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Community-Informed Relationship Violence Intervention in a HighStress, Low-Income Urban Context

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Community-Informed Relationship Violence Intervention in a High-Stress, Low-Income Urban Context

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Objective: To evaluate the effectiveness of the House of Ruth Maryland's Gateway Project, a community-informed and oppression-sensitive relationship violence intervention program (RVIP; commonly labeled "batterer intervention"), designed for a predominantly low-income, racial minority population residing in a high-stress urban context. **Method:** Propensity score matching with data on 744 male program participants (89% Black; 59% unemployed; 76% on probation) was used to compare recidivism rates for those who did, and did not, complete the intervention program. The propensity score matching created comparison groups ($n = 216$ per group) with very similar distributions on 28 balancing factors. **Results:** During the year after program enrollment, program completers had significantly lower frequency of re-arrest for all criminal offenses, $d = 0.16$, $p = .018$ and marginally lower frequency of violent offenses, $d = 0.12$, $p = .075$ than matched non-completers. No treatment effect was identified for partner-abuse-related legal involvements, $d = 0.06$, $p = .365$. Secondary analyses controlling for propensity score in the full sample yielded similar results, and analyses of session attendance as a continuous variable found additional evidence of a significant program effect on violent offenses in the matched sample. **Conclusions:** In contrast to a carefully matched sample of program non-completers, men who completed this 28-session intervention, which adapts the traditional RVIP focus on power and control to address the life context of participants who experience systemic oppression, discrimination, economic distress, and community violence, had lower overall involvement with the criminal justice system.

Keywords: intimate partner violence; offender treatment; recidivism; systemic oppression; racism

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Despite three decades of research, many questions remain regarding the efficacy of psychoeducational and therapeutic programs for individuals who engage in intimate partner violence (Murphy & Richards, in press). Meta-analyses of controlled research on the efficacy of Relationship Violence Intervention Programs (RVIPs, also commonly labeled with the more stigmatizing and ambiguous term “batterer” intervention) have found that program attendance is associated with a small reduction in intimate partner violence (IPV) recidivism that is significant in some, but not all, analyses (Babcock et al., 2004; Cheng et al., 2019; Feder & Wilson, 2005). Recent studies have yielded encouraging results for several new and innovative RVIP approaches, highlighting the value of continued efforts to identify and disseminate effective intervention models (e.g., Lila et al., 2018; Mills et al., 2019; Taft et al., 2016; Zarling et al., 2019).

One of the major gaps in RVIP research to date has been the dearth of studies examining programs designed for specific populations who face unique stressors and challenges. Racial minority members residing in urban contexts, in particular, are exposed to a range of stressors that increase risk of IPV perpetration and engagement with the criminal justice system, including high unemployment, economic struggles, racial discrimination, over-policing and police misconduct, high rates of exposure to community violence and other traumatic stressors, and hopelessness (Holliday et al., 2019). A number of scholars have argued that prominent IPV intervention models are often insensitive to these cultural and contextual factors (Aymer, 2011; Gondolf & Williams, 2001; Hancock & Siu, 2013; Taft et al., 2009; Williams, 1998). Widely used RVIP approaches that focus primarily on gendered expressions of power and control locate male IPV offenders in a unitary position as oppressors, and may ignore or discount their experiences of trauma, marginalization, and systemic racism. In addition, the predominant conceptual models guiding RVIP practice may lead providers to misapprehend unique social class and cultural dimensions of identity and intimate relationship dynamics, further alienating or pathologizing poor and racial minority participants. Oliver Williams, a leading scholar in this area, argued that relationship violence interventions for Black men “must expand to include the ways in which social oppression and social learning from hostile community environments may result in violence toward women” (Williams, 1998, p. 85). Recognition of such concerns has led to the development of culture-centered practices and culturally-focused programs for Black and Latino

men who use violence in their relationships (e.g., Hancock & Siu, 2013; Parra-Cardona et al., 2013; Perilla & Perez, 2002; Williams, 1994). However, very little research has been conducted to evaluate the efficacy of these interventions.

The goal of the current study was to determine whether a community-informed and culturally-sensitive relationship violence intervention program can reduce criminal reoffending for individuals residing in high-stress urban contexts. Our study examines program outcomes for the House of Ruth Maryland’s Gateway Project. The study was conducted in Baltimore, a majority Black city with a long-documented history of systemic oppression of Black communities by criminal justice authorities (U.S. Department of Justice, 2016). The majority of House of Ruth Maryland (HRM) program participants live in neighborhoods characterized by high rates of community violence, intense poverty, few job opportunities, and inadequate educational resources, factors associated with increased risk for IPV (Benson et al., 2003; Holliday et al., 2019; Reed et al., 2009). Many program participants have developed a persona to survive in these contexts, presenting themselves as tough, in control, trusting of no one, resenting of authority, and capable of extreme violence. Using feedback from program participants, intervention facilitators, and local community partners, and consultation from national experts on IPV in the Black community, the HRM program was developed and refined over a number of years to provide RVIP services that are sensitive and responsive to participants’ lived experiences and community context.

The HRM program integrates concepts, structure, and strategies from a number of established approaches, including, but not limited to, Emerge (Adams & Cayouette, 2002) and the Duluth Model (Pence & Paymar, 1993). However, core program concepts from these approaches were adapted to address the perspectives and needs of participants living in high-stress urban communities (Williams, 1994). Most importantly, the core focus on personal accountability for expressions of power and control in intimate relationships is infused with an emphasis on participants’ own experiences of systemic racism, marginalization, and oppression. Session activities help participants to identify how their identity-based and community-based experiences of oppression impact their own abuse of power and control toward relationship partners, using this insight to develop empathy for abuse victims. The desire to be treated with respect is used to understand and challenge problematic interactions

with partners, and a focus on responsible parenting is used to help participants break the cycles of family and community violence.

