AER: Insights 2023, 5(4): 527–545
https://doi.org/10.1257/aeri.20220427

Cognitive Behavioral Therapy Reduces Crime and Violence over Ten Years: Experimental Evidence[†]

By Christopher Blattman, Sebastian Chaskel, Julian C. Jamison, and Margaret Sheridan*

Several small, short-term, or nonexperimental studies show that cognitive behavioral–informed interventions reduce antisocial behaviors over one to two years, but persistence research is rare. We followed 999 high-risk men in Liberia ten years after randomization into eight weeks of low-cost, nonspecialist-led therapy; $200 cash; both; or neither. A decade later, antisocial behaviors (such as robbery and drug selling) fell 0.2 standard deviations from therapy alone—significantly greater than the one-year impacts. Meanwhile, men who received therapy plus cash were 0.25 standard deviations less antisocial—similar to one-year results. In both cases, impacts were concentrated in men exhibiting highest baseline risk. (*JEL* D91, K42, O15, O17)

Cognitive behavioral therapy, or CBT, is a form of psychotherapy long used to address self-destructive thoughts and disorders, including depression, anxiety, and posttraumatic stress (Beck 1979; Johnsen and Friborg 2015). Traditionally, trained professionals deliver CBT in individual clinical settings, but therapies led by peers, community health workers, and other nonspecialists can also relieve moderate to serious symptoms of depression and other disorders (Singla et al. 2017; Baranov et al. 2020; Barker et al. 2022; Bhat et al. 2022).

Increasingly, psychotherapy is being used to tackle antisocial behaviors such as crime and violence. In the United States, CBT-informed programs are quickly becoming one of the main approaches to prevent shootings (Clark 2010; Feucht and

*Blattman: University of Chicago (email: blattman@uchicago.edu); Chaskel: Instiglio (email: sebastian.chaskel@gmail.com); Jamison: University of Exeter and Global Priorities Institute (email: j.jamison@exeter.ac.uk); Sheridan: University of North Carolina (email: sheridan.margaret@unc.edu). Rohini Pande was coeditor for this article. The Network for Empowerment and Progressive Initiatives (NEPI) and Global Communities implemented the program, and NEPI provided ongoing support. Innovations for Poverty Action coordinated all research activities. For research assistance in this latest round, we thank G. Dackermue Dolo, Victor Gamarra, Sebastián Hernández, Walker Higgins, Andreas Holzinger, Bruno Aravena Maguida, Laura McCargo, Anna Mysliwiec, Albert Nyuangar, and Newton Toe. We thank Sara Heller for comments. This round of the study was funded by the National Science Foundation (SES 1919399) and the Wellspring Philanthropic Fund. The contents of this study are the responsibility of the authors and do not necessarily reflect the views of their employers, funding agencies, or governments. The authors have no relevant or material financial interests that relate to the research described in this paper. This study is registered at the randomized controlled trials registry of the American Economic Association under RCT ID AEARCTR-0006736 (Blattman et al. 2020).

[†]Go to https://doi.org/10.1257/aeri.20220427 to visit the article page for additional materials and author disclosure statement(s).

Holt 2016; Abt 2019). Cities and development institutions across Latin America have also begun replicating these programs (Abt and Winship 2016; Chioda 2017; Avitable et al. 2022; Dinarte and Egana-delSol 2019). In all these places, policymakers are searching for preventative interventions, especially alternatives to costly and coercive policing and imprisonment.

Most CBT-informed programs have two elements. First, a facilitator or counselor tries to help the subject become more conscious of their harmful automatic thoughts—especially inaccurate or negative thinking about themselves or others. The idea is that, through awareness, the subject can learn to react in more constructive ways. Second, the literature suggests that sustained changes in behavior also come from practice. That is, thoughts influence actions but actions also shape thoughts—a kind of "learning by doing."

In theory, violence and antisocial behaviors could be amenable to CBT to the extent that they are the product of automatic decision-making and distorted thinking (Beck 2011). For instance, people may react antisocially in haste, have difficulty managing their emotions, fail to consider the consequences of their actions, overlook nonviolent solutions, or hold on to exaggerated and negative beliefs about their rivals.

There is little long-term evidence on such programs, however. Initial enthusiasm for CBT came from a large number of mostly small, nonexperimental studies of criminal rehabilitation programs in the United States. Meta-analyses suggested that these programs were generally effective at reducing antisocial behavior and criminal recidivism, typically over horizons of a few months to a year (Pearson et al. 2002; Del Vecchio and O'Leary 2004; Wilson, Bouffard, and Mackenzie 2005; Lipsey, Landenberger, and Wilson 2007; Saini 2009).

Meanwhile, two large-scale randomized trials suggested that the effects of CBT on antisocial behavior could be short-lived. In one, Heller et al. (2017) examined the one- to two-year effects of an in-school program, Becoming A Man (BAM), with nearly 5,000 at-risk high school students in Chicago joining small-group sessions led by social workers. The study found that criminal arrests fell by about half during the program period but that the effects dissipated shortly afterward. Arrests were still down, but the effects were smaller and no longer statistically significant.

Around the same time, Blattman, Jamison, and Sheridan (2017) studied the one-year effects of an intervention in West Africa: the Sustainable Transformation of Youth in Liberia (STYL) program. STYL recruited nearly 1,000 criminally involved young men and offered half of them eight weeks of nonspecialist-led group therapy. The sessions used CBT techniques in traditional ways (e.g., managing emotions) as well as nontraditional ones (e.g., adopting noncriminal identities). A month later, treated men reduced their antisocial behaviors by 0.2 standard deviations. Thefts and robberies, for example, were about 30 percent lower than the control group. As with BAM, however, a year later the effects of therapy alone had diminished by two-thirds—to 0.075 standard deviations—and was no longer statistically significant.

There were signs, however, that CBT plus an economic intervention could produce lasting behavior change. A quarter of the STYL sample received a $200 cash grant in addition to therapy. Both one month and one year later, the men who received therapy and cash reduced antisocial behaviors by 0.25 standard deviations. For instance, compared to the control group, they were 44 percent less likely to sell

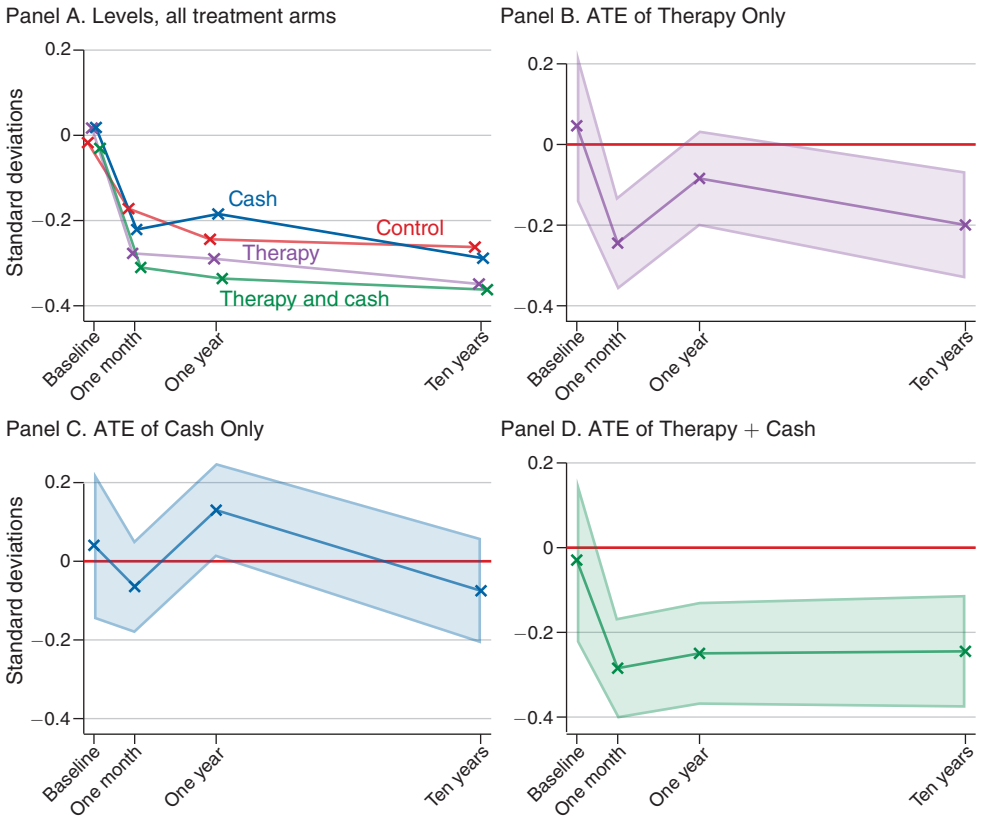

FIGURE 1. PROGRAM IMPACTS ON A STANDARDIZED INDEX OF ANTISOCIAL BEHAVIORS OVER TIME—LEVELS AND AVERAGE TREATMENT EFFECTS (ATES)

