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Problem-solving courts, specialized courts, early release programming, boot camps, sentencing circles, and other sentencing alternatives that effectively shorten incarceration durations are often touted as nearly miraculous panaceas for all that ails the criminal justice system. For example, their supporters and evaluators have variously claimed that these sentencing alternative programs: reduce recidivism, reduce incarceration costs, improve reintegration and reentry outcomes, re-unify families, and on a grand scale reduce overall criminal justice system costs. But do they really?

Are the creators, supporters, and evaluators of early release sentencing options sometimes falling afoul of the "DARE Syndrome"; that is, are they merely touting programs that feel good, sound good, have some logical appeal, but have not been objectively and reliably evaluated, considering all of the programs' true net costs and benefits? Once empirical research caught up with the hugely popular DARE programs, researchers determined there was no discernible benefit to DARE at all. After having spent perhaps millions of dollars on DARE nationwide, research finally determine it was all for naught. Once research catches up with some of these early release sentencing options, might they suffer the same fate?

On the plus side, many program evaluations of Minnesota sentencing alternatives rest on a solid empirical base, driven by such program evaluation experts as Grant Duwe and others. However, even here in Minnesota and its neighbors, some evaluations of sentencing alternative programs are more qualitative than quantitative and worse, are more marketing than assessment.

This short paper and presentation offer a few observations and suggestions for those social scientists who conduct these program evaluations and to criminal justice practitioners and decision-makers who must decide upon which programs to fund and to what degree. In particular, the focus here is on the potential pitfalls of evaluating sentencing alternative programs based solely or even predominantly on recidivism reduction for program participants.

What is Recidivism?

That seems a simple question, but the definition of recidivism is at the core of many of the program evaluation challenges in criminal justice. In spite of reams of scholarly work on recidivism, the research and practitioner communities have yet to settle on a single, universally accepted definition. Is it re-offense, re-arrest, or re-conviction? Is it over one month, three months, six months, one year, three years, or more? When does the clock start to tick: release from incarceration, release from supervision? Must the new crime involve the same family or category of crimes as the original offense? How serious must the new offense be to constitute a

recidivation offense? Is one recidivism offense enough to count? Where there are more than one recidivism offenses, how is that computationally handled in the program evaluation? And many more definitional questions linger. Thus, when one prepares a program evaluation of these programs and uses recidivism as a measure of program impact, one must carefully define the term. Similarly, when criminal just practitioners and decision-makers read these program evaluations, they must closely consider and bear in mind the recidivism definition the researcher used.

Evaluation Design: Experimental or Quasi-Experimental? It Matters.

Ideally, when evaluating the impact of a new program, the researcher wants to compare the program participants against a control group that as closely as possible matches the participant group in all material respects. So, when evaluating a drug court program, for example, the researcher would want to identify, say, 200 persons who would be eligible for that drug court opportunity, had committed similar offenses, with similar criminal histories, of similar socio-economic, racial, ethnic, and age characteristics, and soon. The more similar the better. Then, in an experimental design, the researcher would have half, that is, 100, of them participate in the drug court, and would preclude the other half – the other 100 (the control group) – from participation in the program. Assuming the researcher believes the program is beneficial to participants, it would be unethical to forbid program entry for half.

Instead, therefore, most researchers use a quasi-experimental design. Following that design, the researcher identifies 100 people who participated in the drug court program, then finds another 100, otherwise similar as above, but who did not participate in a drug court program to serve as the control group. This is where one of the data threats enters the analysis. Have we selected an appropriate and comparable control group? If so, comparisons between the participant group and the control group would be meaningful – apples to apples. If not, comparisons between participant and controls would not be meaningful – apples to oranges.

Evaluation Design: The Pre-Ordination Problem

Some alternative sentencing programs impose eligibility requirements some of which are express and some of which are implicit. And some of those requirements for eligibility and for successful completion of the program, coupled with scarce resources, beds, or slots in these programs, cause criminal justice decision makers to grant admission to those most likely to succeed in the alternative sentencing program and deny admission to those most likely to fail. Thus, in some cases, those pre-ordained to succeed in the alternative sentencing program are more likely to be admitted into the program. Thus, their better recidivism results compared to those not admitted to the program offers no useful measure of the program's impact. Those better recidivism results were pre-ordained.

