

Effects of a Hospital-Based Violence Intervention Program on Community Violence in Boston, Massachusetts

A Target Trial Emulation

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Background: Hospital-based violence intervention programs (HVIPs) are widespread, but their effectiveness for violence prevention is unclear.

Objective: To determine the effects of Boston Medical Center's HVIP on future violence outcomes among violently injured young adults.

Design: Target trial emulation using observational data.

Setting: Boston, Massachusetts.

Participants: Young adults aged 16 to 34 years who survived a shooting or stabbing.

Intervention: Target trials of 2 treatment strategies using the same eligibility criteria, time zero, and control group were emulated: 1) any treatment: engaging with the HVIP within 1 month of injury and 2) sustained treatment: initiating within 1 month and engaging more than 4 of the first 8 weeks.

Measurements: Combined measure of violent re-injury or violence perpetration at 1, 2, and 3 years, using hospital and police data.

Results: 1328 patients met criteria; 565 (42.5%) initiated within 1 month. Of these, 58 (10.2%) sustained engagement. In the any-treatment analysis,

estimated cumulative incidence was roughly equal between the treatment and control strategies at 1, 2, and 3 years. In the sustained engagement analysis, treatment was associated with considerably lower cumulative incidence (4.5% [95% CI, 1.1% to 9.3%] at 1 year; 5.1% [CI, 1.1% to 9.3%] at 2 years; 6.4% [CI, 1.4% to 12.9%] at 3 years) versus the control strategy (8.7% [6.6% to 10.0%] at 1 year; 12.3% [10.2% to 14.5%] at 2 years; 14.3% [11.8% to 16.6%] at 3 years), with corresponding risk reductions of 47.6% (−19.8% to 86.7%), 58.5% (21.6% to 91.2%), and 55.3% (4.9% to 90.2%). Confidence intervals were wide.

Limitation: Despite our target trial emulation approach, results could be confounded by unmeasured factors associated with program engagement.

Conclusion: Although HVIPs can improve long-term violence outcomes, these effects seem to require intensive participant engagement.

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Community violence is a critical public health issue. Homicide is a leading cause of death among youth in U.S. cities (1). However, most firearm violence results in nonfatal injuries (2), requiring health care systems and social welfare agencies to meet the needs of violence survivors. Violence survivors often face mental and physical health challenges, such as posttraumatic stress disorder (PTSD) (3) or physical disability (4) and unmet social needs (for example, unemployment or housing insecurity) (5). Importantly, community violence survivors' risk for violent reinjury (6) or violence perpetration (7) approximately doubles for several years after injury.

Hospital-based violence intervention programs (HVIPs) are designed to address those needs. When a person is hospitalized with a community violence injury, an HVIP staff member reaches out to establish a relationship and offer services. Typically, HVIPs offer case management, referrals to social services, and help navigating health care and social needs, leveraging the

"teachable moment" after injury when a person may be most likely to engage with service providers; HVIPs deliver this message through a *credible messenger*, that is, a person whose lived experience helps them gain trust (8).

Having become a cornerstone of the social infrastructure that responds to community violence, more than 60 HVIPs are currently operating in U.S. hospitals (9). Many have been established or grown since 2020, through initiatives to expand the community violence intervention (CVI) ecosystem, that is, the network of programs for people at highest risk for community violence (10). These initiatives have sharpened attention

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on HVIPs as a community violence prevention strategy, even though HVIPs address a range of social and physical outcomes (11). To date, however, evidence of HVIPs' effectiveness for violence prevention has been modest (12, 13). Though studies have reported low rates of violent reinjury or violence perpetration among HVIP participants, such studies were limited by small sample sizes, short follow-up periods, and/or inadequate comparison groups (14–16).

Evaluating HVIPs for violence prevention using randomized controlled trials (RCTs) is difficult because HVIPs are considered the standard of care in many settings, such that randomizing to a no-HVIP condition is unethical. Another problem is statistical power: violence-related outcomes are relatively rare, and the uptake of HVIP services among eligible patients is often modest, which further increases the sample size needed to estimate the effects that would be observed if all participants fully adhered to their assigned protocol (that is, per-protocol effects) (17). Per-protocol effects may be the most important information for eligible persons considering whether to engage with HVIPs, and study results could potentially inform real-world adherence (17), though barriers to engagement remain key issues for study for HVIPs (18) and other violence interventions (19).