Notes: The estimates control for baseline covariates and randomization block fixed effects. The antisocial behaviors index is a composite of underlying survey variables, and here the index is standardized to have zero mean at baseline and unit standard deviation across all survey rounds. Continuous and unbounded variables in each index have been top-coded at the ninety-ninth percentile to reduce the influence of extreme values. The 95 percent confidence intervals use heterosketastic-robust standard errors.

drugs and committed 40 percent fewer thefts and robberies. The cash raised earnings for only a few months, so these one-year impacts were not necessarily driven by higher incomes. Rather, we speculated that cash enabled participants to practice the therapeutic lessons for months after therapy ended, helping to entrench the changes in behavior and identity.

In this paper, we return to the STYL sample in Liberia roughly ten years after subjects completed the program, finding and interviewing 93 percent of surviving members of the original sample. Figure 1 displays levels of antisocial behavior and average treatment effects over time using a standardized index of seven violent and criminal behaviors and attitudes: drug selling, stealing, interpersonal fighting, carrying a weapon, arrests, hostile attitudes, and domestic abuse. The results suggest that *both* therapy alone and therapy plus cash have durable, long-run impacts that equal or even exceed the one-year impacts of therapy plus cash. The effects of therapy plus cash are somewhat larger and more robust, but there are signs that therapy is effective on its own, especially (as we will see) among the highest-risk men. Altogether

these results suggest that CBT-informed psychotherapy may indeed be a powerful tool for sustained violence reduction.

I. Intervention and Experiment

A. *Liberia*

Liberia is a coastal West African nation with roughly five million people. It emerged from 15 years of civil wars and instability in 2003. At the outset of the study, in 2009, the country had enjoyed a fragile order for six years. Among the threats to peace, the government and UN mission were particularly concerned with poorly integrated ex-fighters and other young men involved in crime. They also worried about political violence, including riots, election violence, and mercenary recruitment (Blattman and Annan 2016; Hoffman 2011).

The largest concentration of high-risk men was in the capital, Monrovia. A few thousand young men had turned to drug dealing, pickpocketing, and armed robbery. Brawls and knifings were commonplace. Only a third of men were ex-fighters. Rather, with peace and normalcy, Monrovia was beginning to experience the same problem as so many other large cities around the world: poor, disenfranchised youth drifting into illicit and violent careers.

B. *STYL*

We studied two interventions: cash grants and nonclinical, CBT-informed therapy and training sessions. We used a 2×2 factorial design, producing four treatment arms: Therapy Only, Cash Only, Therapy + Cash, and a control condition.

Therapy.—The therapy was designed by a small local nonprofit, the Network for Empowerment and Progressive Initiatives (NEPI), in cooperation with the authors. NEPI had worked with high-risk men in Liberia for more than a decade. Over the years, they had acquired, tested, and adapted CBT and other techniques from Western manuals as well as through trainings from international organizations.

STYL required counseling by nonspecialists, if only because (to our knowledge) there were no counseling psychologists in all of Liberia at the time. NEPI trained its own facilitators. Most were ex-members of armed groups or former criminals. Most were also past graduates of a previous NEPI rehabilitation program.

Over eight weeks, a pair of NEPI facilitators led groups of 20 men, three times a week, for three to four hours per session. On alternate days when groups did not meet, the facilitators met with men at home or work to provide one-on-one counseling and encouragement. Sessions employed a mix of lectures, group discussions, and practice, including role-playing in class; homework that required practicing tasks; exposure to real situations; and in-class processing of experiences of executing these tasks. These tasks increased in difficulty over time.

The curriculum focused on three related kinds of behavior change. First, to foster future orientation over present-biased behavior, the program taught skills of self-control to manage emotions, reduce impulsivity, become more conscientious and persevering, and become more planful and goal oriented in daily activities.

Second, the program strongly emphasized how to deal with anger, interpersonal violence, and threatening situations. Third, STYL helped men learn to behave and self-identify as normal society members rather than as an outcast or criminal. This identity change component is somewhat unusual compared to standard CBT programs. Online Appendix A.1 elaborates.

Cash Grants.—Winners of the cash lottery immediately received $200 in cash. Losers received $10 as a consolation. Subjects were told that the grant was unconditional and they were free to do what they wished. They were also given 15 minutes of information on how to keep the money safe (e.g., depositing it with a bank) and examples of what they could use it for (e.g., starting a small business).

We anticipated that the cash would relieve liquidity and credit constraints and enable men to engage in petty trade or other legal earnings opportunities. This prediction was in line with broad-based evidence across Africa that unemployed youths have high short-term returns to capital, but this claim had never been tested on a high-risk population (Haushofer and Shapiro 2018; Blattman, Fiala, and Martinez 2020).

Cost.—Delivering therapy cost NEPI roughly $189 per subject, plus $216 for the grant and $125 for administration. The total for both interventions—$530—is equal to about eight months of the sample's baseline earnings.

C. *Experiment*

The study recruited 999 men actively involved in crime and violence. They were age 25 on average. We focused on five mixed-income residential neighborhoods in Monrovia with large populations of high-risk men.

All recruitment was handled by NEPI. In each neighborhood, certain places, groups, and professions had reputations for crime and violence involvement, and recruiters targeted these locations and people. Online Appendix A.2 elaborates.

We tried to minimize general equilibrium effects and spillovers between treatment and control group members. We worked in neighborhoods with tens of thousands of residents, recruiting less than 1 percent of adult men. NEPI recruiters were also instructed to approach just one out of every seven to ten potentially high-risk subjects they identified on the street. This avoided more than 10 percent of high-risk men being treated in a neighborhood. Online Appendix A.3 elaborates.

The 999 subjects were randomized to therapy via public draw. Therapy began a week later. Of those assigned to therapy, 95 percent attended at least the first week and two-thirds attended most sessions.

After the final week of therapy, we recontacted all 999 men and asked them to return for a second, surprise draw for the grants. Of those assigned to the grant, 98 percent received one. Assignment to the four arms was largely balanced along covariates (see online Appendix A.3).

D. *Data*

All data are self-reported survey outcomes. As one of the world's poorest countries, Liberia does not have administrative data on arrests, crimes, or violence. The

main advantage of this strategy is unusually rich outcome measures. A disadvantage is potential bias in self-reported results, discussed in Section IIIC.

We attempted to survey each subject seven times: (i) at baseline; (ii and iii) two and five weeks after the grants; (iv and v) 12 and 13 months after grants, and roughly (vi and vii) 114 and 115 months after grants. We ran pairs of surveys to reduce noise in outcomes with low autocorrelation, such as earnings or criminal activity. We average these pairs into one-month, one-year, and ten-year outcomes.

