

Build Stuff

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I have had this thought in my head for awhile – criminology research to me is almost all boring. Most of the recent advancement in academia is focused on making science more rigorous – more open methods, more experiments, stronger quasi-experimental designs. These are all good things, but to me still do not fundamentally change the practical implementation of our work.

Criminology research is myopically focused on *learning* something – I think this should be flipped, and the emphasis be on *doing* something. We should be building things to improve the crime and justice system.

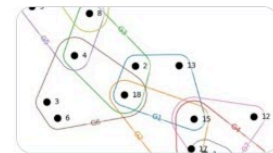
How criminology research typically goes

Here is a screenshot of the recent articles published in the [Journal of Quantitative Criminology](#). I think this is a pretty good cross-section of high quality, well respected research in criminology.

Opportunistic Organization of Illicit Supply Chains

Koen van der Zwet, Ana I. Barros ... Peter M. A. Sloot

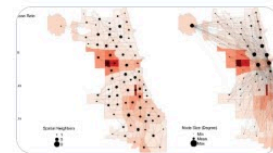
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Arrests and the Opioid Epidemic: An Investigation into the Spatial and Social Network Spillover of Opioid Overdoses in Chicago

Megan Evans, Corina Graif & Anna Newell

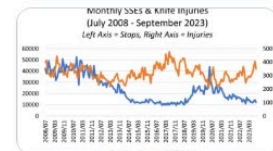
[Original Paper](#) | [Open access](#) | 31 May 2025



Did More Stop and Search by Police Cause Less Knife Injury in London? Evidence from 2008–2023

Alex R. Piquero & Lawrence W. Sherman

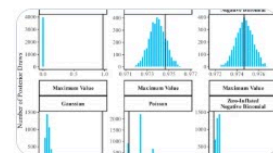
[Original Paper](#) | [Open access](#) | 19 May 2025



Using Bayesian Mixed Effect Generalized Linear Models to Evaluate Criminological Interventions: An Application to Firearm Seizures during Directed Patrol

Jason Rydberg, Emily Ann Greene-Colozzi ... Sean Perry

[Original Paper](#) | 06 May 2025



Three of the four articles are clearly ex-ante evaluations of different (pretty normal) policies/behavior by police and their subsequent downstream effects on crime and safety. They are all good papers, and knowing how effective a particular policy works (like stop and frisk, or firearm seizures) are good! But they are the literal example where the term ivory tower comes from – these are things happening in the world, and academics passively observe and say how well they are working. None of the academics in those papers were directly

involved in any boots on the ground application – they were things normal operations the police agencies in question were doing on their own.

Imagine someone said “I want to improve the criminal justice system”, and then “to accomplish this, I am going to passively observe what other people do, and tell them if it is effective or not”. This is almost 100% of what academics in criminology do.

The article on illicit supply chains is another one that bothers me – it is sneaky in the respect that many academics would say “ooh that is interesting and should be helpful” given its novelty. I challenge anyone to give a concrete example of how the findings in the article can be directly useful in any law enforcement context. Not hypothetical, “can be useful in targeting someone for investigation”, like literal “this specific group can do specific X to accomplish specific Y”. We have plenty of real problems with illicit supply chains – drug smuggling in and out of the US (recommend the contraband show on Amazon, who knew many manufactures smuggle weed from US out to the UK!). Fentanyl or methamphetamine production from base materials. Retail theft groups and selling online. Plenty of real problems.

Criminology articles tend to be littered with absurdly vague accusations that they can help operations. They almost always cannot.

So we have articles that are passive evaluations of policies other people thought up. I agree this is good, but who exactly comes up with the new stuff to try out? We just have to wait around and hope other people have good ideas and take the time to try them out. And then we have theoretical articles larping as useful in practice (since other academics are the ones reviewing the papers, and no one says “erm, that is nice but makes no sense for practical day to day usage”).

Some may say this is the way science is supposed to work. My response to that is I don’t know dude, go and look at what folks are doing in the engineering or computer science or biology department. They seem to manage both theoretical and practical advancements at the same time just fine and dandy.

Well what have you built Andy?

It is a fair critique if you say “most of your work is boring Andy”. Most of my work is the same “see how a policy works from the ivory tower”, but a few are more “build stuff”. [Examples of those include:](#)

- [making patrol areas](#) (also see [fairness constraints](#) in patrol)
- [prioritization for call-ins](#) (an actual algorithm to identify whom to call in in a focused deterrence intervention, not a hypothetical scenario)
- [simple rules for flagging outlying cases when monitoring crime series](#)

In the above examples, the one that *I know* has gotten the most traction are simple rules to identify crime spikes. I know because I have spent time demonstrating that work to various crime analysts across the country, and so many have told me “I use your Poisson Z-score Andy”. (A few have used the patrol area work as well, so I should be in the negative for carbon generation.)