Our literature search revealed only one prior quantitative investigation of a culturally-focused RVIP for racial minority participants living in high-stress urban contexts. This study, conducted in Pittsburgh, PA, randomly assigned Black men to one of three experimental conditions: 1) race-specific groups that used a culturally-focused intervention model; 2) race-specific groups that used the standard agency model; and 3) mixed-race groups that used the standard agency model (Gondolf, 2007). The investigation revealed no significant condition differences in victim partner reports of recidivism during a 12-month follow-up, and a lower rate of partner abuse criminal charges for men in the conventional, mixed-race groups.

Although their results did not reveal any predicted benefits of culturally-focused programming, several design features highlight the need for further research on RVIP approaches adapted for individuals living in high-stress urban communities. First, despite evidence of protocol adherence, Gondolf (2007) expressed concerns about facilitator “buy-in” and competence in the culturally-focused intervention condition. The HRM program, in contrast, was designed to promote competent and enthusiastic service delivery by engaging providers in an extensive process of program development. Second, their culturally-focused intervention was delivered within racially homogeneous groups by a single facilitator of the same race and gender in order to promote disclosure, understanding, and group solidarity (Gondolf & Williams 2001). However, single-race groups may have the unintended consequence of alienating some participants who resent being segregated for any reason or may signal a false notion that the problem of partner violence is confined to specific racial or ethnic groups. In addition, some participants may benefit from the diversity of perspectives and experiences represented in mixed-race groups. While serving a predominantly Black population, the HRM program, in contrast, is delivered in a group format that includes participants from diverse racial and ethnic backgrounds by a male / female group facilitator team. Finally, in order to isolate differential treatment effects, the approach investigated in the Gondolf study was designed to be very distinct from the standard intervention program, and therefore may have downplayed traditional interventions, such as assertiveness and communication skills training, that are potentially helpful for RVIP participants from

many backgrounds. In contrast, the HRM program integrates elements of traditional RVIP approaches while adapting these interventions to the perspectives and needs of the population served.

The current investigation used propensity score analyses to examine differences in criminal-legal system recidivism between similar individuals who did, and did not, complete the HRM intervention. Although Randomized Controlled Trials (RCTs) remain the “gold standard” for evaluating intervention efficacy, alternative study designs remain important given the practical and ethical challenges in implementing RCTs to evaluate real-world RVIP practice. Propensity score methods provide a widely-used alternative to randomized designs that can be applied to observational studies without random assignment. The propensity score is the probability of membership in the treatment or comparison group conditional on the observed baseline covariates (Rosenbaum & Rubin, 1983). In the current study, the propensity score was calculated as the predicted probability of RVIP treatment completion using 28 factors measured at program intake. The propensity score was then used to estimate the effect of treatment on criminal recidivism using two commonly recommended approaches: 1) matching cases with very similar probability of treatment completion and 2) adjusting observed differences between treatment completers and non-completers through covariance analysis (Austin, 2009). This application of propensity score methods capitalizes on the fact that many factors that predict RVIP completion also predict post-RVIP recidivism (Jewell & Wormith, 2010). As a result, simple comparisons of program completers and dropouts are likely to provide highly inflated estimates of RVIP effects relative to estimates derived using propensity scores.

The current study addresses a significant gap in the literature by examining the impact of a community-informed and oppression-sensitive RVIP delivered to a predominantly low-income, racial minority population residing in high-stress urban neighborhoods. The goal was to evaluate the effectiveness of this intervention using propensity score modeling to test the following hypotheses in a sample of men who attended the program intake:

- 1) In a restricted sample of cases matched on the predicted probability of program completion, individuals who completed the HRM program will have lower rates of criminal recidivism during the year after program intake than those who did not complete the program.

2) In the full sample of intake cases, treatment completers will have lower rates of criminal recidivism than non-completers after controlling for the propensity score.

Methods

Participants

The data for the current project came from a multi-site investigation of RVIPs in Maryland, approved through IRB review at the University of Maryland, Baltimore County. One prior publication examined pre-treatment predictors of program attendance and completion in the larger (parent study) data set (Richards et al., 2019). The initial sampling frame for the current investigation consisted of all the intake cases seen at an urban community RVIP during calendar years 2014 and 2015. Agency file data were extracted for a total of 916 consecutive intake cases; 65 individuals were excluded from further analyses because no criminal history data could be located, and 107 women were excluded because they received gender-specific group treatment that varied in structure and content from the men's program under investigation. The remaining sample of men ($N = 744$) ranged in age from 19 to 71 ($M = 34.21$, $SD = 10.21$); 89.4% self-identified as Black; 9.1% as White, 0.4% as Latino; 0.3% as Native American; 0.1% as Asian, and 0.7% as Other or multiracial; 35.0% had less than high school education, 54.6% had high school or equivalency; and 10.4% had more than high school education; 59.3% reported that they were unemployed at the time of program intake; 76.1% were on probation; 43.1% were under a protection order to stay away from the identified victim of their abuse; 35.5% reported that they were still in a relationship with the identified victim, and 20.4% were living together with that partner.

