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**Conclusion #9:** RJC's were cost-effective in all seven UK tests preventing more cost of crime in the short run of 2 years follow-up than the cost of delivering the RJC's, with far more cost-effectiveness among serious offenders with many prior convictions.

Similar cost-effectiveness estimates are not available for the RISE cases.

**Short- or long-term recidivism effects** The Jerry Lee Program has highlighted a central issue in evidence-based policy: how long is long enough, or too long, to measure outcome differences between treatments? While various authorities have recommended a 2-year *minimum* follow-up of any program randomly assigned to individuals, there is currently no discussion of a *maximum* period for follow-up. While our analyses show clear overall effects of RJC's on reducing recidivism at 2 years (Strang et al. 2013), the RISE analyses show that these benefits have disappeared by 15 years (Sherman et al. 2015a, b\*). These data suggest the following assessment:

**Conclusion #10.** While RJC's reduce recidivism for 2 years, analyses of the RISE evidence to date shows no main effect on recidivism after 15 or more years.

### *Victim benefits*

**Short-term victim benefits** The impact of RJC's on victims has been highly beneficial in both RISE and the UK experiments. Some of these findings have been quasi-experimental, before–after differences with the group of victims who attended conferences (Strang and Sherman 2003\*; Strang et al. 2006\*). The most important differences, however, have been based on experimental estimates (Angel 2005\*; Angel et al. 2014\*; Strang 2002\*; Sherman et al. 2005\*).

It was Strang (2002\*: 97) who first showed that RJC's reduced the percentage of victims of violence and property crime who feared that the offender would revictimize them, from 18 to 5 %. More importantly, she showed that RJC's reduced victims' desire for violent revenge (Strang 2002\*: 138–139) against the offenders, from 20 to 7 % (and from 45 to 9 % for victims of violent crimes only) (see also Sherman et al. 2005\*). Finally, she found that victims were more likely to be pleased with the way their case was dealt with if their offenders had been assigned to RJC's (69 %) than if they had been prosecuted (48 %).

**Conclusion #11:** Victims assigned to RJC's in RISE were less fearful of repeat attack by the same offenders, more pleased with the way their case was handled, and less desirous of violent revenge against their offenders than controls.

**UK tests** Short-term victim benefits of RJC were somewhat weaker in the UK evidence than they were in the RISE experiments. Shapland et al. (2007\*: 42) found slightly weaker effects in the UK experiments, when RJC only supplemented the CJ process, rather than substituting: 72 % of RJC-assigned victims were satisfied or very satisfied compared to 60 % of victims whose cases did not receive RJC. But the UK control group (CJ) victims (unlike the RISE CJ victims) had all expressed a willingness to meet with their offenders prior to random assignment, and had often reported disappointment to the constable who obtained their consent about their not being selected for RJC.

The Campbell Systematic Review (Strang et al. 2013\*) also incorporated the findings of eight sets of victim interviews by Strang and Angel, as first reported in Sherman et al. (2005\*); victims were far more likely to receive apologies in RJC than in conventional justice; the RJC-assigned victims were more likely to receive apologies they found to be sincere; they were no less likely to blame themselves for the crime than conventional justice-assigned victims; in the London experiments, the RJC-assigned victims were more likely to forgive their offenders than were the CJ-assigned; and across all eight results, victims were less likely to want violent revenge if they had been assigned to meet with their offenders than if not.

**Conclusion #12:** Victims assigned to RJC in both the UK and RISE were more likely than control group victims to receive offender apologies, be more satisfied with their justice, and less desirous of violent revenge than controls.

The most powerful evidence of victim benefit from RJC is the Angel et al. (2014\*) evidence that RJC reduce the post-traumatic stress symptoms (PTSS) reported by victims. Using a standard psychiatric diagnostic tool in telephone interviews of 192 London victims of robbery and burglary, the Angel team found 49 % fewer victims suffering clinical levels of PTSS among the RJC-assigned victims than among the victims assigned to CJ only. These findings were limited to short-term impact, but they reflect basic life functions such as sleep and ability to leave the home to go to work. They also imply a possible long-term reduction in an otherwise elevated risk of premature mortality, which has been associated with chronic PTSS, even at low levels (Kubzansky et al. 2007).

**Conclusion #13:** London robbery and burglary victims assigned to RJC suffered much less post-traumatic stress than controls.