Papers are not what matter though – papers are a distraction. The applications are what matter. The biggest waste currently in academic criminology work is peer reviewed papers. Our priorities as academics are totally backwards. We are evaluated on whether we get a paper published, we should be evaluated on whether we make the world a better place. Papers by themselves do not make the world a better place.

Instead of writing about things other people are doing and whether they work, we should spend *more* of our time trying to create things that improve the criminal justice system.

Some traditional academics may not agree with this – science is about formulating and testing hypotheses. This need not be in conflict with doing stuff. Have a theory about human nature, what better way to prove the theory than building something to attempt to change things for the better according to your theory. If it works in real life to accomplish things people care about guess what – other people will want to do it. You may even be able to sell it.

Examples of innovations I am excited about

Part of what prompted this was I was talking to a friend, and basically none of the things we were excited about have come from academic criminologists. I think a good exemplar of what I mean here is Anthony Tassone, the head of [Truleo](#). To be clear, this is not a dig but a complement, following some of Anthony's posts on social media ([LinkedIn](#), [X](#)), he is not a Rhodes Scholar. He is just some dude, building stuff for criminal justice agencies mostly using the recent advancements in LLMs.

For a few other examples of products I am excited about how they can improve criminal justice (I have no affiliations with these beyond I talk to people). [Polis](#) for evaluating body worn camera feeds. Dan Tatenko for [CaseX](#) is building an automated online crime reporting system that is much simpler to use. The folks at [Carbyne](#) (for 911 calls) are also doing some cool stuff. Matt White at [Multitude Insights](#) is building a SaaS app to better distribute BOLOs.

The folks at Polis (Brian Lande and Jon Wender) are the only two people in this list that have anything remotely to do with academic criminology. They each have PhDs (Brian in sociology and Jon in criminology). Although they were not tenure track professors, they are former/current police officers with PhDs. Dan at CaseX was a detective not that long ago. The folks at Carbyne I believe are have tech backgrounds. Matt has a military background, but pursued his start up after doing an MBA.

The reason I bring up Anthony Tassone is because when we as criminologists say we are going to passively evaluate what other people are doing, we are saying "we will just let tech people like Anthony make decisions on what real practitioners of criminal justice pursue". Again not a dig on Anthony – it is a good thing for people to build cool stuff and see if there is a market. My point is that if Anthony can do it, why not academic criminologists?

Rick Smith at Axon is another example. While Axon really got its dominate market due to conducted energy devices and then body worn cameras (so hardware), quite a bit of the current innovation at Axon is software. And Rick did not have a background in hardware engineering either, he just had an idea and built it.

Transferring over into professional software engineering since 2020, let me tell my fellow academics, you to can write software. It is more about having a good idea that actually impacts practice.

Where to next?

Since the day gig (working on fraud-waste-abuse in Medicaid claims) pays the bills, most of my build stuff is now focused on that. The technical skills to learn software engineering are currently not effectively taught in Criminal Justice PhD programs, but *they could be*. Writing a dissertation is way harder than learning to code.

While my [python book](#) has a major focus on data analysis, it is really the same skills to jump to more general software engineering. (I specifically wrote the book to cover more software engineering topics, like writing functions and managing environments, as most of the other python data science books lack that material.)

Skills gap is only part of the issue though. The second is supporting work that pursues building stuff. It is really just norms in the current academe that stop this from occurring now. People value papers, NIJ (at least used to) mostly fund very boring incremental work.

I discussed start ups (people dreaming and building their own stuff) and other larger established orgs (like Axon). Academics are in a prime position to pursue their own start ups, and most Universities have some support for this (see Joel Caplan and [Simsi](#) for an example of that path). Especially for software applications, there are few barriers. It is more about time and effort spent pursuing that.

I think the more interesting path is to get more academic criminologists working directly with software companies. I will drop a specific example since I am pretty sure he will not be offended, everyone would be better off if [Ian Adams](#) worked directly for one of these companies (the companies, Ian's take home pay, long term advancement in policing operations). Ian writes good papers – it would be better if Ian worked directly with the companies to make their tools better from the get go.

My friend I was discussing this with gave the example of Bell Labs. Software orgs could easily have professors take part time gigs with them directly, or just go work with them on sabbaticals. Axon *should* support something like that now.

While this post has been focused on software development, I think it could look similar for collaborating with criminal justice agencies directly. The economics will need to be slightly different (they do not have quite as much expendable capital to support academics, the ROI for private sector I think should be easily positive in the long run). But that I think that would probably be much more effective than the current grant based approach. (Just pay a professor directly to do stuff, instead of asking NIJ to indirectly support evaluation of something the police department has decided to already put into operation.)

Scientific revolutions are not happening in journal articles. They are happening by people building stuff and accomplishing things in the real world with those innovations.

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