Measures

Outcome variables. Recidivism offenses during the 12 months after program intake were coded from Maryland Judiciary Case Search, a publicly-available database containing information on legal cases in the state. Each criminal case (i.e., each arrest incident or event) was coded into one of 6 mutually-exclusive categories based on the specific criminal statute associated with an offense (Bouffard & Zedaker, 2016): (1) Partner abuse-related legal involvements, which included issuance of a new Personal Protective Order

[PPO], a new Peace Order [PO], a stalking charge, or violation of a PPO or PO.; (2) Other violent offenses (e.g., assault, battery); (3) Property offenses (e.g., burglary, fraud); (4) Drug offenses (e.g., possession); (5) Driving while intoxicated/under the influence of offenses; and (6) all other offenses (e.g., disorderly conduct, public urination). Coding was hierarchical; each offense incident was coded into the applicable category with the lowest number, starting with partner abuse-related incidents.

These codes were then used to construct outcome variables for the number of recidivist incidents during the 12 months after program intake in three categories: partner abuse-related legal involvements (PA), other violent offenses (VO), and total criminal offenses (TCO). Because the victim's identity or relationship to the offender are not consistently present in the Case Search database, the PA category was restricted to legal involvements that are almost always linked to intimate partner abuse. Specifically, PPOs are issued only for domestic relationships (cohabiting, married, and/or co-parenting), and Peace Orders were designed primarily to provide relief for abuse in other dating or intimate relationships. Any new order or violation was considered a negative outcome, regardless of who requested it (e.g., abuse toward a new relationship partner or multiple relationship partners). Due to the limitations in the available data, it is probable that many partner abuse cases were coded into the VO category (e.g., assault charges that were not accompanied by a PPO or PPO violation).

Propensity score covariates. Twenty-eight variables assessed at the time of program intake were used to estimate the probability of treatment completion for propensity score analyses: age, level of education (less than high school, high school, more than high school), race (White / non-White), employment status (yes/no), living together with the victim partner (yes/no), in a relationship with the victim partner (yes/no), married to the victim partner (yes/no), have children together with the victim partner (yes/no), on probation (yes/no), length of probation sentence (in months, coded as zero for those not on probation), current protective order in place (yes/no), currently abusing substances (yes/no), currently receiving substance use treatment (yes/no), past history of substance use (yes/no), any history of substance use treatment (yes/no), substance use at the time of the referring incident (yes/no), self-report of current or past mental health problems (yes/no), past or current mental health treatment (yes/no), currently taking medication for a

mental health problem (yes/no), current homicidal intention (yes/no), history of suicide attempt(s) (yes/no), history of suicidal ideation (yes/no), current access to weapons (yes/no), partner violence in the family of origin (yes/no), physically abused as a child (yes/no), age at first adult criminal offense (in years), and criminal history, coded as the number of criminal offenses in Maryland records before program intake in the three categories used to assess outcome: PA, VO, and TCO.

Procedures

Data Coding. Data extraction from agency case files and coding of criminal justice data were performed by doctoral level researchers and advanced graduate students.

Treatment Variable. The sample was divided into two groups, those who completed the HRM RVIP ($n = 284$), and those who did not ($n = 460$). Among the treatment non-completers, 278 (37.4% of the total sample) dropped out after completing intake only, and 182 (24.5% of the total sample) dropped out after completing at least one treatment session.

Intervention Approach. The intervention program “The Gateway Project: A Path to Nonviolence,” was developed over many years with consultation from participants, facilitators, local community partners, and national experts. The program is part of the House of Ruth Maryland, a multi-service agency that provides shelter, counseling, outreach, and legal advocacy for IPV survivors and has provided RVIP services in the city of Baltimore since 1979. The HRM program requires participants to attend 28 weekly sessions delivered in same-gender, open-enrollment groups by two co-facilitators. The program uses a two-stage model. Stage 1 group sessions focus on acknowledging the past use of relationship violence and initiating personal accountability. Stage 1 is ideally completed in 4 weeks, but extended for clients who need additional time to meet expected goals. Extended time in Stage 1 does not alter the overall requirement of 28 total sessions for program completion. Stage 2 group sessions build on the work that participants have done in Stage 1 and cover 5 core areas: Healthy Relationships, Communication, Sexual Respect, Masculinity, and Parenting. Stage 2 sessions are designed to engage participants with varied levels of education and literacy, and include group activities and highly interactive discussions. The specific session content areas are detailed in Table 1.

One fundamental program goal is to help participants think about how they impose on their relationship partners similar rules, restrictions, abuses, and deprivations that they have experienced themselves. Facilitators help participants to develop empathy for their partners and children by exploring their own experiences surviving near the bottom of the social power structure within a system of cultural and racial oppression. Participants’ own desire to be treated with dignity and respect is reflected in a core mantra of the program, “Is it respectful?” This question, which resonates deeply with most group members, is raised consistently during discussions of how participants handle relationship conflicts, initiate and end relationships, and discipline children. A related program theme is sexual respect, with the goal of altering coercive sexual behavior, the use of deception and manipulation to obtain sex, and the belief in sexual entitlement with relationship partners. Another program goal is to help end generational cycles of abuse through a focus on parenting. Approximately 85% of program participants have children, and concern for their children’s well-being is an effective hook that encourages program engagement. Group discussions focus on understanding the impact of partner abuse and conflict on children, constructive discipline strategies, and respectful co-parenting. Participants are asked to examine their own exposures to adverse childhood experiences, as well as their children’s exposures, as a way to help motivate change to improve their children’s lives. A final program theme is engagement with the community, including efforts to extend the program reach and impact through partnerships with local organizations that focus on parenting, educational and employment support, and services for mental health and substance use problems. The overarching focus remains on the community context in which participants live, and how their own personal experiences of oppression have influenced their expressions of power and control toward relationship partners.