For these reasons, we used observational data from the HVIP at Boston Medical Center (BMC), one of the longest-running HVIPs in the United States, to emulate target trials on the HVIP's effectiveness for violence prevention. The goal of the study was to answer the causal question of whether HVIP engagement reduced future violent reinjury and violence perpetration among young adults hospitalized for violence-related injuries. We estimated the observational analogues of per-protocol effects for 2 treatment strategies selected in consultation with BMC HVIP staff: a) engaging with the HVIP within 1 month of the initial violence-related injury ("index injury") and b) sustained engagement with the HVIP over the first 2 months post-index injury (engaging >4 of the first 8 weeks).

METHODS

Data Sources

We used data from persons presenting to the BMC emergency department (ED) with violent penetrating injuries (that is, knife or firearm assault injury) between 2013 and 2022. All survivors of violent penetrating injuries in the BMC ED are eligible for the Violence Intervention Advocacy Program (VIAP), the HVIP we studied (18, 20). Data were acquired from the VIAP clinical database. This database links information originating from VIAP advocate reports, BMC electronic health records, trauma registries, the state's death registry, and police reports from the Boston Regional Intelligence Center (BRIC). Treatment measures were generated from VIAP workers' service notes

in the VIAP clinical database. Service notes were used to measure encounters, needs identified, and goals achieved.

Patient-level covariates were measured only at baseline. These included race, ethnicity, gender, and date of birth, which were self-reported and obtained from electronic health records. Race (Black vs. other race), ethnicity (Latinx vs. non-Latinx), and gender (cisgender male vs. other gender identity) were binarized. Housing status and home address were self-reported to VIAP or hospital staff. Patients' comorbidities (that is, mental health disorder, substance use disorder) were obtained from patients' electronic health records. Persons were considered to have a comorbidity if the date the condition was noted was before or at the time of index injury and did not have a resolution date before injury.

Our models also included measures of injury severity, which influences VIAP engagement (18), and neighborhood-level racialized economic segregation, which influences future violence perpetration rates among BMC violent injury survivors (21). Racialized economic segregation is the spatial social polarization that divides communities into predominantly White, affluent neighborhoods and predominantly Black, impoverished neighborhoods. Our measure was the race-poverty index of concentration at the extremes (ICE), a strong predictor of community violence (22, 23).

Target Trial Specification and Emulation

We specified 2 target trials, sharing the same eligibility criteria, time zero, and control group, to estimate the effects of differing HVIP engagement strategies on violence outcomes. **Table 1** summarizes the key protocol components in the target trials and our target trial emulations.

Summary of Intervention

In the VIAP program, survivors of violent penetrating injuries are met in the ED for crisis intervention by a supervisor, then transferred to an advocate. During an inpatient stay, advocates develop relationships with survivors and their families, conduct needs assessments, and create plans to address needs. If a survivor is discharged from the ED before connecting, advocates attempt to contact the person by phone or at follow-up appointments. The VIAP program advocates support clients in addressing tangible needs, while promoting trauma recovery and behavioral change (24). In addition to basic needs (for example, emergency food or clothes), advocates assist clients with injury recovery, medical and mental health, and family needs. The VIAP program also works on housing, education, employment, and legal needs, that is, social determinants of health that could hinder recovery and potentially lead to future violence. The program also includes home visiting nurse services; VIAP

Table 1. Specification and Emulation of Target Trials of HVIP Using Observational Data From the BMC HVIP

| Protocol Component and RCT Specification | Emulation Using Violence Survivor Cohort |
|---|---|
| Study population | |
| Survived a violent penetrating injury between 2013 and 2022 | Same as for RCT specification |
| Age 16-34 y | Required data: Age, housing status, substance use disorder history, residential location, history of violence-related injury |
| Not experiencing homelessness | – |
| No acute substance use disorder diagnosis | – |
| Reside within 5 mi of Boston | – |
| Not treated at BMC for a violent penetrating injury before 2013 | – |
| Treatment conditions | |
| Protocol A (any engagement) | Same as for RCT specification |
| Engage with HVIP services within 1 mo of index injury | Required data: Date of index injury, dates of any HVIP service engagements |
| Do not engage with HVIP services within 1 mo | – |
| Protocol B (sustained engagement) | – |
| Engage with HVIP services within 1 mo of index injury <i>and</i> engage frequently during early postinjury period (>4 wk out of first 8 wk) | – |
| Do not engage with HVIP services within 1 mo | – |
| Treatment assignment | |
| Eligible persons will be randomly assigned to be offered HVIP services, or not to be offered HVIP services | Eligible persons will be assigned to the strategies with which their data were compatible at the time of eligibility; clones will be included and censored at the time when their data stop being consistent with the assigned strategy |
| Outcomes | |
| Combined measure of violent reinjury and/or violence perpetration | Same as for specification |
| Assault victimization (blunt or penetrating) | Required data: Violent injury history from medical records, police data on alleged violence involvement |
| Assault perpetration (blunt or penetrating) | – |
| Follow-up period | |
| From treatment assignment until violence event, death from another cause, administrative end of follow-up, or 3 y, whichever comes first | Same as for specification |
| Causal estimand | |
| Intention-to-treat effect (effect of being assigned to treatment) | Observational analogues of the per-protocol effects |
| Per-protocol effects (effect of receiving treatment as indicated in the protocol) for: a) any treatment and b) high-dose treatment | – |
| Statistical approach | |
| Intention-to-treat analysis | Same as per-protocol analysis |
| Per-protocol analysis (clone-censor-weight) with adjustment for confounders and inverse probability weighting for censoring | Required data: Preassignment confounders |