### *Long-term victim benefits*

The evidence so far shows that victim benefits of RJC last longer than any effects on offender recidivism. While our only long-term victim effects data so far come from a 10-year post-random assignment survey for the RISE violence and property experiments, Strang's (2011\*) research team on this survey achieved a substantial panel response rate of 81 % ( $n = 188$  out of 232 initially interviewed), which was 72 % of 260 initially sought for interviews. After 10 years, the benefits for RJC-assigned victims

remained clear: they still had half as much *anxiety* about being revictimized as victims whose cases had been prosecuted (22 % RJ vs. 44 % court,  $p = .00$ ); half as much *anger* about the crime (58 % RJ vs. 26 % court disagreed that they were still angry,  $p = .01$ ); and half as much *feeling of bitterness* about offense (75 % RJC vs. 38 % court disagreed that they still felt bitter,  $p = .00$ ).

Other benefits for RJC-assigned victims, if borderline in statistical significance, were less general *fear of crime* (22 % RJC vs. 34 % prosecution,  $p = .11$ ), and more disagreement that they would do some *harm to offender now* (80 % RJC vs. 63 % prosecution strongly disagree,  $p = .10$ ).

Two measures that showed no difference between RJCs and court were (1) whether the treatment of their case had *put their minds at rest* (around 75 % of both RJC-assigned and prosecution-assigned said it had not) and (2) whether the victims felt *forgiveness* of the offender (20 % of both treatment groups remained unforgiving). But another, more subtle measure showed an important benefit for the RJC victims, who were more likely to have forgotten just what happened in the justice process they attended (47 %) than court-assigned victims who attended court (33 %).

**Conclusion #14:** Substantial victim benefits in reducing the emotional impact of the crime resulted from random assignment to RJCs in the two Canberra RISE tests and persisted for at least 10 years after the arrest of their offenders.

### Moderator effect findings so far

One strength of the Jerry Lee Program has been its capacity to detect important moderator effects: not just whether RJCs “work,” but *for whom* they work more or less well, or even make things worse. Such differences have been found to date for victim gender, offense severity, offender baseline offending frequency, offender drug use, and initially for race in Australia (Strang and Sherman 2015\*), although the latter appears to have disappeared in a 15-year follow-up (Sherman et al. 2015a, b\*) and will be reported in detail in a separate article.

#### *Post-traumatic stress reduction and gender*

If restorative justice were to be rationed on the basis of the greatest benefits it produces for victims, there is good evidence for prioritizing women. The Angel et al. (2014\*) analysis of the post-traumatic stress symptoms reduction in London showed that while RJCs reduced PTSS as a main effect, women victims had much higher PTSS levels after burglary and robbery victimizations than male victims did. They also showed much more PTSS reduction after RJCs than men: 46 % were above subclinical levels of PTSS in the female RJC-assigned group compared to 78 % for female controls, while men only had a difference of 37 % RJC-assigned versus 45 % for controls.

**Conclusion #15:** Female victims of robbery and burglary in London had much greater short-term reductions in PTSS levels than male victims, although both genders showed benefits of RJC on PTSS.

### *Repeat offending and offense severity*

The Strang et al. (2013\*) systematic review of RJC effects on recidivism included a moderator analysis by offense severity. The biggest effect of any moderator in that analysis (including offender age, time at risk, use of conviction outcomes only or including arrests) was the interaction of RJC with offense severity. The concept of severity was crudely indicated by the instant case being for either a violent crime or a property crime. While one of the RCTs included in the Systematic Review was not part of the Jerry Lee Program, that RCT (McGarrell and Hipple 2007; Jeong et al. 2012) used the same trainers as all the Jerry Lee Program experiments. The overall standardized mean difference in 2-year frequency repeat offending was  $D = -.163$  ( $P = .001$ ), yet the same measure for only the three property crime-only experiments was  $D = .001$  ( $P = .989$ ). The meta-analysis of the five violent crime RCTs, however, yielded a standardized mean difference in favor of the RJC of  $D = -.198$  ( $P = .045$ ). Thus, it seems fair to say that in general:

**Conclusion #16:** The average effect of RJC (compared to CJ) on repeat offending across all three reported property crime experiments was nil, while the average effect of RJC across five experiments with violent crime was a modest but statistically significant reduction in the frequency of repeat offending.