Subjects typically had no fixed address and lived under aliases or clandestinely. By collecting social network and contact information and through intensive tracking, we located most surviving respondents. After one year, we interviewed 947 of the 999 (95 percent). After ten years, we surveyed 833 (83 percent of the full sample), plus the friends or relatives of 103 who had died. Therefore, we have outcomes for 93 percent of known survivors. Of the remaining 63, 14 refused the survey, 9 were unreachable (out of the country), 7 were imprisoned, and 33 could not be found.

Response rates varied slightly by treatment group, though no differences are statistically significant. After ten years, for example, Therapy Only subjects were 4 percentage points less likely to be found than controls, partly due higher levels of mortality (not signficiant). Therapy + Cash subjects were 4 percentage points more likely to be found. Online Appendix A.4 elaborates.

II. Short-Run Impacts and Long-Run Hypotheses

As seen in Figure 1, antisocial behaviors fell steeply in all treatment arms in the first year after randomization, even in the control arm.[1] But antisocial behaviors fell further and more quickly in the therapy arms. In the Therapy + Cash arm, antisocial behaviors declined 0.31 standard deviations compared to the control group after one month ($p = 0.001$) and by 0.25 after one year ($p = 0.005$). In the Therapy Only arm, they declined by 0.25 after one month ($p = 0.005$) and 0.08 after one year ($p = 0.36$). See Blattman, Jamison, and Sheridan (2017) and also Table 2.[2]

Economic performance was also a primary outcome of interest in the one-year study. This was largely because of the Cash Only arm but also because in principle, any increase in legal earnings could reduce criminality by increasing the returns to legal enterprises and raising the opportunity cost of antisocial behavior.[3] We measured performance with an index of six components: earnings, consumption, homelessness, savings, investment, and employment.

According to surveys, most of the grant recipients used the funds for small enterprise, such as petty trading. After one month, grants increased economic performance by roughly 0.6 standard deviations in both the Cash Only and Therapy + Cash arms ($p < 0.001$). There was no effect of Therapy Only.

[1] At the time, there were two possible explanations. One is mean reversion—a mechanical effect of recruiting people at their nadir. Another possibility is life cycle effects—in many countries, crime rates peak in early adulthood. Given that antisocial behaviors did not decline between the one- and ten-year surveys, we think this suggests mean reversion over life cycle effects.

[2] In the Cash Only arm, there was no evidence of improvement: antisocial behaviors declined by just 0.08 standard deviations after one month ($p = 0.385$) and actually rose by 0.13 after one year ($p = 0.18$).

[3] For example, Becker (1968); Blattman and Ralston (2015). In Blattman, Jamison, and Sheridan (2017), we developed a theoretical model of occupational choice between criminal and noncriminal careers that illustrated how each treatment could affect economic performance in the short and long run.

Within a year, however, the impacts of cash had dissipated. Grants increased one-year economic performance by a negligible 0.002 standard deviations from Cash Only ($p = 0.98$) and by 0.056 with Therapy + Cash ($p = 0.55$). Qualitative interviews suggested that in the year following their investments, most of the enterprises failed due to theft, seizure by authorities, or adverse shocks such as weather or illness.

Given that the grants raised employment and incomes for only a few months, why did Therapy + Cash have larger and more robust effects on antisocial behaviors than Therapy Only? After all, by the time of the one-year survey, the Cash Only arm should no longer have had any effect on the opportunity cost of engaging in crime.

The theory underlying CBT suggested one hypothesis: receiving cash was akin to an extension of therapy in that it provided more time for the men to practice independently and to reinforce their changed skills, identity, and behaviors. That is, therapy helped participants change their aspirations, identity, and behavior. The grant then supplied them with the means to maintain their new identity—to avoid homelessness, to feed themselves, to continue to dress decently, and to avoid turning back to crime. The grant was also a way to practice their new skills and identity. In this way, the grant may have parallels to "booster sessions" commonly used in therapy. Experimental research on CBT for aggression or substance abuse indicates that follow-up therapy sessions weeks or months after the intervention improve 12- to 13-month outcomes (e.g., Lochman 1992).

Long-Run Hypotheses.—Given that the effects of cash disappeared within a few months, for the ten-year survey we preregistered only one primary outcome: the index of antisocial behaviors. Granted, the absence of economic effects in the short run does not preclude them emerging later. Nonetheless, we now treat economic performance as a secondary outcome, alongside a variety of other mechanisms.[4]

We also collected expert predictions. We sent an anonymous survey to 88 scholars who had cited the one-year results, and 30 responded. Almost all expected Therapy Only or Cash Only to have no effect on antisocial behaviors after ten years. For Therapy + Cash, a third predicted no effect and two-thirds predicted diminished impacts, for an average prediction of about one-third the one-year impact. Our own expectations were in line with these forecasts.

III. Ten-Year Impacts

A. *Antisocial Behavior*

Table 1 reports means and average treatment effects for the index and its components. Table 2 compares one- and ten-year program impacts. We estimate intent-to-treat effects by regressing outcomes on indicators for each treatment arm, a vector of baseline characteristics, and randomization block fixed effects (see online Appendix B.1).

First, both Therapy Only and Therapy + Cash lead to large, statistically significant reductions in antisocial behavior. After ten years, Therapy Only reduces the

[4] See Social Science Registry AEARCTR-0006736 at https://www.socialscienceregistry.org/trials/6736. In general, we follow the one-year study for index construction, covariate adjustment, and heterogeneity analysis. The only difference is that we no longer adjust standard errors for two primary outcomes.

Table 1—Ten-Year Impacts on Antisocial Behaviors and Mortality

		Average treatment effects			Differences	
	Control mean (1)	Therapy Only estimate (SE) [p-value] (2)	Cash Only estimate (SE) [p-value] (3)	Both estimate (SE) [p-value] (4)	Therapy Only vs. both estimate [p-value] (5)	Cash Only vs. both estimate [p-value] (6)
Antisocial behaviors	0.116	−0.200	−0.075	−0.245	−0.045	−0.170
	(1.119)	(0.106)	(0.105)	(0.104)	(0.636)	(0.081)
		[0.059]	[0.476]	[0.019]		
Usually sells drugs (indicator)	0.102	−0.016	−0.024	−0.046	−0.029	−0.022
	(0.267)	(0.027)	(0.028)	(0.027)	(0.171)	(0.357)
		[0.542]	[0.390]	[0.092]		
Thefts/robberies in past two weeks (count)	1.827	−0.960	−0.239	−0.876	0.083	−0.636
	(4.855)	(0.400)	(0.433)	(0.432)	(0.788)	(0.047)
		[0.017]	[0.578]	[0.043]		
Disputes and fights in past two weeks (z-score)	−0.025	−0.136	0.017	−0.131	0.004	−0.149
	(0.871)	(0.076)	(0.086)	(0.067)	(0.941)	(0.052)
		[0.073]	[0.842]	[0.051]		
Carries a weapon on body (indicator)	0.132	−0.075	−0.005	−0.044	0.031	−0.039
	(0.339)	(0.033)	(0.036)	(0.035)	(0.329)	(0.282)
		[0.024]	[0.879]	[0.205]		
Arrested in past two weeks (indicator)	0.082	−0.012	−0.013	−0.029	−0.017	−0.016
	(0.238)	(0.024)	(0.022)	(0.023)	(0.454)	(0.442)
		[0.610]	[0.563]	[0.217]		
Aggressive behaviors (z-score)	0.027	−0.060	−0.029	−0.062	−0.002	−0.033
	(0.579)	(0.058)	(0.057)	(0.062)	(0.978)	(0.568)
		[0.301]	[0.611]	[0.316]		
Verbal/physical abuse of partner (z-score)	−0.019	0.032	−0.043	−0.082	−0.114	−0.039
	(0.931)	(0.109)	(0.106)	(0.109)	(0.289)	(0.708)
		[0.767]	[0.685]	[0.453]		
Any death	0.086	0.039	0.015	−0.007	−0.045	−0.021
	(0.282)	(0.030)	(0.030)	(0.028)	(0.108)	(0.456)
		[0.190]	[0.632]	[0.812]		
Violent death	0.018	0.020	0.002	0.002	−0.018	−0.000
	(0.134)	(0.017)	(0.015)	(0.015)	(0.266)	(0.974)
		[0.256]	[0.878]	[0.906]		
Health complications	0.064	0.009	−0.005	−0.010	−0.020	−0.005
	(0.245)	(0.024)	(0.026)	(0.023)	(0.389)	(0.833)
		[0.692]	[0.840]	[0.658]		