BMC = Boston Medical Center; HVIP = hospital-based violence intervention program; RCT = randomized controlled trial.

does not require any predefined timing or duration of program engagement.

Eligibility Criteria

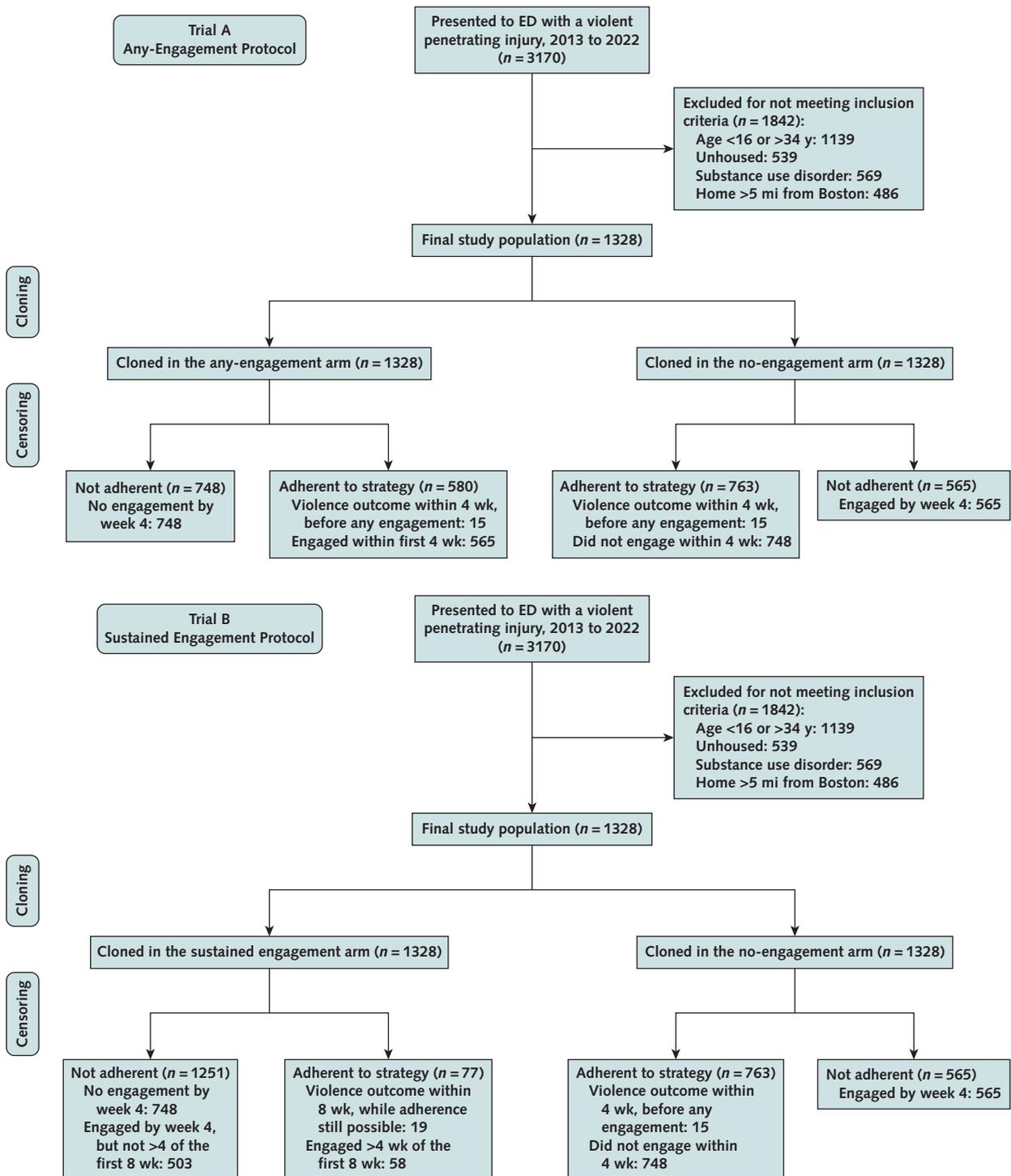
Our eligibility criteria limited the study to young adults (aged 16 to 34 years) and excluded persons experiencing homelessness or previously diagnosed substance use disorder at the time of index injury. Although VIAP offers services to any survivor of a violent penetrating injury, VIAP and other HVIPs are principally designed to meet the needs of young adults, that is, the highest-risk group for future violence involvement (25). The VIAP program is not principally designed to meet the needs of violence survivors experiencing homelessness and/or substance use disorder, though these are important, high-risk subpopulations (26) and VIAP continues to develop models for meeting their needs. We also excluded persons whose