During the period of the current evaluation, a total of 7 facilitators provided group services (2 men, 5 women; 6 Black, 1 White). All were full-time employees with a bachelor’s or master’s degree in a human services or mental health field. Facilitators completed a 60-hour training program on relationship violence intervention, received regular one-on-one supervision, and participated in a monthly peer-consultation group that included review of videotaped recordings of group sessions. With a few exceptions during periods when a facilitator position was vacant, all group sessions were co-facilitated by a male/female team.

Table 1: *Program Session Content***Stage One**

Session 1: The Pyramid of Oppression
Session 2: Labels for Women / Stereotypes
Session 3: Costs and Benefits of Using Power and Control
Session 4: Cycle of Violence / Self- Talk

Stage Two**Section I: Healthy Relationships**

Session 1: What is a relationship?
Session 2: Respect vs Disrespect (part one)
Session 3: Respect vs Disrespect (part two)
Session 4: Breaking Trust: Male Privilege
Session 5: Breaking Trust: Emotional Abuse
Session 6: Breaking Trust: Lying (Cheating/Substance Abuse)
Session 7: Rebuilding Trust: Being Trustworthy
Session 8: Rebuilding Trust: Honesty and Accountability
Session 9: Rebuilding Trust: Consistency
Session 10: Intimacy
Session 11: Ending a Relationship

Section II: Communication

Session 1: Feelings
Session 2: Styles of Communication
Session 3: Practicing Communication
Session 4: Negotiation and Compromise

Section III: Sexual Respect/Sexual Abuse

Session 1: Sexual Respect (part one)
Session 2: Sexual Respect (part two)

Section IV: Masculinity

Session 1: The Mask
Session 2: Messages About Being a Man

Section V: Parenting

Session 1: Ages & Stages
Session 2: Roots of Truth
Session 3: Styles of Parenting
Session 4: Discipline vs Punishment
Session 5: Collaborative Parenting

Data Analysis Plan

Missing Data. Some case file data on propensity score covariates were missing, especially for variables coded from open-ended interview questions or narrative event descriptions. Blank or incomplete responses yielded ambiguous information as to whether a question was not asked, not answered, or not noted in the intake file. Specifically, 7 variables had 0-5% missing data, 3 variables had 5-10% missing data; 4 variables had 10-20% missing, and 14 variables had more than 20% missing (maximum was 29.6%). These missing data were addressed through multiple imputation using SPSS version 26.

Propensity Score Computation. The logistic model predicting program completion from the 28 baseline covariates was run on 25 imputed data sets, and the median predicted probability of group membership was used as the propensity score in subsequent analyses. This median propensity score significantly predicted treatment completion ($\chi^2 = 195.30$, *Wald Statistic* = 145.81, $p < .001$), accounted for approximately thirty percent of the variance in completion status (*Nagelkerke's* $R^2 = .31$), and correctly classified 72.6% of participants.

Analyses of Outcome. For all analyses, the outcome variables were the number of recidivist incidents during the 12 months after program intake in each of three categories – partner abuse-related legal involvements (PA), other violent offenses (VO), and total criminal offenses (TCO). Analyses of treatment outcome were conducted in three ways. First, one-to-one propensity score case matching was completed with the Matchit package in R (Ho et al., 2011) using nearest neighbor matching with a caliper width equal to .2 of the pooled standard deviation of the logit of the propensity score (Austin, 2011). Using that method, 216 treatment completers (76.1%) were successfully matched to a non-completer case, and analyses of treatment outcome were conducted using paired-sample t-tests (Austin, 2009). Case matching is considered to provide the most accurate estimate of treatment effects (Austin, 2011), but has the limitation of reducing the sample size and deleting cases with very high or low likelihood of treatment completion. Therefore, subsequent analyses tested differences between program completers and non-completers using the entire sample, contrasting results obtained with, and without, statistical control of the propensity score. A final set of analyses examined treatment session attendance as a continuous variable in order to account for treatment exposure among non-completers and the fact that non-

completers who attended some treatment sessions were more likely than other non-completers to be included in the matched sample analyses.

Sensitivity Analyses. With alpha set to .05 in a two-tailed test of mean differences, the available sample for the matching analyses ($n = 256$ pairs) provides sufficient power (.80) to detect a small effect size ($d = .18$). In addition, the full sample ($n = 744$) provides sufficient power (.80) to detect a small effect ($f = .10$) in the covariance analyses (Faul et al., 2007).

Results

Pre-Treatment Differences between Treatment Completers and Non-Completers

Table 2 displays data on the covariates used in the propensity score matching, comparing treatment completers and non-completers. The left columns present comparisons for the full sample, and the right columns present comparison for the propensity-score matched sample. For the full sample, 10 of the 28 balancing factors differed significantly between completers and non-completers at $p < .05$. In contrast to non-completers, treatment completers were older, had fewer prior legal involvements for violent offenses and overall offenses; were older at the time of their first adult arrest; were more likely to be on probation; had longer probation sentences; had more formal education; were more likely to be White and employed; and were less likely to report ever having had a mental health problem.

As displayed in the right columns of Table 2, in the propensity-score matched sample, the 28 covariates were distributed quite evenly across groups, with no significant differences observed between treatment completers and non-completers (all p values $> .05$). The propensity score matching worked efficiently to create matched groups of treatment completers and non-completers who are comparable on the background variables measured at program intake, including case demographics, relationship status, mental health indicators, and criminal history.