### *Repeat offending and offender baseline frequency*

Another issue in using RJC is whether it is best used only for first offenders (as often claimed), and inappropriate with high-frequency offenders since for them it is “too late”: they have become “hardened criminals.” The evidence from the Jerry Lee Program in two hemispheres shows exactly the opposite.

Both the Canberra (Woods 2009\*) and London experiments (Bennett 2008\*) provide consistent evidence on how RJC effects vary by baseline offending frequency. Analyses in both cities use arrest frequency over a 5-year period prior to random assignment as the baseline rate of offending. The repeat offending measure in Canberra was arrest frequency in a 5-year follow-up; in London, it was time-to-failure from random assignment (or prison release) to date of first offense resulting in arrest in the time period 2002 through 2005. In both cities, the evidence shows that RJC effectiveness appears to be curvilinear: they work best for offenders with the highest and lowest frequency of prior offending. RJC work least well for offenders with a moderate frequency of prior arrests.

Sarah Bennett’s (2008\*) analysis of offender time-to-failure in the two London experiments found no statistically significant differences between the RJC-assigned offenders and those equally willing to meet with consenting victims randomly assigned to the control group. “Failure time” in Bennett’s analyses was the number of days between release from prison (or random assignment date for those not in custody) and the date of the first offense that led to an arrest (Bennett 2008\*: 79). This “crime-free” period was actually longer for RJC cases (compared to controls) in both experiments (Bennett 2008\*: 82), especially in the robbery experiment (522 days for RJC vs. 371 days for controls), but the differences had very wide confidence intervals (range

of error). Yet, since only 61 % of the sample offenders had any rearrest during the follow-up period ending December 31, 2005, there was substantial variation to explain.

When Bennett specified more homogeneous subgroups of the experimental samples, more than a “chance” number of subgroups showed statistically significant differences between the RJC and control groups in time-to-failure. This phenomenon may be an example of Weisburd et al.’s (1993) paradox, in which smaller sample sizes are more likely than larger samples to produce statistically significant differences because smaller samples may be less heterogeneous, with smaller standard deviations. The most important instance of this was the level of baseline frequency of arrest.

First, Cox regression results indicated that the frequency of arrests in the 5 years prior to random assignment had a statistically significant interaction effect with RJC and time to failure (Bennett 2008\*: 159), in both the burglary experiment ( $n=227$ ) and the robbery and burglary experiments combined ( $P<.0001$ ). She defined high frequency offenders as those with a mean of over seven arrests per year at risk in the 5-year pre-random assignment baseline period. These high-frequency offenders had a mean of 94 days to first offense in the control condition, but 234 days (a 149 % increase) in the experimental condition (Bennett 2008\*:160).

Second, Bennett (2008\*: 160) found that London robbery offenders ( $n=128$ ) showed the same pattern. Offenders with a baseline rate of over seven arrests per year for 5 years before pleading guilty to a robbery charge had over twice the mean survival time after random assignment to an RJC (316 days) than after assignment to CJ (140 days).

In the same experiments, however, Bennett (2008\*:160) also found evidence that RJC worked better to delay repeat offending if they had the *lowest* baseline rates of arrest than if they had *medium* rates. She defined the lowest rates of baseline arrests as less than two arrests per year, and medium rates as between two and seven arrests per year, in the 5 years prior to date of random assignment. Robbery offenders with the lowest baseline rates had a mean survival time of 382 days in the control and 634 days in the RJC-assigned condition, or a statistically significant 66 % increase in time to first repeat offense (see Fig. 1). A significant increase in failure time for lowest baseline-rate burglary offenders was in the same direction, but much smaller: 507 days over 474 days (7 % more).

Bennett’s (2008: 160) London analysis also found evidence against using RJC for medium rate offenders (2–7 arrests per year in baseline). Medium baseline-rate offenders in burglary had only a 13 % increase in failure time after assignment to RJC. Even worse, medium-rate robbers had a statistically non-significant, but *backfiring* effect from RJC—which cut their mean time to failure from 350 days for controls to 219 days for RJC (a 37 % reduction, or a 60 % benefit from *not* using restorative justice).