residence was more than 5 miles outside of the city of Boston because their follow-up outcomes would be less likely to be captured by Boston-based data sources. Finally, we omitted persons who had been treated at BMC for another violent penetrating injury before 2013 as not being treatment naive.

Treatment Strategies

In consultation with the VIAP team, we identified 2 protocols to test: a) any engagement and b) sustained engagement. Any engagement means engaging with a VIAP advocate within 4 weeks after index injury. This 1-month window postinjury aligns with the teachable moment that HVIPs aim to leverage for recruitment and behavior change. Sustained engagement means engaging more than 4 of the first 8 weeks postinjury (engagement on the injury date, that is, week 0, counts toward this total). According to VIAP workers,

Figure 1. Flow diagrams.



ED = emergency department; HVIP = hospital-based violence intervention program.

this level of consistent, sustained engagement advances the therapeutic relationship and enables workers to meet more substantial needs. In the target trial, persons would be randomly assigned to treatment strategies and might be aware of their treatment allocation;

in the target trial emulation, assignment was based on real-world receipt of the treatment strategy within pre-specified grace periods (4 weeks for any engagement; 8 weeks for sustained engagement). The sustained engagement trial used the same control group as the

Table 2. Baseline Characteristics of Eligible Persons by Treatment Assignment When Emulating Target Trials of an HVIP and Future Violence Risk: BMC, 2013 to 2023*

| Characteristic Count | Treatment Category | | | |
|--|---|--|---|--|
| | Target Trial A | | Target Trial B | |
| | Any Engagement (Within 4 wk Postinjury) | Comparison Group (Did Not Engage Within 4 wk Postinjury) | Sustained Engagement (>4 wk of First 8 wk Postinjury) | Comparison Group (Did Not Engage Within 4 wk Postinjury) |
| Patients, n (% of total eligible) | 565 (42.5) | 763 (57.5) | 58 (4.4) | 763 (57.5) |
| Median age (IQR), y | 24 (20 to 28) | 24 (20 to 28) | 23 (19 to 28) | 24 (20 to 28) |
| Gender, n (%) | | | | |
| Female | 99 (17.5) | 103 (13.5) | 11 (19.0) | 103 (13.5) |
| Male | 466 (82.5) | 660 (86.5) | 47 (81.0) | 660 (86.5) |
| Race and ethnicity, n (%) | | | | |
| Black, non-Latinx | 381 (67.4) | 488 (64.0) | 34 (58.6) | 488 (64.0) |
| Black, Latinx | 11 (2.0) | 23 (3.0) | 2 (3.5) | 23 (3.0) |
| Other race, Latinx | 126 (22.3) | 172 (22.5) | 19 (32.8) | 172 (22.5) |
| Other race, non-Latinx | 47 (8.3) | 80 (10.5) | 3 (5.2) | 80 (10.5) |
| Injury type, n (%) | | | | |
| Gunshot wound | 371 (65.7) | 339 (44.4) | 50 (86.2) | 339 (44.4) |
| Stab wound | 194 (34.3) | 423 (55.4) | 8 (13.8) | 423 (55.4) |
| Admitted to hospital, n (%) | | | | |
| Yes | 386 (68.3) | 347 (45.5) | 51 (87.1) | 347 (45.5) |
| No | 179 (31.7) | 416 (54.5) | 7 (12.1) | 416 (54.5) |
| Trauma activation, n (%) | | | | |
| Yes | 507 (89.7) | 526 (68.0) | 56 (96.6) | 526 (68.0) |
| No | 58 (10.3) | 237 (31.1) | 2 (3.4) | 237 (31.1) |
| Mental health disorder diagnosis, n (%) | | | | |
| Yes | 81 (14.3) | 119 (15.6) | 8 (13.8) | 119 (15.6) |
| No | 484 (85.7) | 644 (84.4) | 50 (86.2) | 644 (84.4) |
| Median home neighborhood ICE† (IQR) | −0.12 (−0.21 to 0.05) | −0.15 (−0.23 to 0.05) | −0.14 (−0.25 to −0.07) | −0.15 (−0.23 to 0.05) |

BMC = Boston Medical Center; HVIP = hospital-based violence intervention program; ICE = race-poverty index of concentration at the extremes.