Criminal Justice Outcomes for Treatment Completers and Non-Completers in the Propensity-Score Matched Sample

Hypothesis 1 was tested using paired-sample statistics (Austin, 2009). In the propensity-score matched sample, the total frequency of re-arrest for any offense (TCO) was significantly lower for treatment completers ($M = 0.98$, $SD = 1.39$) than for non-completers ($M = 1.38$, $SD = 1.93$), $t(215) = 2.39$, $p = .018$, $d = 0.16$. The frequency of re-arrest for violent offenses (VO)

Table 2: Covariates at Program Intake for RVIP Treatment Completers and Non-Completers in the Full Sample and Propensity-Score Matched Sample

Variable	Full Sample			Propensity-Score Matched Sample			
	(n = 284) ^a	Completers (n = 460) ^b	Non-Completers (n = 216)	(n = 216)	Completers (n = 216)	Non-Completers (n = 216)	p (diff) ^c
		M (SD)	M (SD)	p (diff) ^c	M (SD)	M (SD)	p (diff) ^c
Age		35.27 (10.14)	33.56 (10.20)	.026 *	34.25 (9.64)	34.80 (10.87)	.578 (ns)
Age at First Adult Offense		23.73 (8.10)	21.66 (6.51)	.001 ***	23.00 (7.32)	23.18 (7.96)	.806 (ns)
Prior PA Incidents		1.43 (1.69)	1.68 (2.14)	.099 (ns)	1.51 (1.84)	1.59 (1.70)	.645 (ns)
Prior VO Incidents		1.89 (2.11)	2.44 (2.60)	.003 **	1.94 (2.12)	2.04 (2.41)	.641 (ns)
Prior TCO Incidents		6.23 (6.14)	8.54 (7.28)	.001 ***	6.73 (6.60)	7.19 (6.05)	.566 (ns)
Probation Length (Months)		20.77 (23.00)	16.20 (15.50)	.004 **	17.81 (14.71)	17.80 (14.93)	.994 (ns)
		(%)	(%)		(%)	(%)	
Education				.001 ***			.607 (ns)
Less than High School	28.5%	39.6%			32.4%	32.5%	
High School		56.2%	55.5%		54.0%	56.9%	
More than High School	15.3%	6.9%			13.6%	10.5%	
Race (% non-White)		86.9%	93.5%	.003 **	88.8%	90.2%	.648 (ns)
Employment (% employed)		48.2%	35.4%	.001 ***	43.3%	40.1%	.512 (ns)
In relationship with victim partner		35.1%	35.9%	.838 (ns)	36.0%	33.5%	.595 (ns)
Living with victim partner		19.5%	21.1%	.628 (ns)	20.1%	18.7%	.723 (ns)
Married to victim partner		12.9%	12.2%	.771 (ns)	13.3%	12.9%	.900 (ns)
Children with victim partner		53.4%	54.3%	.815 (ns)	52.1%	48.3%	.434 (ns)
Current protective order		43.2%	43.0%	.964 (ns)	44.9%	42.9%	.682 (ns)
On probation		80.3%	73.5%	.035 *	77.3%	78.7%	.727 (ns)

Table 2 Continues

Table 2 (Continued)

Access to weapons	6.1%	4.2%	.301 (ns)	5.2%	2.1%	.090 (ns)
Substance use (ever)	73.3%	74.9%	.670 (ns)	73.2%	73.2%	.999 (ns)
Substance use treatment (ever)	22.1%	25.4%	.363 (ns)	23.0%	22.2%	.850 (ns)
Substance use treatment (current)	7.9%	7.1%	.742 (ns)	6.6%	7.1%	.851 (ns)
Substance use at time of incident	21.0%	19.0%	.586 (ns)	18.0%	20.4%	.535 (ns)
Mental health problems (ever)	18.9%	28.8%	.006 **	21.7%	20.8%	.822 (ns)
Mental health treatment (ever)	21.5%	27.2%	.119 (ns)	23.7%	17.8%	.141 (ns)
Mental health medication (current)	9.5%	11.2%	.524 (ns)	11.1%	6.1%	.071 (ns)
History of homicidal ideation ^d	14.0%	12.8%	.695 (ns)	14.2%	13.8%	.898 (ns)
History of suicidal ideation	10.8%	9.8%	.718 (ns)	11.4%	8.7%	.366 (ns)
History of suicide attempts	2.5%	1.5%	.415 (ns)	3.3%	0.5%	.069 (ns)
Partner abuse in family of origin	22.7%	16.4%	.067 (ns)	19.4%	17.0%	.533 (ns)
Child abuse history	2.9%	2.3%	.658 (ns)	3.9%	2.1%	.294 (ns)

^a n ranges from 243 to 284 for specific analyses.

^b n ranges from 252 to 460 for specific analyses.

^c p value from t-test of group differences for continuous variables and χ^2 test for categorical variables (Fisher's exact test for categorical variables with expected cell frequencies less than 5).

^d History includes homicidal ideation, threats, or attempts.

PA = Partner abuse-related legal involvements; VO = other violent offenses; TCO = total criminal offenses.

* p < .05; ** p < .01; *** p < .001.

was also lower for treatment completers ($M = 0.27$, $SD = 0.61$) than for non-completers ($M = 0.41$, $SD = 0.87$), but this result was not statistically significant $t(215) = 1.79$, $p = .075$, $d = 0.12$. New legal involvements for partner abuse (PA) were not notably different for treatment completers ($M = 0.38$, $SD = 0.78$) and non-completers ($M = .45$, $SD = 0.97$), $t(215) = 0.91$, $p = .365$, $d = 0.06$.

