as being a risk to society.

The third problem is related; our lack of understanding around protective factors. Simply, we just do not account for them. We only look for 'risk factors' which may lead to a series of confirmation biases amongst intelligence analysts. Protective factors may come in many forms and include individual factors (e.g. attitudes, academic achievement, social orientation, self-control, personality factors), peer factors (e.g. close relationships with non-criminal peers, pro-social norms within peer group, number of affective relationships), family factors (e.g. highly connected to family, involvement in social activities).

One related concept might be that of opportunity. Two of the case studies in this issue highlight the key role of opportunity in moderating action. For example, in the German case study, 'U' works at an airport frequented regularly by travelling U.S. army personnel. 'U' therefore had access to this particular target group on a regular basis and the capability to attack them through access to firearms via his brother and also because presumably airport staff do not go through the same rigorous screening process at airports that travellers go through. Without any of these factors (e.g. access to potential victims, access to firearms, capability of getting firearm to where the potential victims frequented), it is unlikely the attack would occur. It seems 'U's grievance was directly related to the actions of U.S. army personnel (see the section on the videos he consumed and what he thought he overhead U.S. army personnel talking to each other about), so displacement of his attack to the 'West' in general, may seem less likely in the case he couldn't attack U.S. army personnel. The Lucheni case study, outlined by van der Meer (*this issue*), also implicitly highlights the importance of opportunity. Lucheni held a grievance against royalty in general and held no deep conviction about who in particular should be targeted. He initially zoned in on the Duke of Orleans and the Italian King. The former cancelled his visit to Geneva (where Lucheni resided). The latter was located too far away. It appears that Lucheni only turned his attention toward his victim, the Empress of Austria, the day before the attack when he learnt, via a newspaper, that she was staying in a hotel in the town he lived. Not only was she geographically accessible, but she was also quite idiosyncratic in her loose security regime and disregard for close protection (as detailed in the case study) providing further opportunity for action.

Not every lone actor will be as meticulous or willing to change plans irrespective of opportunity like Anders Breivik (see the Meloy, Habermey & Guldimann case study, *this issue*). Some will be fixated on one key attack, largely facilitated by access to an opportunity. If the opportunity goes away, so too may their intent to attack. Intelligence agencies with access to cases where persons of interest developed a plan, were disrupted, and later abandoned their intent should re-examine those cases to see what was different about these individuals compared to those who bounce from Plan A to Plan B to Plan C and so on as highlighted by Gill (2015).

The fourth problem is that of weighting. In most studies of radicalization indicators, all indicators are treated equally. For example, the Safire Project (http://www.safire-project-results.eu/documents/focus/8.pdf) outlines 21 indicators, ranging from "lingering concerns with questions of meaning and identity" to "dependence on communication technology" to "associating with extremist groups" and "training travel". The first two indicators are clearly very different in scope, nature and threat than the latter two yet they appear weighted equally within their toolkit. One of the particular innovations within the TRAP-18 is that it splits these indicators into two categories – distal and proximal – and outlines how their relative presence should inform different responses. "The presence of distal characteristics of the TRAP should bring attention to a subject for monitoring. The presence of warning behaviors should focus the work on active risk management" (Meloy et al). Bockler, Hoffman and Zick (*this issue*) go a step further, and state that the presence of one proximal

indicators necessitates instant. This is an interesting, logical and yet rarely made argument. Of course, in reality not all indicators are equal. A part of the problem is that the study of indicators is almost trying to do too much – from highlighting indicators of someone adopting an extremist ideology to highlighting indicators of someone planning an attack. These are very different processes, underpinned by very different behaviours and necessitating intervention by very different parts of the policing/intelligence/partner agency framework. The radicalization literature is nowhere near specific enough in terms of what it is studying the indicators of. It can learn much from contemporary threat management literature.

The fifth problem is related to behavior clustering. Bockler, Hoffman and Zick's case study of the Frankfurt Airport Attack is a great example of how risk crystalises. There is no silver bullet indicator. Instead, it is a story of how the experience of one risk factor led to the experience of another risk factor and so on. Each acted as a force multiplier upon each other and led the individual to the point of engaging in a terrorist act. Whilst the four papers in this issue make an excellent job of outlining the prevalence of the different 18 factors, it might now be time to look at which factors are (a) more likely to co-occur together and (b) whether some of these factors act as substitutes for each other rather than co-occur together. Various forms of multi-dimensional scaling and cluster analyses will help bring the analysis from the descriptive, to the inferential and mirror paradigm shifts witnessed in the study of other offenders like arsonists (REF).