Notes: The table reports intent-to-treat estimates of each treatment arm after ten years, controlling for baseline covariates and randomization block fixed effects, as in equation (2) in online Appendix B.1. Indexes are standardized to have zero mean and unit standard deviation. Continuous and unbounded variables in each index have been top-coded at the ninety-ninth percentile to reduce the influence of extreme values. We rescale all indexes to have mean zero for this ten-year round (rather than normalize to baseline, as in Figure 1). Death indicators are for deaths before the two ten-year survey rounds. Online Appendix B.4 elaborates on mortality measures and coding. Heterosketastic-robust standard errors are reported in parentheses, and p-values are reported in brackets.

index of all seven antisocial behaviors by 0.20 standard deviations compared to the control group ($p = 0.059$)—almost three times the one-year impact of 0.075 (though this difference is not statistically significant). Therapy + Cash reduces antisocial behaviors by 0.25 ($p = 0.019$)—nearly identical to the one-year impact. Finally, Cash Only is associated with a smaller and not statistically significant decrease in antisocial behaviors (−0.075 standard deviations, $p = 0.47$), which we can distinguish from the effect Therapy + Cash with some confidence ($p = 0.08$).

Table 2—One- versus Ten-Year Impacts on Antisocial Behaviors and Secondary Outcomes

Outcome index (*z*-score)	One-year			Ten-year			One- vs. ten-year difference		
	Therapy Only (1)	Cash Only (2)	Both (3)	Therapy Only (4)	Cash Only (5)	Both (6)	Therapy Only (7)	Cash Only (8)	Both (9)
Antisocial behaviors	−0.084	0.130	−0.250	−0.200	−0.075	−0.245	−0.099	−0.192	0.026
	(0.093)	(0.097)	(0.088)	(0.106)	(0.105)	(0.104)	(0.120)	(0.129)	(0.117)
	[0.365]	[0.180]	[0.005]	[0.059]	[0.476]	[0.019]	[0.406]	[0.135]	[0.825]
Secondary outcomes									
Economic performance	0.073	0.002	0.057	0.081	0.026	0.190	−0.046	−0.008	0.137
	(0.104)	(0.099)	(0.095)	(0.115)	(0.115)	(0.112)	(0.135)	(0.139)	(0.133)
	[0.487]	[0.985]	[0.551]	[0.483]	[0.823]	[0.090]	[0.734]	[0.956]	[0.304]
Forward-looking time preferences	0.141	0.099	0.199	0.132	0.013	0.247	0.029	−0.017	0.061
	(0.095)	(0.095)	(0.098)	(0.096)	(0.097)	(0.093)	(0.124)	(0.124)	(0.124)
	[0.139]	[0.297]	[0.043]	[0.168]	[0.895]	[0.008]	[0.812]	[0.894]	[0.624]
Self-control skills	0.159	−0.025	0.244	0.178	0.049	0.119	0.056	0.100	−0.077
	(0.090)	(0.095)	(0.095)	(0.103)	(0.103)	(0.100)	(0.123)	(0.126)	(0.121)
	[0.080]	[0.794]	[0.011]	[0.086]	[0.634]	[0.235]	[0.646]	[0.427]	[0.527]
Identity and values	0.028	−0.084	0.099	−0.038	−0.092	0.100	−0.072	−0.001	−0.007
	(0.093)	(0.092)	(0.092)	(0.101)	(0.099)	(0.099)	(0.124)	(0.123)	(0.120)
	[0.767]	[0.362]	[0.278]	[0.703]	[0.349]	[0.310]	[0.564]	[0.995]	[0.957]
Positive self-regard/mental health	0.022	−0.024	0.227	0.088	−0.032	0.207	0.112	−0.006	−0.020
	(0.091)	(0.091)	(0.090)	(0.104)	(0.102)	(0.102)	(0.125)	(0.126)	(0.124)
	[0.808]	[0.792]	[0.012]	[0.398]	[0.754]	[0.042]	[0.371]	[0.961]	[0.874]
Substance abuse	−0.091	0.083	−0.073	−0.058	−0.046	−0.102	0.016	−0.097	0.002
	(0.081)	(0.082)	(0.079)	(0.096)	(0.094)	(0.096)	(0.111)	(0.112)	(0.111)
	[0.262]	[0.310]	[0.359]	[0.542]	[0.629]	[0.288]	[0.882]	[0.386]	[0.987]
Quality of social networks	0.059	−0.037	0.015	−0.026	−0.072	0.085	−0.127	−0.078	0.031
	(0.096)	(0.098)	(0.098)	(0.112)	(0.109)	(0.102)	(0.130)	(0.131)	(0.127)
	[0.540]	[0.711]	[0.877]	[0.814]	[0.513]	[0.407]	[0.328]	[0.552]	[0.807]

Notes: The table reports intent-to-treat estimates of each treatment arm after one and ten years, controlling for baseline covariates and block fixed effects, as in equation (2) in online Appendix B.1. All indexes are standardized to have zero mean and unit standard deviation. We conducted this standardization *across all endline rounds*, pooled. This aids comparability of treatment effects across rounds, but it means that the one-year point estimates reported in this table are slightly different than those reported in Blattman, Jamison, and Sheridan (2017). All such differences are trivial, however, and standardizing within rounds has no substantive effect on estimates or significance. Finally, as with previous analysis, continuous and unbounded variables in each index have been top-coded at the ninety-ninth percentile to reduce the influence of extreme values.

Second, while the Therapy + Cash estimate is larger and more robust, we cannot reject that it is equivalent to the effects of Therapy Only ($p = 0.639$). That said, note that the treatment effect on Therapy Only has a p-value above the typical threshold ($p = 0.059$) and gains or loses statistical significance depending on the specifications.

These impacts are broadly robust to changes in specification, as reported in online Appendix B.2. Therapy + Cash impacts are robust to changes in the control vector, index construction, censoring extreme values, and extreme attrition scenarios. The Therapy Only results gain statistical significance when we choose control variables using the double lasso method or when we treat each round as a different observation.[5] The results are also robust to attrition bounds that impute extreme values for missing sample members.[6]

[5] We did not use these more robust models or approaches in the one-year results, so we do not report them as our main specification. Note that these more robust specifications when applied to our one-year results do not lead to Therapy Only having statistically significant effects after one year.

[6] The results become less significant using "Lee bounds" that trim for excess attrition, but given the absence of statistically significant differential attrition, there is not a clear rationale for Lee bounds.