Full-Sample Analyses With and Without Adjustment for Propensity Scores

Hypothesis 2 was tested by examining differences between treatment completers and non-completers in the full sample with the propensity score as a covariate (see Table 3). The propensity score significantly predicted all three recidivism variables in the full sample ($n = 744$): for PA, $r = -.11$, $p = .002$, for VO, $r = -.19$, $p < .001$, and for TCO, $r = -.21$, $p < .001$. In preliminary analyses that did not include the propensity score as a covariate, TCO and VO recidivism were significantly lower for treatment completers than for non-completers, with effect sizes in the small-to-medium range of magnitude. With the propensity score included as a covariate, TCO remained significantly different, with a small effect size. The effects for VO and PA were not significant. These results directly parallel the findings from the case-matching analyses described above.

Analyses of Treatment Attendance as a Continuous Variable

Data on the number of program sessions attended were available in 739 of the 744 case files (99%). Within the non-completer comparison group, 38.4% attended at least one treatment session prior to program dropout ($M = 3.08$ sessions attended, $sd = 5.64$). Not surprisingly, the propensity score was significantly and positively correlated with the total number of treatment sessions attended, both within the full sample ($r = .56$, $p < .001$) and among program non-completers ($r = .41$, $p < .001$). In addition, program non-completers who were retained in the propensity-score matched sample attended significantly more treatment sessions, on average, ($M = 4.95$, $sd = 6.51$)

than those who were not retained in the matched sample ($M = 1.41$, $sd = 4.07$), $t(df = 456) = 7.05$, $p < .001$. Given that treatment exposure in the comparison group may have impacted the estimation of program effects, additional analyses examined the number of sessions attended as a continuous variable.

Table 4 displays correlations between the number of sessions attended and recidivism variables for the full sample, and for the matched subsample. For the full sample, individuals who attended more sessions had lower recidivism across all three indicators. These correlations were all significant, with small effect sizes. When the propensity score was held constant through partial correlation, the full-sample association between session attendance and recidivism remained significant for TCO, but was no longer significant for PA and VO. These full-sample findings parallel the analyses contrasting treatment completers and non-completers, with one exception: the association between program attendance and PA in the uncontrolled analysis was significant when analyzing session attendance as a continuous variable ($p = .011$) but not significant when analyzing program completion as a dichotomous variable ($p = .054$).

For the subsample that was included in the propensity-score matching analyses, individuals who attended more sessions had lower recidivism for VO and TCO, with effect sizes in the small range of magnitude. The association between session attendance and PA was not significant. Adding the propensity score as a covariate had very little impact on the observed correlations, and the predictive associations of session attendance with VO and TCO remained statistically significant. These restricted-sample findings parallel the analyses contrasting treatment completers and non-completers, with one exception: the association between program attendance and VO was significant when analyzing session attendance as a continuous variable ($p = .019$) but not when analyzing program completion as a dichotomous variable ($p = .075$).

Table 3: *Analyses of Differences between Treatment Completers and Non-Completers with and without the Propensity Score as a Covariate in the Full Sample*

Outcome Variable	Completers (<i>n</i> = 284)		Non-Completers (<i>n</i> = 460)		Without Covariate			Covarying Propensity Score		
	% ^a	<i>M</i> (<i>SD</i>)	% ^a	<i>M</i> (<i>SD</i>)	<i>F</i>	<i>p</i>	<i>d</i>	<i>F</i>	<i>p</i>	<i>d</i> ^b
PA	24.6	0.37 (0.77)	30.7	0.50 (0.96)	3.72	.054	.15	0.20	.653	.04
VO	19.4	0.25 (0.58)	30.4	0.49 (0.92)	15.11	<.001	.31	2.15	.143	.13
TCO	45.8	0.90 (1.31)	60.9	1.58 (2.03)	25.34	<.001	.40	4.64	.032	.19

PA = Partner abuse-related legal involvements; VO = other violent offenses; TCO = total criminal offenses; ^aPercentage of the completer and non-completer groups with any recidivist offense; ^bCalculated as the difference in estimated marginal means divided by the square root of the mean square error from the analysis of covariance.

Table 4: *Continuous Variable Correlations between Treatment Session Attendance and Recidivism*

Outcome Variable	Full Sample (<i>n</i> = 739)		Propensity-Score Matched Sample (<i>n</i> = 429)	
	<i>r</i> ^a	Partial <i>r</i> ^b	<i>r</i> ^a	Partial <i>r</i> ^b
PA	-.09*	-.03 (<i>ns</i>)	-.08 (<i>ns</i>)	-.07 (<i>ns</i>)
VO	-.16***	-.06 (<i>ns</i>)	-.13**	-.11*
TCO	-.20***	-.08*	-.16**	-.14**

PA = Partner abuse-related legal involvements; VO = other violent offenses; TCO = total criminal offenses; ^aZero-order correlation with number of sessions attended; ^bPartial correlation with number of sessions attended controlling for the propensity score.

* $p < .05$; ** $p < .01$; *** $p < .001$; *ns* = not significant (at $p < .05$).

Discussion

This study examined the impact of a community-informed and oppression-sensitive intervention program designed to meet the needs of a predominantly low-income and racial minority treatment population of IPV offenders living in high-stress urban communities. Consistent with our first hypothesis, the matching analyses revealed that program completion has a significant effect on total criminal offenses (TCO) during the year after program enrollment. However, those analyses revealed no significant program effect on new legal involvements for partner abuse (PA), and a marginal effect on other violent offenses (VO). The second hypothesis also received partial support. In full sample analyses, when the propensity score was not controlled, treatment completion was associated with significantly lower TCO and VO. With the propensity score included as covariate, the treatment effect on TCO remained significant. A final set of analyses estimated treatment effects using the number of sessions attended as a continuous variable in order to account for treatment exposure in the comparison group of non-completers. When the propensity score was not adjusted, and using the full sample, individuals who attended more treatment had significantly lower rates of PA, VO, and TCO. When the propensity score was included as a covariate, the effect of session attendance was only significant for TCO. In the restricted sample that was matched on the probability of treatment completion, both TCO and VO were significantly lower for those who attended more treatment sessions.