* The treatment group in trial B is a subset of the treatment group in trial A. Both trials share the same comparison group.

† Scaled [−1 to 1] (lower = more deprived, higher = more privileged).

any-engagement trial (that is, control participants were persons with no engagement through week 4) to facilitate clear interpretation and comparison between trials.

Outcomes and Follow up

The primary outcome was a combined endpoint encompassing violent reinjury and violence perpetration because high risk for victimization and perpetration typically converges in the same persons (27), and HVIPs use the same approaches to prevent both outcomes. Outcomes were not limited to penetrating injuries (unlike index injuries). In both the target trial and our emulation, follow-up started at baseline (that is, date of index injury) and ended at the earliest date of outcome occurrence, 3 years, or 31 December 2023. All-cause (except homicide) mortality was used to capture censoring due to nonviolent events.

Causal Estimand

Our target trial protocol includes estimating intention-to-treat effects and the per-protocol effects for

each treatment strategy. The target trial emulation estimated only the observational analogues of the per-protocol effects.

Statistical Approach

Because both target trial protocols involved a grace period to determine treatment status, we used the clone-censor-weight procedure (28). Eligible persons were cloned to both treatment and control and censored at the first week that their data deviated from an assigned protocol. All persons received time-varying, stabilized, inverse probability weights for protocol adherence, administrative censoring, and nonviolent deaths. Weights were estimated via logistic regression and truncated at prespecified tails (1st and 99th percentiles). Inverse probability weight models included all patient-level covariates listed above. We adjusted only for baseline covariates (the observational analogue of prerandomization factors) that predicted adherence and other censoring because postrandomization covariates (for example, social or clinical factors) were not available.

Table 3. Crude Service Contacts and Violence Outcomes by Treatment Assignment*

| Contacts and Outcomes at 3 y Postinjury | Target Trial A | | Target Trial B | |
|---|---|--|---|--|
| | Any Engagement (Within 4 wk Postinjury) | Comparison Group (Did Not Engage Within 4 wk Postinjury) | Sustained Engagement (>4 wk of First 8 wk Postinjury) | Comparison Group (Did Not Engage Within 4 wk Postinjury) |
| Total, n (%) | 565 (42.5) | 763 (57.5) | 58 (4.4) | 763 (57.5) |
| Median service contacts (IQR) | 3 (1-7) | 0 (0-0) | 15.5 (11-23) | 0 (0-0) |
| Any violence outcome, n (%) | 68 (12.0) | 120 (15.7) | 2 (3.4) | 120 (15.7) |
| Violent reinjury, n (%) | 41 (7.3) | 67 (8.8) | 0 (0) | 67 (8.8) |
| Violence perpetration, n (%) | 27 (4.8) | 53 (7.0) | 2 (3.4) | 53 (7.0) |

* The treatment group in trial B is a subset of the treatment group in trial A. Both trials share the same comparison group. Groups do not include the cloned observations included in the modeling procedures.

We applied these weights in a pooled logistic regression model for the discrete-time hazard. For the baseline hazard function, we interacted time terms with indicator variables for treatment strategy to allow the function to vary by group; we tested alternatives by comparing AIC, yielding a quadratic time function for the any-engagement analysis and a fourth-degree natural spline time function for the sustained engagement analysis. The model also included all patient-level covariates listed above.

We estimated the 3-year cumulative incidence (risk) of the outcome for each treatment strategy and 3-year risk ratios. We estimated 95% CIs for the cumulative incidence curves with nonparametric bootstrap with 1000 iterations. Uncertainty for between-arm comparisons was assessed using bootstrap CIs for the risk ratio. Because there is no agreed-upon follow-up period of clinical relevance for reinjury or violence perpetration, we compared study arms at 52 weeks, 104 weeks, and 156 weeks (that is, 1, 2, and 3 years) to facilitate direct comparisons with other work using these follow-up periods (29-32).

We also conducted post hoc analysis to assess service delivery outcomes between the low- and high-dose treatment groups. Using needs categories from Pino and colleagues (18), we tallied the needs identified by treatment group and whether those needs were met.

This study was approved by the Boston University Medical Institutional Review Board. We followed the TARGET reporting guideline (33). Data were analyzed using R, version 4.2.2 (R Project for Statistical Computing). We adapted some analytic code from Murray and colleagues (34).