Evaluation Design: The Volunteer (Self-Selection) Problem

Some alternative sentencing programs impose eligibility requirements some of which are explicit and some of which are implicit. For example, the La Crosse County Drug Treatment Court guidelines provide:

> The person, if accepted, must agree to abide by all Drug Treatment Court rules and modifications. Once a participant, he/she must agree to abide by any future rule modifications while they are in the Drug Treatment Court by signing the Drug Treatment Court contract and releases of information.

One can easily imagine a person, knowing they were not likely to successfully adhere to all those program rules, and persons who do not want to be impeded or controlled by such program rules, would choose not to enter the program. Still other such programs implicitly require adherence to strict testing, treatment, or counseling regimens that some who might otherwise participate in the program may find onerous or impossible to meet, and therefore may choose not to participate in the program. In other situations, the potential program applicant may choose to avoid the hassle and intrusion that some of these program require by not enrolling in the program.

Of course, the opposite situation also occurs. A person who is particularly rule-compliant may know that they would thrive in a program with copious rules. Or a person who expects to be able to complete treatment and remain abstinent without a hitch may relish the opportunity to enter the program to be successful in it.

The volunteer problem, as it relates to program evaluation, is a function of these realities. Persons who are not likely to succeed in the program or follow program rules and persons who resent control and oversight are more likely to decline participation in one of these alternative sentencing programs. Conversely, those most likely to succeed in the program, manage and adhere to the rules, and thrive under close supervision and intrusion are relatively more likely to enroll in an alternative sentencing program. The former group refuse to enter these programs; the latter group are the "volunteers."

Can Propensity Matching Correct for These Biases?

Researchers are increasingly using propensity matching in an attempt to normalize the participant and control groups. In short, propensity matching creates a control group from those who did not participate in the program by identifying a set of characteristics of the participant group then developing a control group comprised of people with similar characteristics. Often, those characteristics include age, race, and crime of conviction for those who might otherwise be eligible to participate in the alternative sentencing program but did not participate (because, among other things, they declined, they were not chosen to participate, or the program was not offered in the county where they were sentenced). An example makes the point. In the June 2012 Minnesota Statewide Adult Drug Court Evaluation, the control group was selected to include "1. Participants eligible for drug court but receiving traditional case processing during the same time period . . . 2. Participants eligible for drug court but who were from a different court within a court's jurisdiction from which individuals were not eligible to participate in the drug court [; and] 3. Participants eligible for drug court who had similar charges and were matched on certain characteristics."

Without more, however, this propensity matched control group from this 2012 Minnesota study violates both the pre-ordination and volunteer problems identified above. Further, this propensity matched control group was not really matched based on "propensity" at all. Indeed,

although that control group shared some similar characteristics with the drug court participant group, propensity to recidivate was not one of those similar characteristics. This was, inadvertently, an apples-to-oranges comparison, and any effort to credit the alternative sentencing program (in that case, Minnesota's drug courts) with recidivism impacts based on recidivism differences between the participant group and the control group is misguided.

That is not an indictment of this 2012 Minnesota drug court study, but is a cautionary tale for researchers and consumers of their research. If the control group is matched on time or crime of sentencing or age or race of offender, those matching characteristics need not reflect matched propensity at all. This sort of quasi-experimental approach, as the 2012 Minnesota researchers themselves acknowledged, is far inferior to a truly experimental approach. So then, how might a truly experimental approach be applied to allow researchers to truly measure the impact of an alternative sentencing program on recidivism?