It is helpful to interpret these results in light of prior RVIP outcome research, including studies using similar evaluation methods. Consistent with prior meta-analyses of RVIP outcome research (Babcock et al., 2004; Cheng et al., 2019), the treatment effects in the current study were in the small range of magnitude. On one hand, these small program effects are encouraging for this study population. On average, they had more than 7 prior criminal justice-involved incidents before RVIP enrollment, and over half had one or more recidivist involvements with the criminal justice system during the year after program enrollment. On the other hand, small effects encourage further innovations that may increase program impact, including supportive services to address risk factors associated with structural oppression, such as unemployment and traumatic stress (Murphy & Richards, in press, Radatz & Wright, 2016). Notably, the estimated effect of intervention was much smaller in analyses

that controlled for the propensity to complete treatment than in analyses that did not. This pattern is consistent with meta-analytic results that have found much smaller RVIP effects in controlled trials than in studies that used unadjusted comparisons of program completers and dropouts (Babcock et al., 2004).

In addition, across all three data analytic approaches, the strongest and most consistent program effect was observed for overall criminal reoffending. This result is consistent with a recent study that used propensity score matching to evaluate a specialized RVIP for high risk offenders in Connecticut (Cox & Rivolta, 2019). They found a significant program effect on re-arrest for any crime during a 12 month follow-up, but no significant effect on partner abuse crimes. However, a recent meta-analysis reported somewhat a larger average RVIP effect on partner abuse crimes than on crime overall, even though both were significant (Cheng et al., 2019). These findings must be interpreted in light of important limitation in the outcome data available for the current study. Specifically, PA recidivism was coded only for judicial involvements that could be unambiguously attributed as partner-related, primarily indicated by the issuance or violation of a protection order (which could occur together with, or separate from, criminal charges such as assault). As a result, the VO category likely included a substantial number of partner violence incidents involving relevant criminal charges that could not be attributed as partner-related with the available information. In addition, some PA incidents may not reflect recidivist violence, for example protection order violations from non-violent contact.

It is also interesting to consider the current findings in light of Gondolf's (2007) randomized trial, which found no significant differences in partner reports of violence recidivism for Black men who attended culturally-focused groups versus standard RVIP groups in either mixed race or single race formats. Notably, the HRM community-informed approach differs in some ways from culturally-focused interventions that focus on the beliefs and practices of specific racial or ethnic groups. Such approaches may connote cultural "pathology" that needs to be changed. For example, group discussions focused on the lyrics of rap music or the concept of "machismo" may inadvertently stigmatize participants' cultural values or tastes. In addition, membership in a specific racial or ethnic group does not guarantee identification with specific cultural beliefs or practices, and therefore some participants may reject or de-identify with

such interventions. In contrast, the HRM program emphasizes the ways in which participants' life context and experiences of systemic oppression can negatively impact their intimate relationships. This expanded focus on power and its abuses provides an inclusive framework for engaging group members who vary in their identity-based and community-based experiences of privilege and oppression. However, the current study design can only indicate whether program participation was associated with lower criminal recidivism, and cannot indicate whether this approach is more effective than other RVIP approaches.

Limitations

Our analytic approach has some important limitations. On one hand, unmeasured predictors of program completion and measurement error may have led to under-adjustment in the propensity score analyses, and over-estimation of treatment effects. On the other hand, the inclusion of participants with varied levels of program attendance in the comparison group may have under-estimated program effects. This latter concern is supported by the results for treatment attendance as a continuous variable, which showed additional significant program effects on violent offenses within the matched sample that were not found in the primary analyses. More research is needed to determine whether, and under what conditions, propensity score analyses can provide results that approximate those obtained through randomized studies of RVIP effects.

The timing and duration of the follow-up period, twelve months from the time of program intake, invoke additional concerns. The program is designed to be completed in approximately 6 months, and therefore recidivism was assessed both during and after scheduled program completion. Similar RVIP evaluations have found high rates of re-assault during the first 3-6 months after program enrollment (Gondolf, 2000). The current study may have under-estimated the effect of the intervention if treatment exposure has a cumulative impact over time, or if behavior change reliably occurs only after extended exposure to the intervention. Some prior evidence, for example, indicates that a minimum exposure of 3 months may be necessary to observe RVIP effects (Gondolf, 2000). In addition, the one-year follow-up period is insufficient to evaluate the long-term impact of the intervention.

Another limitation was the amount of missing data on baseline covariates, particularly those coded from open-ended intake questions and narrative information in case files. The use of multiple imputation

allowed for the retention of the entire sample in developing logistic models to predict treatment completion, and the prediction accuracy was quite similar to a prior propensity score RVIP investigation conducted with experimenter-collected data (Jones et al., 2004). However, it remains possible that missing data on covariates may have influenced the current results in unknown ways.

Another important limitation is that this study was not designed to isolate the effects of the community-informed program adaptations versus traditional RVIP elements. A comparative treatment trial would be needed to evaluate the relative contribution of these program features. Along similar lines, the current study evaluated a program as implemented under real-world conditions in a community agency, and did not assess treatment fidelity or protocol adherence. Although expected in state-of-the-art clinical trials, fidelity assessment is actually quite rare in research on RVIP efficacy (Murphy & Richards, in press). In the current study, the fact that program staff were deeply involved in the development of the intervention likely contributed to enthusiastic and competent service delivery. However, no objective data are available to verify that assumption.