Daniel Woods’ (2009\*) analysis of the three RISE experiments that included juvenile offenders ( $n=512$ ) discovered a strikingly consistent replication of the patterns Bennett (2008\*) found with burglary and robbery offenders in London. While the mean frequency of arrests in the RISE 5-year baselines (about two arrests per year for crimes with personal victims in the highest-frequency trajectory, and less than one per year in the lowest) was far lower than in the London tests, RISE also showed a curvilinear pattern of RJC working better on high-rate and low-rate offenders than medium-rate offenders. Using an even longer follow-up period in Canberra than Bennett could use in

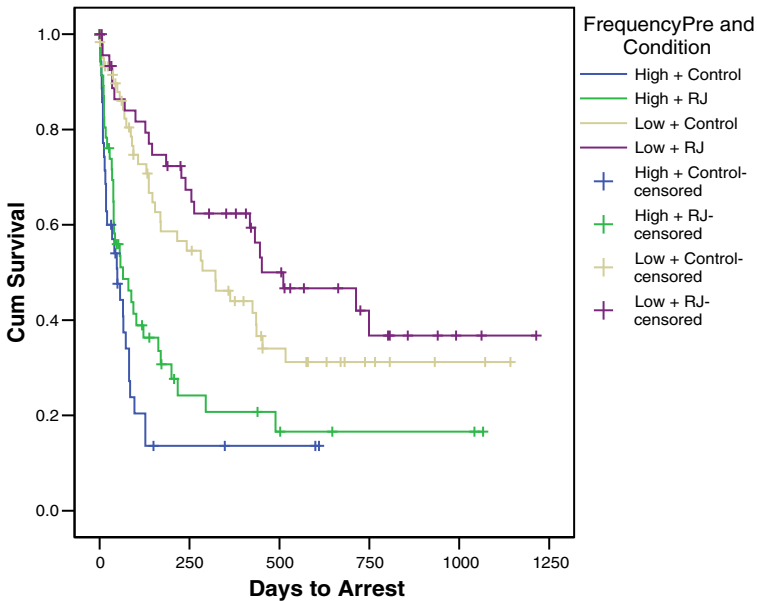


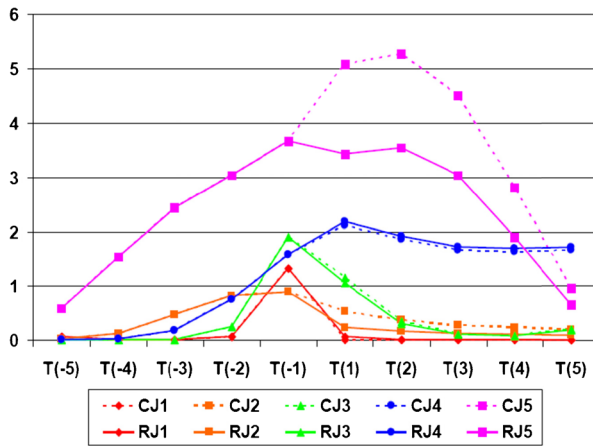
Fig. 1 Source: Bennett 2008\*: 162

London (a 5-year follow-up after the 5-year baseline for all Canberra cases, for a total of 10 years of measurement), Woods used annual frequency of arrests of a specific kind (rather than time-to-failure for any new offense, as in London) as the outcome measure.

Woods (2009\*) grouped all offenders in the three RISE experiments with juveniles into six trajectories of frequency of arrests for *crimes with personal victims only* (using trajectory analysis as described by Nagin 2005). His premise was that the RJC emphasis on empathy with victim suffering would be best tested by its impact on crimes against victims, as opposed to drug possession, drink-driving and other offenses without personal victims.

Woods then adjusted for the moderating effects of restorative justice with Aboriginal versus non-Aboriginal offenders, which led to his omitting all of the Aboriginal offenders from his final trajectory analysis, including two outlier cases that later analysis suggested to be driving overall findings about Aboriginals (Sherman et al. 2015a\*, b\*). Woods' decision in 2009 had the effect of reversing an initial (1 year after random assignment) increase in arrest frequency among highest-frequency offenders receiving RJCs (as Fig. 2 shows in the solid line rather than the dotted line controls in the same trajectory group). This procedure showed the biggest benefits of RJCs in reducing recidivism frequency among the most frequent offenders in the baseline period.

**Conclusion #17:** In three RISE tests and the robbery and burglary experiments in London, RJCs had the biggest effects on reducing recidivism on those offenders who had the highest rates of offending in the baseline period, and modest effects on very low-rate or first offenders, but was ineffective or criminogenic for those offenders with medium rates of offending in the baseline period.