Role of the Funding Source

Study funders had no role in the design, conduct, and analysis of the study nor the decision to submit the manuscript for publication.

RESULTS

Figure 1 shows flow charts of participants for both trials, and Table 2 shows baseline characteristics of the 1328 eligible persons. Supplement Table 1

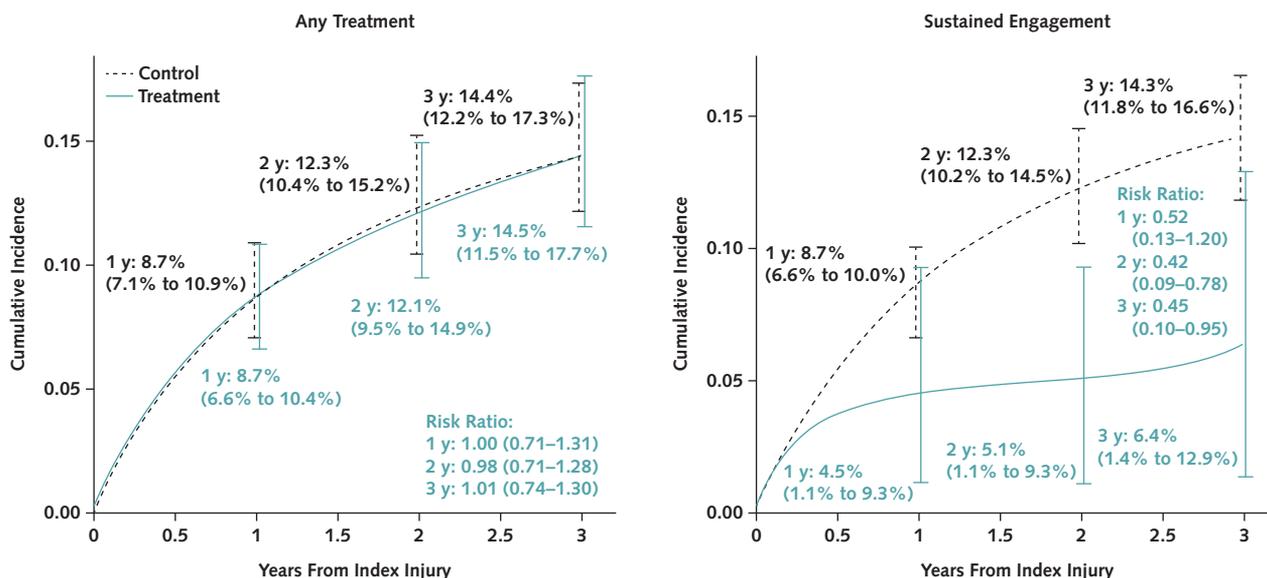
(available at Annals.org) compares those persons to the overall pool of violently injured persons. The eligible individual population was 68% Black and 25% Hispanic, had a mean age of 24.2 years, was mostly cisgender male (85%), and presented with a gunshot wound (55%). Persons in the study population typically resided in neighborhoods burdened by racialized economic segregation (median ICE, -0.14 [scale, -1 to 1]). Seventeen people were censored for nonviolent deaths, and 169 were censored administratively because they had not completed 3 years' follow-up by the end of 2023.

Slightly less than half of the study population ($n = 565$; 42.5%) engaged with the HVIP within 4 weeks of index injury; of these, 58 (10.2%, or 4.4% of the total study population) had sustained engagement. Gunshot injury and hospital admission were higher among those who engaged than those who did not; male gender was less common (Table 2).

As expected, VIAP contacts over the full study period were near zero among those who did not engage early (median, 0 contacts [IQR, 0 to 0]). Study-period contacts were modest among all early engagers (median, 3 contacts [IQR, 1 to 7]), but were much higher among high-frequency early engagers (median, 15.5 contacts [IQR, 11 to 23]).

Community violence events (that is, violent reinjury or violence perpetration) were most common in months 1 to 3 after index injury, which comprised 8% of all follow-up time but saw 21% of all violence-related outcomes (Supplement Figure 1, available at Annals.org). Within 3 years of index injury, community violence events were observed for 12.0% of persons who adhered to the any-engagement protocol and 15.7% of the comparison group (Table 3). Community violence outcomes were lower in the sustained engagement group (3.4%).

In the any-treatment analysis, engagement was not associated with community violence outcomes: adjusted cumulative incidence was roughly equal at all follow-up time points between the treatment and control conditions (Figure 2, left). In the sustained engagement analysis, treatment was associated with considerably lower incidence (4.5% [95% CI, 1.1% to

Figure 2. Cumulative incidence curves for violent reinjury or violence perpetration.