Research Implications

The use of criminal justice data to measure IPV program outcomes is controversial given that direct reports from victim partners tend to yield higher recidivism estimates. However, reliance on partner reports may also introduce systematic biases, including failure to detect abuse perpetrated in new relationships established during or after RVIP participation. This point is noteworthy given that two-thirds of the current sample were no longer in a relationship with the identified victim at program intake. In addition, RVIP attendance may increase the probability that participants will remain in the relationship or reunite after separation, and may enhance the individual's prospects for child custody or visitation. Such effects may increase the likelihood that those who attend RVIP would continue to interact with the same relationship partner over time, whereas those who do not attend RVIP may be more likely to interact with new partners. Finally, it can be difficult to obtain contact information and to successfully reach and interview victim partners. In almost all studies to date, partner report data is missing on a sizeable proportion of cases, up to 70% in some trials (e.g., Feder & Dugan, 2002). These distinctions are highlighted by a recent meta-analysis which found significant benefits of RVIP attendance when criminal justice data were used to

measure outcomes, but not when victim partner reports were used (Cheng et al. 2019). Currently, there is no way to discern whether this disparity reflects biases in partner report data (e.g., failure to detect abuse in new relationships; problems with recruitment of partners; high loss to follow-up) or biases in the use of criminal justice data (e.g., program attendance reducing arrest without altering victim exposure to abusive behavior).

The use of propensity score matching, although rare in RVIP evaluation research, offers a promising alternative to RCT designs in real-world practice settings. Although RCTs remain the gold standard, many stakeholders resist the idea of randomizing offenders to a no-treatment or minimal intervention control. In addition, randomization in the context of criminal prosecution may limit individuals' capacity to provide fully voluntary research consent. Further, those assigned to RVIP (versus a minimal or no-treatment control) may experience differential demands (e.g., session attendance and fee payment) that increase risk of non-compliance, creating unequal legal jeopardy as a function of research condition assignment.

Randomization to two or more active intervention conditions is a helpful alternative design, but one that asks questions about relative, rather than absolute, program effects. Without a minimal treatment control, null findings from comparative trials cannot indicate whether both treatments were effective, or whether neither treatment was effective. Similarly, significant findings cannot indicate whether one treatment provided more benefit than the other, or caused less harm. Determination of program efficacy requires a minimal or no-treatment condition in order to address the critical social policy issue of whether such programs should be offered at all. Propensity-score matching with dropout or untreated cases can provide a helpful alternative approach to address this need when randomization to minimal treatment is not possible.

Clinical and Policy Implications

The community-informed approach investigated here provides a framework for helping IPV offenders who reside in high-stress urban contexts and experience many life challenges that are correlated with IPV use, including discrimination, social marginalization, unemployment, and exposure to community violence and other traumatic stressors (Benson et al, 2003; Holliday et al., 2019; Reed et al., 2009). The careful and thoughtful adaptation of traditional psychoeducational approaches took many years with ex-

tensive engagement from community partners and national experts. These innovations were designed to facilitate connection and rapport with this treatment population. One persistent example involves group discussions of power and control, which are typically delivered within a unidimensional, gender-based framework (e.g., Pence & Paymar, 1993). By relying on a more intersectional analysis, HRM program staff are encouraged to understand power and oppression within the participant's life context. Facilitators help clients to identify and articulate ways in which they have experienced oppression, and use that insight to understand the ways in which clients have abused power in their own relationships. From this perspective, accountability work begins with empathy and understanding, consistent with motivational approaches that have been very helpful in increasing participant engagement and reducing IPV in other contexts (e.g., Lila et al., 2018).

In line with recent meta-analytic findings (Cheng et al., 2019), the current study highlights the potential value of IPV intervention in reducing participant engagement with the criminal justice system. In addition, the prospect that RVIP attendance may reduce general criminal offending, and violent offending more specifically, may reflect program impact on decision making and impulsive behavior that goes beyond intimate relationship functioning. Although the effect sizes obtained in the current study were small in magnitude, any reduction in criminal offending can reduce participants' risk for a variety of negative life consequences, including probation violations, fees, fines, employment challenges, and incarceration. The negative social, personal, and family costs associated with persistent legal involvements warrant further consideration as RVIP program outcomes. Other scholars have argued that RVIPs are well positioned to address general criminogenic needs of this population, and thereby reduce offending (e.g., Radatz & Wright, 2016). The current results indicate that community-informed and oppression-sensitive adaptations of RVIP may help facilitate these goals. From a policy perspective, RVIPs should gain increasing recognition as a potential crime-reduction strategy. This shift in focus may create access to funding streams designated to reduce incarceration, and could alleviate competition for funding allocated to support victims (Murphy & Richards in press).

Finally, it is relevant to note that the current study is the result of a collaborative partnership between practitioners and researchers from diverse disciplines including criminology, public health, clinical

psychology, counseling, and social work. We believe that advances in IPV offender rehabilitation will benefit from the broadened perspectives afforded by such collaborations, which are particularly helpful in efforts to meet the needs of diverse, underserved, and understudied populations.

Conclusions

The results indicate that completion of the HRM program, a community-informed and culturally-sensitive intervention for IPV offenders residing in high-stress urban contexts, has a significant impact on

the rate of overall criminal re-offending during the year after program enrollment. These results provide encouraging support for further practice innovations and research that builds on existing IPV interventions by broadening the focus beyond a unidimensional analysis of gender-based expressions of power and control to consider participants' lived experiences of oppression, discrimination, and marginalization, and the many life stressors that impact their relationship quality and violence risk.

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