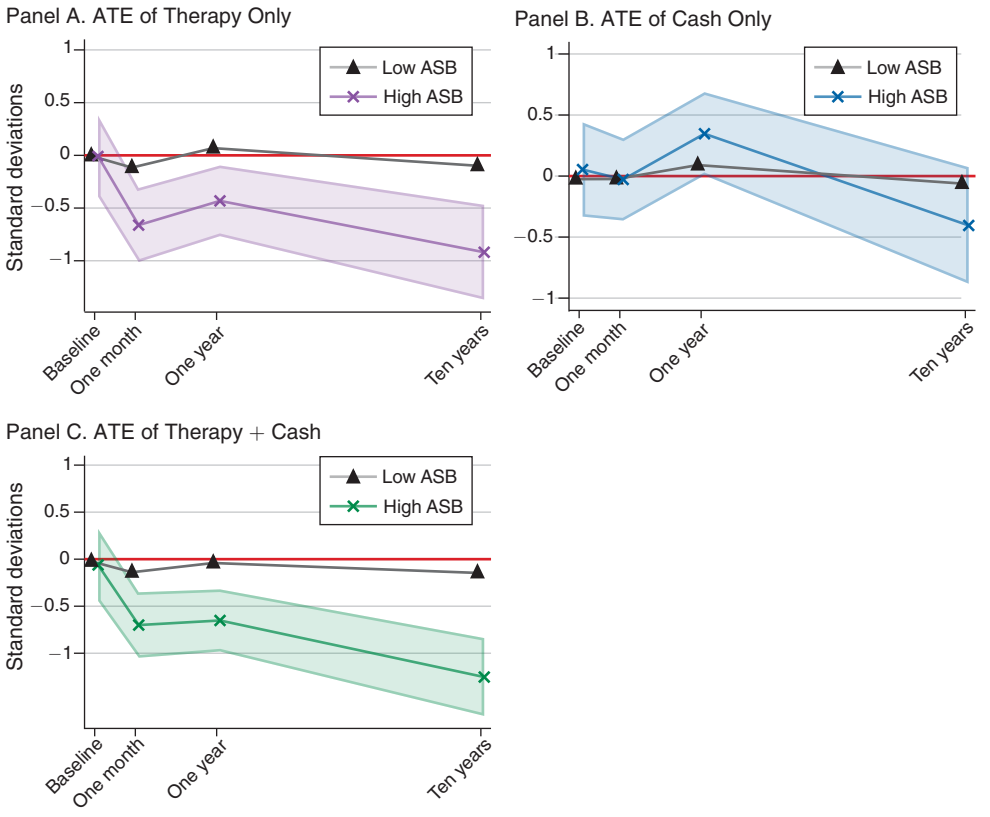

FIGURE 2. HETEROGENEITY IN PROGRAM IMPACTS BY BASELINE ANTISOCIAL BEHAVIOR

Notes: The figure reports intent-to-treat estimates of each treatment arm after ten years for two subgroups: those with baseline antisocial behavior (ASB) above the seventy-fifth percentile (high ASB) and those below it (low ASB). Online Appendix B.3 reports the full and prespecified heterogeneity analysis with a continuous measure of baseline antisocial behaviors.

Third, the impacts of Therapy Only and Therapy + Cash are greatest among the highest-risk men. As in the one-year evaluation, we conducted only one heterogeneity analysis, by baseline antisocial behavior. We reprise this analysis using a continuous measure of baseline antisocial behavior in online Appendix B.3 and find that impacts are concentrated in the most violent and criminal young men at baseline. Again, results are larger and more significant for Therapy + Cash, but we cannot reject equivalence. To illustrate the degree of concentration, Figure 2 plots average treatment effects on antisocial behavior for two subgroups: those above and below the seventy-fifth percentile of baseline crime and violence. In this highest-quartile group, Therapy Only and Therapy + Cash reduce ten-year antisocial behavior by 0.910 and 1.25 standard deviations ($p < 0.01$), while effects in the lower three quartiles are close to zero.

Fourth, Therapy Only and Therapy + Cash seem to lead to large declines in several index components, especially thefts and robberies. If we interpolate between the one- and ten-year treatment effects, over ten years it implies that the average

Therapy + Cash participant committed 200 fewer crimes compared to a control group member.[7]

Impacts on these components are exploratory, not only because of the number of hypothesis tests but also because not all impacts are statistically significant. Nonetheless, to give a sense of what is likely to be driving the overall index, we summarize component treatment effects here, focusing on magnitudes.

- *Drug selling:* At baseline, 17 percent of the control group reported selling drugs often, falling to 14 percent after one year and 10 percent after ten. This decline is steeper in the therapy arms. After ten years, drug selling is 1.6 percentage points lower with Therapy Only (a −16 percent change relative to the ten-year control mean $p = 0.542$) and 4.6 points lower with Therapy + Cash (−46 percent, $p = 0.092$).

- *Thefts and robberies:* We asked men the number of times they committed a crime in the previous two weeks (eight kinds, from pickpocketing to armed robbery). In the control group, the total fell from 5.7 acts at baseline to 1.8 after a year and 1.9 after ten years. After ten years, men receiving Therapy Only reported one fewer crime each two weeks (−54 percent, $p = 0.017$) than the control group. Those receiving Therapy + Cash reduced crime by 0.87 (−49 percent, $p = 0.043$). These are conservative effects—if we do not top-code this variable at the ninety-ninth percentile, the control mean and treatment effects grow by a third (not shown).

- *Disputes and fights:* We asked about the frequency and severity of nine types altercations in the prior two weeks with peers, neighbors, leaders, and police. The control group reported a total of 2.21 physical fights at baseline, compared to 0.79 after one year and 0.80 after ten. After ten years, an index of these disputes fell by 0.14 standard deviations from Therapy Only ($p = 0.073$) and by 0.13 standard deviations with Therapy + Cash ($p = 0.051$).

- *Weapons:* At baseline, 8 percent of the control group said they carried a weapon on their body, increasing to 15 percent after a year and decreasing to 13.2 percent after ten. (Typically this was a knife, as guns are rare.) After ten years, weapon carrying was 7.5 percentage points lower with Therapy Only (−57 percent, $p = 0.024$) and 4.4 percentage points lower with Therapy + Cash (−33 percent, $p = 0.206$).

- *Arrests:* 14 percent of the control group reported an arrest in the two weeks before the one-month survey, 12 percent before the one-year survey, and 8 percent before the ten-year survey. After ten years, this was 1.3 percentage

[7] If we annualize the two-week theft and robbery figures, the therapy arms each result in about 25 fewer such crimes per year after ten years. These are similar to the one-month and one-year reductions of about 32 and 19 crimes per year (Blattman, Jamison, and Sheridan 2017). We do not have data for intervening years, but assuming the impacts were stable, this implies at least 200 fewer crimes per participant.

points lower with Therapy Only (-15 percent, $p = 0.609$) and 2.9 percentage points lower with Therapy + Cash (-36 percent, $p = 0.217$).

- *Aggressive behaviors:* We asked 19 questions about aggression, such as the frequency of yelling, cursing, bullying, cheating, or losing one's temper. After ten years, an index falls 0.061 standard deviations with Therapy Only ($p = 0.299$) and 0.062 with Therapy + Cash ($p = 0.316$).

- *Intimate partner abuse:* We have a crude measure of intimate partner abuse among those with a partner—three questions on verbal abuse and one on physical abuse in the past two weeks. An index shows almost no change from Therapy Only (0.032 standard deviation increase, $p = 0.771$) and a decrease of 0.082 standard deviations from Therapy + Cash ($p = 0.453$).

Mortality.—Mortality, including violent death, far exceeded our expectations, so it was not part of our prespecified outcomes. Nonetheless, it is an important potential long-term outcome, so we report mortality impacts at the base of Table 1.

We collected cause of death data from two friends or relatives of every member of the sample who died. The vast majority died due to illness or injury. About a quarter of deaths were violent, however, mainly due to mob killings of suspected thieves.

We see no evidence that either treatment decreased the risk of violent or nonviolent death. As discussed previously, there are slightly elevated levels of mortality in the Therapy Only group, although the difference is not significant in most specifications. Online Appendix B.4 elaborates on these data and analysis.

B. *Impacts on Secondary Outcomes and Potential Mechanisms*

The STYL program was designed to shape antisocial behaviors by fostering three main "noncognitive" traits: future orientation and planfulness, self-control skills, and the values associated with a mainstream social identity. Our previous paper developed a formal model of occupational choice between legal and illegal that illustrates how these three traits could shape economic activity and participation in violence—through time preferences, productivity, and the intrinsic utility placed on legal over illegal work.