**Fig. 2** Source: Woods 2009\*: non-aboriginal subsample—impact of *RJ* vs. *CJ* on arrests for crimes with personal victims in three RISE Experiments (violence, property crime and shoplifting)

*Repeat offending and offender multiple drug use*

The link between drugs and crime is perhaps most hotly debated when discussing justice for drug-using offenders. The complexity of that debate runs into moderator effects on justice with offenders using different kinds of drugs one-at-a-time, or the difference between people using only one kind of drug vs. two or more kinds of illicit drugs simultaneously. Bennett (2008\*: 202–204) used this discussion to examine any moderator effects of drug use patterns of the effects of RJC on time-to-failure. She found the London experiments offered a good opportunity. While 89 % of the London robbery and burglary offenders were reported to be using drugs at the time of arrest, only 53 % of burglars and 37 % of robbers were using both crack cocaine and heroin (combined  $n = 152$ ). For those who did *not* use *both* crack and heroin, assignment to an RJC raised the mean days to first offense by 26 %, from 355 days to 447. But for offenders who *did* use *both* heroin and crack, assignment to an RJC backfired, by reducing their time to failure 29 %. The mean number of days at risk to first offense was 340 in the control group, but only 242 in the RJC group. The evidence thus supports this assessment:

**Conclusion #18:** London offenders who used both crack and heroin reoffended more quickly if they had been assigned to RJC than to controls, but offenders who did not use that combination of drugs reoffended more slowly if they were assigned to RJC than to controls.

*Race and restorative justice*

Early evidence in RISE suggested that RJC had been criminogenic for Aboriginal offenders (Strang and Sherman 2015\*). Subsequent analyses have called this conclusion into question (Sherman et al. 2015a\*, b\*) and will be the subject of a detailed analysis in a future report.



## Discussion: more work to be done

It seems unlikely that the 18 conclusions distilled in this review would have been produced in an ad hoc, one-RCT-at-a-time collection of experiments. The conclusions repeatedly draw on comparisons of answers to similar research questions across different kinds of offenses, offenders, and stages of the criminal process, as well as different countries. The external validity of the collective findings when analyzed in this fashion would seem to be far greater than what might be possible with 12 different experiments done by different research teams and organizations. That said, the addition of the independent evaluators in the UK experiments, combined with a standard approach to experimental design by the Jerry Lee Program, adds extra credibility to the external validity of the patterns (see Eisner 2009; Sherman and Strang 2009b). Given the frequent lack of any replication of policy experiments, with too many variations in practices being tested (and control groups compared to them) even when experiments are repeated, the Jerry Lee Program has clearly been different.

With this compilation of findings as an example, we are now able to make a stronger case in favor of governments and foundations obtaining greater benefits from a program of RCTs, rather than providing the same amount of funding for an ad hoc collection of experiments. Yet we must also ask whether we have made the most of the opportunity provided to us by a 12-RCT program. We can answer that question by reflecting on what else might be done with evidence from the Program, and specifically what we can aim to accomplish in the near-term.

There seems to be sound argument for three priorities: (1) we should publish more theoretically-focused articles or books that would feed the academic appetite for advancing theories, and not just facts, about crime and justice; (2) we should produce more highly specific manuals for practitioners, or “field guides” for how to create “suction” of criminal cases into RJs in different settings; and (3) we should push even harder to test RJs in more controversial areas, such as serious crimes, where our evidence shows that the benefits in harm reduction would be far greater for crime victims than where it is currently used.

## But how does it work in theory?

One obvious way to get knowledge into practice is to make the knowledge more central academically, not just professionally. This is obvious because academics are the primary knowledge brokers on crime policy. While the professional or political demand for knowledge about justice innovations may not be great, the opportunities to supply knowledge may be heavily concentrated in the hands of university-based criminologists. These scholars not only advise the media and their local justice agencies on their opinions of what works. Academics also shape the views of tens of thousands of students who may go on to make and deliver justice policies.

Despite the 75 publications listed in the [Appendix](#), the Jerry Lee Program has arguably made little dent in academic thinking about justice innovations. Had at least some of the publications taken a more explicitly theoretical approach, there may have been more attention paid to restorative justice in undergraduate courses on the criminal justice processes. There might even have been more academically-initiated experiments

and research on RJs in a wider range of jurisdictions, offense types, and stages of the criminal justice process.