Any-treatment (*left*) and sustained engagement (*right*) strategies, standardized by inverse probability of censoring weights, with CIs at 1, 2, and 3 years. Curves show marginal cumulative incidence by arm, with 95% CI bars from a nonparametric bootstrap with 1000 iterations. For between-arm comparisons, risk ratios were calculated within each bootstrap draw and used to generate CIs. Risk-ratio CIs were used to assess uncertainty for between-arm comparisons, rather than an overlap of arm-specific CIs (35).

9.3%] at 1 year; 5.1% [CI, 1.1% to 9.3%] at 2 years; 6.4% [CI, 1.4% to 12.9%] at 3 years) versus the control strategy (8.7% [CI, 6.6% to 10.0%] at 1 year; 12.3% [CI, 10.2% to 14.5%] at 2 years; 14.3% [CI, 11.8% to 16.6%] at 3 years) (Figure 2, *right*). The corresponding proportional risk reductions were 47.6% (CI, –19.8% to 86.7%) at 1 year, 58.5% (CI, 21.6% to 91.2%) at 2 years, and 55.3% (CI, 4.9% to 90.2%) at 3 years. The CIs were wide but supported a finding of reduced risk at 2 and 3 years.

The sustained engagement treatment group had 69% more needs identified and 86% more needs met than the any-engagement treatment group (Table 4). The sustained engagement group had higher success rates for 2 of the needs considered hardest to meet (18): education (41% in sustained engagement vs. 34% in any-engagement) and employment (76% in sustained engagement vs. 59% in any-engagement). However, housing needs were met at low rates in both groups (51% in sustained engagement vs. 46% in any-engagement).

DISCUSSION

Among persons who engaged consistently with VIAP, we found sizable, long-term reductions in community violence outcomes compared with persons who did not engage. Few other community violence prevention interventions have reported such large-magnitude effects. However, the sustained engagement group was a small subset (10%) of all persons who engaged with VIAP, and the statistical precision

of our estimates was low. In the any-engagement trial, we found no effects on community violence rates. These findings demonstrate not only the ability of HVIP programs to improve long-term outcomes for persons at high risk for violent reinjury and violence perpetration but also the need to maximize participant engagement and to align HVIPs with other community violence intervention strategies.

Our results bolster the evidence in favor of HVIPs for CVI. The sustained engagement clients of VIAP had community violence reductions comparable to the effects reported in recent evaluations of promising CVI programs. For example, evaluations of READI (Rapid Employment and Development Initiative) Chicago (36) and Chicago CRED (Create Real Economic Destiny) (37) reported slightly larger effects for violence perpetration outcomes (65% and 73%, respectively) but null, or more modest, effects on a combined violence indicator like ours. Although ours is not the first HVIP study to report positive results, past evaluations have been criticized for small samples, short follow-up periods, and inappropriate comparison groups (12, 13). Our study used a large sample of persons who presented with violent penetrating injuries to the same hospital during the same time period with up to 3 years of follow-up. Our use of target trial emulation added clarity about the causal questions being tested and avoided common pitfalls in observational study design, including immortal time bias from assigning treatment status after the start of follow-up.

At the same time, our findings underscore the importance of dosage in HVIP implementation. On average,

Table 4. Needs Identified and Goals Achieved Through VIAP Client Services, by Treatment Group*

| Need Type | Engagement Level | Needs Identified | Needs Met | Needs Identified per Person | Needs Met per Person | Success Rate, % |
|---------------------------|------------------|------------------|-----------|-----------------------------|----------------------|-----------------|
| Basic needs | Sustained | 41 | 34 | 0.71 | 0.59 | 83 |
| | Any | 175 | 137 | 0.31 | 0.24 | 78 |
| Education | Sustained | 17 | 7 | 0.29 | 0.12 | 41 |
| | Any | 98 | 33 | 0.17 | 0.06 | 34 |
| Employment | Sustained | 34 | 26 | 0.59 | 0.45 | 76 |
| | Any | 208 | 123 | 0.37 | 0.22 | 59 |
| Family | Sustained | 31 | 26 | 0.53 | 0.45 | 84 |
| | Any | 168 | 116 | 0.30 | 0.21 | 69 |
| Housing | Sustained | 37 | 19 | 0.64 | 0.33 | 51 |
| | Any | 203 | 93 | 0.36 | 0.16 | 46 |
| Injury recovery | Sustained | 52 | 50 | 0.90 | 0.86 | 96 |
| | Any | 315 | 265 | 0.56 | 0.47 | 84 |
| Legal | Sustained | 47 | 35 | 0.81 | 0.60 | 74 |
| | Any | 284 | 177 | 0.50 | 0.31 | 62 |
| Medical and mental health | Sustained | 55 | 45 | 0.95 | 0.78 | 82 |
| | Any | 382 | 346 | 0.68 | 0.61 | 91 |
| Personal wellness | Sustained | 12 | 10 | 0.21 | 0.17 | 83 |
| | Any | 51 | 28 | 0.09 | 0.05 | 55 |
| Total | Sustained | 645 | 468 | 5.62 | 4.34 | 77 |
| | Any | 1884 | 1318 | 3.33 | 2.33 | 70 |