At the same time, we recognized that therapy could also affect antisocial behavior through several other channels, including general mental health, substance abuse, social networks, and any indirect impacts that all these noncognitive traits have on economic performance and the opportunity cost of crime.

We measured each of these potential mechanisms with a variety of survey modules and grouped them into seven broad outcome families to reduce the number of hypotheses tested. Table 2 reports one- and ten-year treatment effects. Online Appendix B.5 describes index construction and component measurement in more detail and reports treatment effects on index components.[8]

[8] Note that with seven indexes, any p-value adjustment for multiple hypotheses would reduce statistical significance below conventional levels. Thus, we consider this analysis exploratory.

Before discussing the results, note that this study has neither the research design nor the statistical power to isolate mechanisms. Nonetheless, to be influential, any mechanism must be impacted by treatment. Moreover, since impacts on antisocial behavior hold steady over ten years (especially in the Therapy + Cash group), we should expect to see mechanisms persist as well. These criteria help to narrow down candidates.[9]

Broadly speaking, the results suggest that the therapy was effective at changing future orientation and (to a lesser extent) self-control skills in a durable way. We see no evidence of identity and value change, but we do see suggestive evidence of improved self-regard and mental health. All are strongly correlated with antisocial behaviors (see online Appendix B.5) and so are plausible mechanisms for the reduced crime and violence we observe.

Forward-Looking Time Preferences.—We constructed an index of four measures of patience and four of present bias, including incentivized gameplay. Therapy Only increases the index by 0.141 standard deviations after one year ($p = 0.137$) and 0.132 after ten years ($p = 0.168$), but these estimates are not statistically significant. Therapy + Cash is associated with larger, more significant increases: 0.199 standard deviations after one year ($p = 0.043$) and 0.247 after ten years ($p = 0.008$). Looking across components, point estimates are larger and more robust for patience than present bias.

Self-Control Skills.—We measured self-control using psychometric questionnaires for impulsiveness, conscientiousness, grit, and reward responsiveness. After a year, Therapy Only and Therapy + Cash were associated with increased self-control of 0.159 and 0.244 standard deviations ($p = 0.08$ and 0.011). After ten years, the size and significance diminish somewhat: Therapy Only and Therapy + Cash are associated with increased self-control of 0.178 and 0.119 standard deviations ($p = 0.086$ and 0.235).

Anticriminal Identity and Values.—Identity and values were among the more difficult traits to measure. Our index includes self-reported attitudes toward several acts of crime and violence; prosocial behaviors and group activities; and whether dress and appearance are consistent with mainstream social identity. We see weak evidence of long-run increases in these values. Therapy Only is associated with only a 0.038 standard deviation decrease ($p = 0.703$), while Therapy + Cash with a 0.100 standard deviation increase ($p = 0.310$).

Positive Self-Regard/Mental Health.—This family pools a wide-ranging set of six mental health outcomes: neuroticism, self-esteem, locus of control, subjective well-being, depression, and distress. Looking at the overall index, after ten years we see a small and not significant effect of Therapy Only (0.088 standard deviations, $p = .399$) and a larger and significant increase in mental health from Therapy + Cash (0.207 standard deviations, $p = 0.042$). The subjective well-being and self-esteem

[9] All secondary outcomes are strongly correlated with antisocial behavior (another prerequisite to be influential). See online Appendix B.5.

impacts are especially influential, although we see meaningful improvements in other components, such as symptoms of depression (see online Appendix B.5).

Substance Abuse.—Although it was not a focus of the therapy, NEPI nonetheless tried to equip participants with strategies to cut back substance abuse. We see weak evidence that the interventions reduced use of alcohol, marijuana, or hard drugs. An index declines 0.058 standard deviations with Therapy Only ($p = 0.543$) and 0.102 with Therapy + Cash ($p = 0.289$).

Quality of Social Networks.—We asked respondents several traits of their five closest friends, as well as closeness to family members, former rebel commanders, and criminal bosses. An index exhibits little change after one year. After ten years, there is little significant change from either Therapy Only (-0.026 standard deviations, $p = 0.814$) or Therapy + Cash (-0.085, $p = 0.407$).

Economic Performance.—Finally, as with the one-year results, we see no evidence that Cash Only affects economic performance (a 0.024 standard deviation increase, $p = 0.835$). There is, however, suggestive evidence that therapy increased long-run earnings and employment—Therapy Only is weakly associated with a 0.081 standard deviation increase in economic performance ($p = 0.483$) and Therapy + Cash with a larger and more significant 0.190 standard deviation improvement ($p = 0.090$). This could be a function of several of the above impacts—their future orientation and planning, their emotional regulation and self-control skills, and positive self-regard.

C. *Measurement Error Concerns*

An advantage of self-reported data is that we can measure outcomes and mechanisms that are not typically available. At the same time, they raise concerns of experimenter demand. We would be especially concerned if treated subjects were less likely to report antisocial behaviors.

Four factors mitigate these concerns somewhat:

(i) We do not observe treatment effects from the cash arm. Experimenter demand effects would have to be confined to the therapy arm.

(ii) Ten-year treatment effects are at least as large as one-year effects, implying that any experimenter demand effects would go undiminished with time.

(iii) We do not observe treatment effects on some of the outcomes most emphasized in the therapy sessions, including self-control skills, anticriminal values, and substance abuse. While impacts move in the expected direction, they are smaller and less statistically significant than many other outcomes. If men were simply repeating back their lessons, we might expect these treatment effects to be larger than average. Meanwhile, other measures, such as time preferences, are based on incentivized games where subjects are playing for actual money, and treatment effects persist in these outcomes.

Table 3—Assessing Systematic Measurement Error in One-Year Survey Outcomes

Covariate	$y^s - y^v$, sensitive behaviors (observations = 239)				
	All (0–4) (1)	Marijuana (2)	Gambling (3)	Homeless (4)	Stealing (5)
β_0 (Constant)	0.015	0.062	−0.109	0.093	−0.029
	[0.177]	[0.061]	[0.093]	[0.076]	[0.087]
β_1					
Therapy	−0.004	0.015	0.025	−0.025	−0.019
	[0.199]	[0.057]	[0.097]	[0.091]	[0.084]
Cash	−0.237	−0.042	−0.085	−0.077	−0.038
	[0.195]	[0.067]	[0.090]	[0.079]	[0.088]
Both	0.079	−0.024	0.077	0.031	−0.006
	[0.183]	[0.062]	[0.095]	[0.089]	[0.080]

Notes: We took four potentially sensitive and underreported measures and qualitatively validated the survey response through several days of close observation and trust building. We use the difference between the surveyed outcome (y^s) and the validated outcome (y^v) as a proxy for measurement error and regress it on treatment indicators and a constant. The coefficient on the constant indicates systematic over- or underreporting of the behaviors, and the coefficients on treatment indicate measurement error correlated with treatment. From Blattman, Jamison, and Sheridan (2017).

(iv) We attempted to validate a subset of questions using intensive qualitative observation and see no indications of systematic measurement error.

Specifically, during the one-year survey effort, we selected 7 percent of the 4,000 endline surveys for "qualitative validation" (Blattman et al. 2016). A Liberian qualitative researcher visited each of these respondents several times over several days shortly after the survey. Validators used a mix of in-depth participant observation, open-ended questioning, and efforts to build relationships to elicit more truthful answers. Over several days of trust-building and conversation, plus direct observation, the validator tried to elicit a direct admission or discussion of the behavior.

We used this approach to assess the answers to four potentially sensitive behaviors—marijuana use, thievery, gambling, and homelessness. Without knowing the respondent's survey response, y^s, each validator coded an indicator of whether or not the respondent engaged in the behaviors in the two weeks prior to the survey, y^v. Of the 297 men selected for validation, validators assessed 240 (81 percent). y^s and y^v were identical about 80 percent of the time. As expected, however, $\bar{y}^s$ was slightly lower than $\bar{y}^v$—the average respondent reported 1.21 sensitive behaviors in validation compared to 1.12 in the survey. Online Appendix B.6 elaborates.