How do we know there has been little academic impact of the findings to date? One indicator is as simple as Google Scholar citation counts. Of the top ten publications listed when the words “Restorative Justice” are entered into Google Scholar, only three contain data from the Jerry Lee Program. Of those three, the highest citation count (1642 since a 2002 publication, or 130 Citations per year) is for the most theoretically elaborated interpretation of the experimental evidence (Braithwaite 2002). Other highly cited work is also more theoretical than the majority of the publications we have produced, which emphasize the empirical results over their theoretical meaning.

Why is it so important to use theory to gain academic attention and credibility? The answer is not limited to academics. The desire for understanding *why* something is true (Tilly 2006) is quite general, and may affect people’s willingness to believe that something really *is* true. Closely related to the desire to know why is a preference for *stories* over statistics, as the key funder of our Program, the radio broadcasting entrepreneur Jerry Lee of Philadelphia, has so often said. Stories about people provide a narrative that allows readers of any background to empathize with anyone—including offenders or victims who have been offered or denied RJs. A decade ago, we suggested the power of experimental ethnography, as a marriage of quantitative and qualitative methods, to address this appetite (Sherman and Strang 2004a). Yet, we have so far not produced a rigorously theoretical, let alone a qualitative–quantitative, analysis of our programmatic evidence in a mainstream peer-reviewed criminology or social science journal.

### **A field guide to getting criminal cases**

At the opposite end of the continuum of theory to practice, we have failed to provide enough how-to-do-it instruction for practitioners. The need for such guidance is evident in every new initiative that is funded to provide restorative justice. Every such initiative of which we have heard has crashed against a wall of too few cases being offered for a program to be viable. Even the initiatives funded by the Home Office in 2001 that were not RCTs faced far greater difficulties than we did in generating cases that were dealt with by restorative justice.

We arguably have a lot of ‘good practice’ to share, at least in terms of implementation. Including our UK (non-controlled) Phase I practice cases, the Jerry Lee Program in 2001–2005 recruited over 1000 cases in which both offenders and victims agreed to meet (some 400 of which were randomly assigned to control groups). As far as we know, no other organization has ever produced 1000 cases in which full agreement was reached to conduct RJs. How we did it is something that can be spelled out, but it is usually too detailed for academic or scientific publications.

A case in point was recently suggested by the experience of the post-2013 legislative authorization of Judges adjourning cases for RJs prior to sentencing in Crown Court. That is exactly what we had tested in London in 2001–2005, obtaining some 500 cases of agreements by victims and offenders. Yet when Home Office funding was provided in 2014–15, the practitioners could hardly extract any cases from the Crown Court in which to conduct RJs (Collins 2015). Why was it so much harder to get cases in normal practice than in our tests?

The best explanation appears to be the decision of Judges supervising RJC in 2014 to diverge substantially from our practice in 2001–2005. They required that in order to conduct an RJC between guilty plea and sentence, the victim had to agree to do so even *before* the offender had pled guilty—which many of them do at the last minute. Not only did the RJ staff have zero time to ask the victims in the latecomer cases, they also could rarely assure victims that the offender was planning to plead guilty, nor could they say whether the offender was willing to meet with their victim. This system differed from what we tested in at least three respects: (1) we had been allowed time by Judges after each guilty plea to go *first* to the offenders, and only *second* to the victims, to seek consent for an RJC; (2) we had police officers, rather than “civilians,” approaching both offenders and victims for consent; and (3) we offered the assurance that the RJC itself would also be conducted by a police officer, which may have inspired some confidence in both offenders and victims that they would be protected from physical violence or other disorder by a police presence.

These details may seem petty, but they could also be the small things that make a big difference, the tipping points between getting cases or not getting cases. In justice experiments, the importance of conducting programs in *exactly the same administrative system* as they have been tested in RCTs is not widely understood. In contrast to medicine, where every tiny step of a medical procedure or pharmacological treatment is micro-managed, justice systems tend to be highly variable. There is no tradition in justice of worrying about little things making a difference, even though they might.