VIAP = violence intervention and advocacy program.

* There were 58 persons in the sustained engagement group and 565 persons in the any-engagement group. The sustained engagement group was a subset of the any-engagement group.

persons who engaged with VIAP did not experience reductions in community violence outcomes at 3 years postinjury. This finding is logical, considering that this group had relatively few program encounters and relatively few needs met. Indeed, low dosage may explain why the recent HVIP-based RCT by Lyons and colleagues found no violence prevention effects (30). Further work is needed to understand why most clients of VIAP engage at low dosage. Readiness is one possible explanation (discussed further below) but social barriers must also be considered. Evidence from the current study—that sustained engagement reduces risk for future violence involvement—may be useful to prospective HVIP clients and can help inform program design. Some HVIPs require a heavier time commitment than VIAP (for example, weekly meetings for the first 8 weeks [15, 38]), suggesting that high-dose strategies are practicable.

Uptake is crucial because policymakers and funders may reasonably expect HVIPs to reduce violence in the population that is offered their services. If most clients do not engage with the service frequently, it seems that this goal will not be achieved. However, HVIPs must consider the diverse needs of clients. Reinjury and violence perpetration are among dozens of proposed outcomes from a recent survey of HVIP providers (11). Low-dose engagement may provide different benefits, and allowing low-dose engagement may increase the total benefits delivered by the program. Moreover, traditional studies likely underestimate the violence prevention benefits of individual-level CVI, which could extend to the friends, family, and neighbors of program enrollees—for future research, computer simulation modeling could explore these effects (39, 40).

Another issue is the timing of community violence risk. We found risk was highest in the first 3 months postinjury, meaning some persons who might otherwise have engaged with VIAP were unable because they were injured or incarcerated first. Our results indicate that HVIP effects may accrue gradually, as engagement is sustained, leaving a gap in prevention even for those who engage. To address risk immediately postinjury, HVIPs could partner with street outreach programs that deescalate disputes and prevent retaliation, as others have noted (13).

There are limitations. Our study was observational and therefore lacked some strengths of an RCT. However, we strengthened causal inference by using methods to reduce susceptibility to nonrandom dropout, using target trial emulation to avoid immortal time bias, and conducting a dose-response analysis. Our estimated effects using this approach were sizable. Moreover, RCTs are ethically dubious because HVIPs are widely established as a standard of care (14). Even if HVIP effectiveness for violence prevention was still disputed, HVIPs provide needed services that would not be appropriate to withhold. Thus, building knowledge from rigorous observational studies is needed.

Our observational approach could still be confounded by unmeasured factors associated with early or sustained engagement. It is unclear a priori which direction self-selection would bias this analysis because engagers might either a) correctly assess themselves as high risk or b) be more willing or able to accept services, which could lower their risk. The latter characteristic has been described as “readiness,” and it is a key prerequisite of most individual-level antiviolence

programs, which depend on voluntary participation (see Ross and colleagues for a detailed discussion [37]). If effects can only be seen among those displaying readiness, increasing readiness should be a major focus for these programs.

It is unknown how our results may generalize to other HVIPs. There is no single, agreed-upon package of services for all HVIPs, and VIAP offers more than most. The VIAP program also has resources to serve a larger swath of patients, though we restricted our analysis to the young adults prioritized by most HVIPs.

Finally, our outcomes data did not include every possible source of community violence outcomes. Violent reinjuries treated at Boston hospitals other than BMC, those unknown to Boston police, and those occurring outside of the city were not captured. However, BMC treats approximately 70% of patients with gunshot and stab wounds in Boston (21). We also omitted persons residing more than 5 miles from Boston. Although regional police intelligence center reports are the most comprehensive data source available, they omit violence events unknown to the police and could include false positives in which police allegations were incorrect.

In conclusion, VIAP reduced community violence outcomes for the subset of persons who sustained engagement over time. This result affirms the violence prevention potential of HVIPs and demonstrates the need for a new generation of HVIP research focused on effective implementation.

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