By way of another example, a 2008 Duwe and Kirschner study of Minnesota's Challenge Incarceration (Boot Camp) Program ("CIP") created its control group using the following characteristics:

> The treatment [that is, participant] group consisted of individuals who participated in CIP from its inception in October 1992 through June 30, 2002.
>
> * * * * *
>
> The comparison group was composed of all individuals released from a Minnesota correctional facility from January 1, 1993 to December 31 who did not participate in CIP and were matched to individuals in the treatment group on the following variables: offender sex, offender race, offense type (property, drug, or other), metro area, length of stay, disciplinary history, age at release, age at first arrest, age at first felony conviction, age at first prison commitment, prior arrests, prior felony convictions, and prior prison commitments (N=1,555). The comparison excluded individuals who were incarcerated for sex and other person crimes or were discharged (as opposed to being placed on supervised release).

Consider whether using these characteristics to create a control group truly ensured that the CIP participant group and the control group were propensity-matched or if there may be a better way to ensure that.

The flaw of these quasi-experimental and propensity matching approaches is that researchers and practitioners may attribute positive impacts (such as recidivism reduction) to alternative sentencing programs that did not verifiably or actually cause those positive impacts (that is, where the program did not actually reduce recidivism for participants). The problem is not solely that the recidivism impact assessment is incorrect; the problem is that, quite often, dollar savings are assigned to those incorrect recidivism impact assessments, thereby overstating the savings flowing from alternative sentencing programs. That savings overstatement that afflicts some alternative sentencing program evaluations can fuel unfortunate government decisions to fund some programs over others based on savings that were a mirage – the program

costs will be real but the estimated program savings were not. Governments in tight economic times cannot afford to invest in programs based on flawed cost-benefit evaluations.

A Sample Experimental Approach to Recidivism Scoring for Minnesota Alternative Sentencing Programs

As noted by many researchers and program evaluators, an experimental approach (where participants and control group members are randomly selected from an appropriate universe) would be vastly superior to the quasi-experimental approaches used too frequently today to measure recidivism impacts flowing from alternative sentencing programs. The pitfall of experimental designs in this alternative sentencing program evaluation context, however, is similar to the pitfall when using experimental designs for any "treatment" the researchers a priori believe to be beneficial. That is, when a researcher randomly relegates an otherwise eligible participant to the control group, and thus denies that person the purportedly beneficial "treatment," how can that be done ethically? Is it ethical to deny the purportedly beneficial "treatment" to every other prospect? How about choosing based on alphabetical array by last name? Or could one ethically deny the beneficial "treatment" to those over or under a certain age? Or deny "treatment" to persons from one community or another?

This may seem like an intractable problem. However, I submit that the current situation with limited state budgetary support for some of these programs offers more opportunity than threat in terms of program evaluation. Let's say, for example, that combined state and county funds plus donations are available to support just 100 participants each year in an intensive, treatment-based court for adult drug offenders in County X, but at least 200 offenders in County X are qualified and interested in participating each year. In that situation, not unheard of in the current budgetary squeeze, one could randomly select the 100 participants from among the 200 qualifying and interested prospects without violating ethics, because funding absolutely restricted the maximum number of participants in County X. Now we have a usable and valid participant group and a usable, valid, and truly propensity- and characteristic-matched control group. By following that path, the researcher can follow the much more reliable experimental design (not a quasi-experimental design) approach, through which more valid comparisons between participant and control groups can be assured and by which much more accurate cost-benefit assessments of recidivism reduction can be computed to drive wiser public choices on which alternative sentencing projects are cost-effective and therefore are best to fund.

In the brief presentation at the 2017 Minnesota CJI, the presenter and participants will discuss these concepts, apply them to one or more actual programs and one or more paradigm programs, then will together discuss ethical paths for using true experimental methods to better assess alternative sentencing program successes at recidivism reduction.

Of course, there may be benefits to alternative sentencing programs aside from recidivism reduction (such as reducing the total cost to the criminal justice system by using these alternative sentencing programs rather than full incarceration); however, if one is using recidivism reduction as part of the purported "benefit" of these programs to compare against program costs, one must ensure that recidivism reduction is actually caused by the program

(rather than an artifact of control group selection methodology) and is accurately and objectively measured in the program evaluation.

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