The sixth problem is related to aggregated understandings of the 'terrorist'. There has been some recent improvements in the academic literature here. Sub-set comparisons are now much more common. For example, Merari (2010) found suicide bombers to display symptoms of depression more often than suicide bomber organizers. Gruenewald et al. (2013) illustrated that extreme right-wing lone actors more likely display mental disorders than extreme right-wing group actors. Gill et al.'s (2014) sample of extreme right-wing lone actors were significantly less educated, less likely to conduct dry-runs and less likely to leak aspects of intent than their al-Qaeda-inspired lone actor counterparts. Finally, Corner and Gill (2015) found lone actors with mental health disorders were significantly more likely to experience recent stressors than those lone actors without mental health problems. LaFree (2013) refers to sub-set comparisons as the "third and final development on the road to the empirical study of terrorist attacks". He may be optimistic in calling it the "final" development however given the range of concerns outlined in this paper.

The seventh problem relates to sequencing these behaviors. Case studies are a particularly fruitful tool in this regard but perhaps lack the generalizability of other approaches. Quantifying these sequences is possible. By sequencing the behaviours we might get a sense of how long it takes to move through the gears from adopting a radical ideology toward attack planning and ultimately carrying out the attack. We get some good insight from isolated case studies. Bockler et al. note that case study's "help us understand both the unique characteristics and commonly shared attributes of those who are ideologically motivated to commit

violence toward noncombatants. They often allow for a deep dive into the distal and proximal variables which contributed to the targeted violence." For example, this issue's German case study notes: "we observed only a very brief and accelerated pathway towards the violent act. This represents an important change in the demands placed upon threat assessors." They continue: "the actual pathway to violence, primed by his immersion in Salafist ideology, did not unfold over the course of weeks or months, but in a day or two. This represents an important change in the demands placed upon threat assessors: there is much less time to find and then interdict along the late stages of the pathway (planning, preparation, implementation), in contrast to the relatively slower proximal behaviors of fixation on a cause and identification as a "soldier" for the cause, which still appear to take months, if not years, to fully develop."

The recent case of Brustholm Ziamani appears just as quick. Three months prior to stepping out his door with a backpack filled with an ISIS flag, a hammer and a long knife, Ziamani was a Jehovah's Witness. In the space of three months, he converted, adopted a radical interpretation of Islam, decided to act, built the capability and came very close to carrying out an attack. The Breivik case study, on the other hand, appears to be a much slower pathway toward violent action (largely moderated by the technical sophistication needed to acquire and synthesize the materials for the bombing component of his plot). More analyses need to be conducted to illustrate whether one of these is the norm, the other the outlier or whether there is a recognizable number of trajectories into action (and whether these are moderated by attack type).

The final problem relates to taking time into account in a very different way. Methodologically speaking, the study of lone-actor terrorists and other such low base rate offenders are heavily informed by risk and threat assessment research on high-volume offenders. The latter typically use samples of offenders highly clustered in time. These approaches can pinpoint applicable risk factors to that particular cohort at that particular temporal period. Generalizability comes from replication studies utilizing different spatial and/or temporal cohorts that weed out non-replicated risk factors. However, the study of low base-rate offences does not have this assurance mechanism built in simply because they have such a low base-rate (see Gill et al., Under Review for further discussion). For example, in this issue Reid, Roshdi, Glaz-Ocik and Hoffman examine 22 offenders over a 36-year time period. This is quite typical within the wider research literature with studies having a offender-to-year ratio between 0.60 to 4.44 (Hempel, Meloy and Richards, 1999; Gill, 2015). Whilst these descriptive studies highlight risk factors, we largely overlook whether these findings are driven by temporal-cohorts or are uniformly distributed within the observational pool. By temporally analyzes the data, it may indicate that the behaviour's prevalence is actually decreasing (or increasing) over time. This has large implications for threat assessment and threat management protocols.

In a very short space of time, the empirical study of terrorist behavior has made some large steps with multiple data-driven, methodologically-rich projects producing a lot of insight. The literature is finally at a point where data access is not so much of a problem as it was previously. The next big challenges are essentially conceptual and hopefully this commentary can work towards

synthesizing and standardizing approaches across these multiple data-driven endeavors. To progress, we need think more carefully about base rates, protective factors, weighting/clustering risk factors, thinking about the 'terrorist' in a more nuanced way and take temporality into account.

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