We are mainly concerned with measurement error correlated with treatment. We use $y_i^s - y_i^v$ as a proxy of measurement and regress it on treatment:

$$y_i^s - y_i^v = \beta_0 + \beta_1 T_i + \mu_i, \tag{1}$$

where $\beta_0 < 0$ would indicate general underreporting of sensitive behaviors and $\beta_1 < 0$ would indicate that treated men are more likely to misreport.

Table 3 reports results. None of the coefficients on treatment are statistically significant. Rather, the patterns suggest that, if anything, the control group underreported sensitive behaviors such as stealing. If so, the treatment effects may actually underestimate therapy's impacts.

These results are consistent with our qualitative work, which suggested that the men are members of a subculture where drugs, crime, and gambling are commonplace and admitting to the behaviors in a survey carries little stigma.

IV. Discussion

Violence, theft, and drug selling plague cities worldwide. In fragile states, governments also confront election violence, rioting, and rebellion. CBT-informed programs have emerged as a promising way to tackle these problems. Low-cost, nonspecialist-led programs are especially attractive to low-income countries, which have limited police forces and prison systems.

Long-term evidence is essential, however. Short-term evidence from BAM and STYL helped inspire similar programs around the world.[10] It is unclear how long the impacts of psychotherapy last, however, especially therapy alone. CBT programs might still pass a cost-benefit test if impacts last only a year, due to the costly acts they temporarily deter. But this would be a disappointing conclusion for policymakers looking for alternatives to policing and incarceration.

Most of all, the ten-year results from Liberia show that simplified, nonexpert-led psychotherapy can have lasting impacts on violence and other antisocial behaviors. Results suggest slightly larger and more sustained impacts when therapy was combined with one-time economic assistance. Still, Therapy Only has strikingly large and robust long-run impacts, especially when we look at the highest-risk participants. The one-year estimated impacts of Therapy Only may have been noisy or not representative of longer-run impacts.

The results also suggest that there may be high returns to targeting the most violent and antisocial young men, as program impacts were concentrated in the most antisocial quarter of our sample at baseline.

We also see suggestive evidence that these changes in criminal and violent behavior come in part because the therapy helped to improve future orientation, skills of self-control, and mental health (especially self-regard). We see no evidence that the identity change component of STYL changed our measured attitudes or behaviors, but measuring social identity is challenging, and future research should innovate here.

Finally, the program appears to have been highly cost effective, alone and in combination with economic assistance. For instance, just looking at theft and robbery impacts alone, we estimate that every therapy participant resulted in at least 200 fewer crimes. We know of no figures on the social cost of crime in sub-Saharan Africa, but this implies a cost of less than $2.50 per crime averted given the $530 cost of both interventions—ignoring any of the other antisocial behaviors, from drug selling to aggression.

Given that Therapy + Cash was only modestly more impactful than Therapy Only, it is no longer so clear that implementers should include costly economic components. We need more testing of programs with and without economic components, as well as alternative economic interventions.

[10] See introduction and *The Economist* (2019); Bhatt et al. (2022).

To the extent that cash grants simply provided extra practice time and reinforcement, one alternative is to seek cheaper ways of reinforcing the lessons, such as "booster" therapy sessions some months after the intensive therapy. An alternative is to seek other low-cost means of economic assistance that lead to more sustained income growth. Mobile banking, gradual cash transfers, and the availability of credit and insurance products (to cope with shocks) are all promising.

Finally, these findings are significant not just because they address an important policy need but also because of what they imply about the malleability of adult preferences and behavior, the return to late-stage interventions, the durability of CBT-induced behavioral changes, and the important role of sustained practice. A literature shows that investment in childhood noncognitive skills improves long-run economic performance and criminal activity (Nagin and Pogarsky 2004; Heckman, Stixrud, and Urzua 2006; Borghans et al. 2008; Cunha, Heckman, and Schennach 2010). Absent any evidence, some scholars have been skeptical that noncognitive investments in adulthood can affect social and economic success (Heckman and Kautz 2014; Hill et al. 2011). The results of STYL suggest that adults engaged in the most socially harmful behaviors may be responsive to remedial investments and that these interventions could have huge social returns.

REFERENCES

Abt, Thomas. 2019. *Bleeding Out: The Devastating Consequences of Urban Violence—and a Bold New Plan for Peace in the Streets*. New York: Basic Books.

Abt, Thomas, and Christopher Winship. 2016. *What Works in Reducing Community Violence: A Meta-review and Field Study for the Northern Triangle*. Washington, DC: US Agency for International Development.

Akerlof, George A., and Rachel E. Kranton. 2000. "Economics and Identity." *Quarterly Journal of Economics* 115 (3): 715–53.

Almlund, Mathilde, Angela Lee Duckworth, James Heckman, and Tim Kautz. 2011. "Personality Psychology and Economics." In *Handbook of the Economics of Education*, Vol. 4, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, 1–181. Amsterdam: North-Holland.

Avitabile, Ciro, Janina Cuevas, Rafael de Hoyos, and Julian Jamison. 2022. *The Impact of School-Based Cognitive Behavioral Therapy on High School Dropout Rates in Mexico*. Cambridge, MA: J-PAL.

Baranov, Victoria, Sonia Bhalotra, Pietro Biroli, and Joanna Maselko. 2020. "Maternal Depression, Women's Empowerment, and Parental Investment: Evidence from a Randomized Controlled Trial." *American Economic Review* 110 (3): 824–59.

Barker, Nathan, Gharad Bryan, Dean Karlan, Angela Ofori-Atta, and Christopher Udry. 2022. "Cognitive Behavioral Therapy among Ghana's Rural Poor Is Effective Regardless of Baseline Mental Distress." *American Economic Review: Insights* 4 (4): 527–45.

Beck, Aaron T. 1979. *Cognitive Therapy and the Emotional Disorders*. New York: Penguin Group.

Beck, Judith S. 2011. *Cognitive Behavior Therapy: Basics and Beyond*. New York: Guilford Press.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169–217.

Bhat, Bhargav, Jonathan de Quidt, Johannes Haushofer, Vikram H. Patel, Gautam Rao, Frank Schilbach, and Pierre-Luc P. Vautrey. 2022. "The Long-Run Effects of Psychotherapy on Depression, Beliefs, and Economic Outcomes." NBER Working Paper 30011.

Bhatt, Monica P., Sara B. Heller, Max Kapustin, Marianne Bertrand, and Christopher Blattman. 2023. "Predicting and Preventing Gun Violence: An Experimental Evaluation of READI Chicago." NBER Working Paper 30852.

Blattman, Christopher, and Jeannie Annan. 2016. "Can Employment Reduce Lawlessness and Rebellion? A Field Experiment with High-Risk Men in a Fragile State." *American Political Science Review* 110 (1): 1–17.

Blattman, Christopher, Sebastian Chaskel, Julian C. Jamison, and Margaret Sheridan. 2020. "The Long Run Effects of a Program Aimed at Reducing Crime and Violence: Experimental Evidence from Cognitive Behavioral Therapy in Liberia." AEA RCT Registry. November 19. https://doi.org/10.1257/rct.6736-1.1.

Blattman, Christopher, Sebastian Chaskel, Julian C. Jamison, and Margaret Sheridan. 2023. "Replication data for: Cognitive Behavioral Therapy Reduces Crime and Violence over Ten Years: Experimental Evidence." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/E184306V1.

Blattman, Christopher, Nathan Fiala, and Sebastian Martinez. 2020. "The Long-Term Impacts of Grants on Poverty: Nine-Year Evidence from Uganda's Youth Opportunities Program." *American Economic Review: Insights* 2 (3): 287–304.