To be fair to the Judges in 2014, however, they could ask the Jerry Lee Program a very good question: “Why did you not write up the exact methods you used in successfully suctioning 1000 cases into RJC?” The answer is less important than the premise. The fact is that we did not spell out the procedures we used at the level of detail necessary for anyone to codify “best practice” for implementation. We did touch on it in a kind of field guide for youth justice practices (Sherman et al. 2008), but we did not produce field guides specific to different settings, such as Crown Courts. Nor did we pursue the issue of police versus civilians in their ability to recruit victims and offenders, which remains a key policy and funding issue in delivering RJC. Nor, in fact, did we offer to provide seminars to Crown Court Judges after our research results were analyzed, despite general invitations from individual judges to do so, another lacuna we regret.

### **To each according to their need**

Perhaps the most serious critique of the Jerry Lee Program is that we have failed to convince policymakers that RJC are better used for serious cases and with chronic offenders than with minor crimes by juveniles and first offenders. Our unsystematic observation is that far more RJC are conducted with minor matters than with serious crimes and criminals. Our evidence shows that this is poor triage, giving RJC to people who have little need of it, and denying it to those whose need is greatest. If there is one conclusion that we should try to spread to a very wide audience, it is this one. How we can do that remains a question we cannot answer, except by the basic tools we use for all our work: grounded theory, trial and error, and systematic evidence.

It is not just the Jerry Lee Program that needs more knowledge about spreading knowledge effectively. It is all of experimental criminology, and science itself. This

article not only gives us a chance to reflect on how to put knowledge to work. It should give our readers the same opportunity, if only by thinking about how our Program could do better.

We close with one key plan for further research and analysis, driven in large part by the preceding discussion. The plan is to follow-up on the mortality differences between victims and offenders in the UK experiments, testing for any effects of RJC on life expectancy. Our evidence from 121 offenders under age 30 in one of the RISE tests is highly suggestive (Angel et al. 2013): while none of the 62 offenders randomly assigned (1995–2000) to the RJC group in the violence experiment had died by 2013, fully 10 % (6) of the 59 assigned to prosecution were dead (Fisher's Exact  $P=.01$ ). In the UK, we can explore similar questions for victims with psychiatric evidence on PTSS. If we are able to find medical evidence that lower PTSS levels predict longer life span, we may well get more attention from governments, judges and police. We must be mindful of the responsibility we have to pursue this question, with the fully identified records of over 2000 people in our safekeeping. It may well be that RJC, like other criminal justice decisions (Sherman and Harris 2013, 2015), could be a matter of life and death.

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## **Appendix: Publications arising from the Jerry Lee Program of Randomized Trials in Restorative Justice 1995–2015**

- Ahmed, E., Harris, N., Braithwaite, J., & Braithwaite, V. (2001). *Shame management through reintegration*. Melbourne: Cambridge University Press.
- Angel, C. M. (2005). *Crime victims meet their offenders: Testing the impact of restorative justice conferences on victims' post-traumatic stress symptoms*. Ph.D. Dissertation in Nursing and Criminology, University of Pennsylvania.
- Angel, C. M., Strang, H., Sherman, L. W., Bennett, S., Inkpen, N., Keane, A., & Richmond, T. (2008). *The impact of restorative justice on post-traumatic stress symptoms in male and female victims of serious crime*. Paper presented to the 15th World Congress of Criminology, Barcelona, Spain.
- Angel, C. M., Barnes, G. C., & Hyatt, J. With Strang H. & Sherman, L.W. (2013). *The effects of restorative justice on offender mortality, health, and welfare: A ten year follow-up*. Paper presented to the American Society of Criminology annual Meeting, Chicago, IL.
- Angel, C. M., Sherman, L. W., Strang, H., Ariel, B., Bennett, S., Inkpen, N., Keane, A., & Richmond, T. (2014). Short-term effects of restorative justice conferences on post-traumatic stress symptoms among

- robbery and burglary victims: a randomised, controlled trial. *Journal of Experimental Criminology*, 10, 291–307.
- Barnes, G. C. (1999). *Procedural justice in two contexts: Testing the fairness of diversionary conferencing for intoxicated drivers*. PhD Dissertation, University of Maryland, College Park, MD.
- Barnes, G. C., Hyatt, J. M., Angel, C. M., Strang, H., & Sherman, L. W. (2015). Are restorative justice conferences more fair than criminal courts? Comparing levels of observed procedural justice in the reintegrative shaming experiments (RISE). *Criminal Justice Policy Review*, 26, 103–130.
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