Blattman, Christopher, Julian Jamison, Tricia Koroknay-Palicz, Katherine Rodrigues, and Margaret Sheridan. 2016. "Measuring the Measurement Error: A Method to Qualitatively Validate Survey Data." *Journal of Development Economics* 120: 99–112.

Blattman, Christopher, Julian C. Jamison, and Margaret Sheridan. 2017. "Reducing Crime and Violence: Experimental Evidence from Cognitive Behavioral Therapy in Liberia." *American Economic Review* 107 (4): 1165–206.

Blattman, Christopher, and Laura Ralston. 2015. "Generating Employment in Poor and Fragile States: Evidence from Labor Market and Entrepreneurship Programs." Unpublished.

Borghans, Lex, Angela Lee Duckworth, James J. Heckman, and Bas ter Weel. 2008. "The Economics and Psychology of Personality Traits." *Journal of Human Resources* 43 (4): 972–1059.

Chioda, Laura. 2017. *Stop the Violence in Latin America: A Look at Prevention from Cradle to Adulthood*. Washington, DC: World Bank.

Clark, Patrick. 2010. "Preventing Future Crime with Cognitive Behavioral Therapy." *National Institute of Justice Journal* (265). https://nij.ojp.gov/topics/articles/preventing-future-crime-cognitive-behavioral-therapy.

Coopersmith, Stanley. 1967. *The Antecedents of Self-Esteem*. San Francisco: W. H. Freeman & Company.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach. 2010. "Estimating the Technology of Cognitive and Noncognitive Skill Formation." *Econometrica* 78 (3): 883–931.

Del Vecchio, Tamara, and K. Daniel O'Leary. 2004. "Effectiveness of Anger Treatments for Specific Anger Problems: A Meta-analytic Review." *Clinical Psychology Review* 24 (1): 15–34.

Dinarte, Lelys, and Pablo Egana-delSol. 2019. "Preventing Violence in the Most Violent Contexts: Behavioral and Neurophysiological Evidence." World Bank Policy Research Working Paper 8862.

Duckworth, Angela Lee, and Patrick D. Quinn. 2009. "Development and Validation of the Short Grit Scale (Grit-S)." *Journal of Personality Assessment* 91 (2): 166–74.

Feucht, Thomas, and Tammy Holt. 2016. "Does Cognitive Behavioral Therapy Work in Criminal Justice? A New Analysis from CrimeSolutions." *National Institute of Justice Journal* (277). https://nij.ojp.gov/topics/articles/does-cognitive-behavioral-therapy-work-criminal-justice-new-analysis-crimesolutions.

Haushofer, Johannes, and Jeremy Shapiro. 2018. "The Long-Term Impact of Unconditional Cash Transfers: Experimental Evidence from Kenya." Unpublished.

Heckman, James J., and Tim Kautz. 2014. "Fostering and Measuring Skills: Interventions That Improve Character and Cognition." In *The Myth of Achievement Tests: The GED and the Role of Character in American Life*, edited by James J. Heckman, John Eric Humphries, and Tim Kautz, 341–430. Chicago: University of Chicago Press.

Heckman, James J., Jora Stixrud, and Sergio Urzua. 2006. "The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior." *Journal of Labor Economics* 24 (3): 411–82.

Heller, Sara B., Anuj K. Shah, Jonathan Guryan, Jens Ludwig, Sendhil Mullainathan, and Harold A. Pollack. 2017. "Thinking, Fast and Slow? Some Field Experiments to Reduce Crime and Dropout in Chicago." *Quarterly Journal of Economics* 132 (1): 1–54.

Hill, Patrick L., Brent W. Roberts, Jeffrey T. Grogger, Jonathan Guryan, and Karen Sixkiller. 2011. "Decreasing Delinquency, Criminal Behavior, and Recidivism by Intervening on Psychological Factors Other than Cognitive Ability: A Review of the Intervention Literature." NBER Working Paper 16698.

Hoffman, Danny. 2011. *The War Machines: Young Men and Violence in Sierra Leone and Liberia*. Durham, NC: Duke University Press.

Holt, Charles A., and Susan K. Laury. 2002. "Risk Aversion and Incentive Effects." *American Economic Review* 92 (5): 1644–55.

Johnsen, Tom J., and Oddgeir Friborg. 2015. "The Effects of Cognitive Behavioral Therapy as an Anti-depressive Treatment Is Falling: A Meta-analysis." *Psychological Bulletin* 141 (4): 747–68.

Judge, Timothy A., and Joyce E. Bono. 2001. "Relationship of Core Self-Evaluations Traits—Self-Esteem, Generalized Self-Efficacy, Locus of Control, and Emotional Stability—with Job Satisfaction and Job Performance: A Meta-analysis." *Journal of Applied Psychology* 86 (1): 80–92.

Lipsey, Mark W., Nana A. Landenberger, and Sandra J. Wilson. 2007. "Effects of Cognitive-Behavioral Programs for Criminal Offenders." *Campbell Systematic Reviews* 3 (1): 1–27.

Lochman, John E. 1992. "Cognitive-Behavioral Intervention with Aggressive Boys: Three-Year Follow-Up and Preventive Effects." *Journal of Consulting and Clinical Psychology* 60 (3): 426–32.

Maruna, Shadd, and Kevin Roy. 2007. "Amputation or Reconstruction? Notes on the Concept of 'Knifing Off' and Desistance from Crime." *Journal of Contemporary Criminal Justice* 23 (1): 104–24.

McCrae, Robert R., and Paul T. Costa, Jr. 1997. "Personality Trait Structure as a Human Universal." *American Psychologist* 52 (5): 509–16.

Nagin, Daniel S., and Greg Pogarsky. 2004. "Time and Punishment: Delayed Consequences and Criminal Behavior." *Journal of Quantitative Criminology* 20 (4): 295–317.

Pearson, Frank S., Douglas S. Lipton, Charles M. Cleland, and Dorline S. Yee. 2002. "The Effects of Behavioral/Cognitive-Behavioral Programs on Recidivism." *Crime & Delinquency* 48 (3): 476–96.

Robinson, Terry E., and Kent C. Berridge. 2000. "The Psychology and Neurobiology of Addiction: An Incentive-Sensitization View." *Addiction* 95 (S2): S91–117.

Saini, Michael. 2009. "A Meta-analysis of the Psychological Treatment of Anger: Developing Guidelines for Evidence-Based Practice." *Journal of the American Academy of Psychiatry and the Law* 37 (4): 473–88.

Sapp, Stephen G., and Wendy J. Harrod. 1993. "Reliability and Validity of a Brief Version of Levenson's Locus of Control Scale." *Psychological Reports* 72 (2): 539–50.

Shayo, Moses. 2020. "Social Identity and Economic Policy." *Annual Review of Economics* 12: 355–89.

Singla, Daisy R., Brandon A. Kohrt, Laura K. Murray, Arpita Anand, Bruce F. Chorpita, and Vikram Patel. 2017. "Psychological Treatments for the World: Lessons from Low- and Middle-Income Countries." *Annual Review of Clinical Psychology* 13: 149–81.

The Economist. 2019. "Applying Lessons from War-Torn Africa to Chicago." May 4. https://www.economist.com/united-states/2019/05/04/applying-lessons-from-war-torn-africa-to-chicago.

Vigil, James Diego. 2003. "Urban Violence and Street Gangs." *Annual Review of Anthropology* 32: 225–42.

Wilson, David B., Leana Allen Bouffard, and Doris L. Mackenzie. 2005. "A Quantitative Review of Structured, Group-Oriented, Cognitive-Behavioral Programs for Offenders." *Criminal Justice and Behavior* 32 (2): 172–204.

Wood, Elisabeth Jean. 2008. "The Social Processes of Civil War: The Wartime Transformation of Social Networks." *Annual Review of Political Science* 11: 539–61.