COGNITIVE BEHAVIOR THERAPY REDUCES CRIME AND VIOLENCE OVER 10 YEARS:
EXPERIMENTAL EVIDENCE

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ABSTRACT

In most societies, a small number of people commit the most serious violence. Short-term studies have shown that cognitive behavioral therapy (CBT) can reduce such antisocial behaviors. These behavior changes may be temporary, however, especially from therapy on its own. This is unsettled, however, for there has been little randomized, long-term research. We follow 999 high-risk men in Liberia 10 years after randomization into either: 8 weeks of a therapy; a $200 grant; both; or a control group. A decade later, both therapy alone and therapy with economic assistance produce dramatic reductions in antisocial behaviors. Drug-selling and participation in thefts and robberies, for example, fall by about half. These impacts are greatest among the highest-risk men. The effects of therapy alone, however, are smaller and more fragile. The effects of therapy plus economic assistance are more sustained and precise. Since the cash did not increase earnings for more than a few months, we hypothesize that the grant, and the brief legitimate business activity, reinforced the habit formation embodied in CBT. Overall, results suggest that targeted CBT plus economic assistance is an inexpensive and effective way to prevent violence, especially when policymakers are searching for alternatives to aggressive policing and incarceration.

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1 Introduction

Urban crime and violence is one of the most costly and divisive issues facing cities around the world. A wide range of antisocial behaviors plague neighborhoods, from homicides to theft, street fighting, and drug selling. While crime in rich countries receives significant attention, the problem of antisocial behavior is also acute in other parts of the world, where there are added risks of election violence, rioting, and rebellion.

Whether they are concerned with criminal justice, ethnic tensions, or post-conflict peace-building, policymakers are desperately searching for preventative measures, especially for alternatives to coercive tools (such as aggressive policing, punishment, and imprisonment). This search is especially intense in the United States, but it is also true in poor and fragile states that do not have the resources to imprison offenders, and where the aggressive pursuit of high-risk men could be politically destabilizing.

Increasingly, policy-makers are turning to cognitive behavioral therapy (CBT) to tackle these social problems. In the United States, for instance, CBT-informed programs are quickly becoming one of the principal direct non-police responses to gun violence.[1] This approach springs from the idea that much crime and violence is the product of poor decision-making and distorted thinking: people may react in haste, fail to consider the long run consequences of their actions, or overlook alternative solutions to their problems. They may hold on to exaggerated, negative beliefs about a rival. Or they may have difficulty managing their emotions or impulses. Programs informed by CBT have long tried to help people become aware of these harmful thoughts and patterns, and learn to think and react differently.[2]

More recently, two large-scale randomized trials suggested that the full effects of CBT alone may be short-lived. In one, Heller et al. (2017) examined the 1–2 year effects of an in-school program, *Becoming A Man* (BAM), with nearly 5,000 at-risk high school students in Chicago. They found that criminal arrests fell by about half during the program period,

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[1]Clark (2010); Feucht and Holt (2016); Abt (2019)

[2]e.g. see meta-analyses by Lipsey et al. (2007); Wilson et al. (2005)

but also that the effects dissipated shortly afterwards.

Around the same time, Blattman et al. (2017) studied the 1-year effects of an intensive 8-week CBT-informed intervention in West Africa, the *Sustainable Transformation of Youth in Liberia* (STYL) program, with nearly 1,000 street youth and criminally-involved men. They too found that a wide range of criminal and violent behaviors fell by about half in the month immediately after the program. Like BAM, the effects of therapy alone diminished after a year. A quarter of the STYL sample, however, received a $200 cash grant in addition to therapy. Even after one year, the men who received therapy plus cash had reduced their crime and violence by about half. The grants did not affect incomes in a sustained way, however, suggesting that the cash enabled the young men to continue to practice the lessons of the CBT for months after the program ended, helping to entrench the changes in behavior.

This evidence has inspired similar programs around the world, targeting the highest-risk offenders with a combination of CBT and employment.[3] However, many questions remain unanswered. While meta-analyses consistently find that CBT-informed programs reduce criminal recidivism, most of the underlying studies have a small number of subjects who are typically only followed for about a year, especially among experimental studies (Lipsey et al., 2007; Wilson et al., 2005). Thus, the long term effects of these interventions on violence are unknown—especially whether a combination of CBT and economic assistance leads to more sustained behavior change.

Furthermore, most existing studies of CBT and crime focus on offenders in developed countries, mainly the United States, a population with significant advantages relative to those in low-income and unstable states. There is a need for larger, longer-term studies on more diverse populations, with attention to a wider range of behaviors.

In this study, we return to the STYL sample in Liberia roughly 10 years after subjects completed the program. At the time men entered the program, between 2009 and 2011, they were mainly in their twenties. Most were engaged in some form of crime or violence,

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[3]e.g The Economist (2019)

ranging from street fighting to drug dealing, petty theft, and armed robbery. We investigate whether these antisocial behaviors continued into later adulthood, and whether the short run impacts of therapy or therapy plus cash persist.

STYL was designed by a small local nonprofit, the Network for Empowerment and Progressive Initiatives (NEPI), in cooperation with the authors. Developed over 15 years through trial and error, the 8-week group therapy 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 their daily activities. Second, the program strongly emphasized how to peacefully deal with anger, interpersonal violence, and threatening situations. Finally, the therapy tried to help men learn to behave and self-identify as normal society members rather than as an outcast or criminal.

Following the therapy, all subjects were eligible for a cash lottery of $200. Winners were free to use the grant how they saw fit, but we expected the grant to relieve constraints on investment, and help the men start small business activities, such as petty trading or services such as shoe shining.

We evaluated the therapy and cash grants using a 2×2 factorial design, producing four arms: Therapy Only; Cash Only; Therapy+Cash; and a Control condition. The average cost of delivering both therapy and cash was $530 per person, inclusive of all implementation costs: $189 for delivering the 8-week STYL program; $216 for the grant and associated distribution; and $125 for program administration.

Ten years after the interventions, we found and resurveyed the original sample, collecting self-reported data on a range of antisocial behaviors, as well as data on potential mechanisms and choices in incentives games. Of the 999 men, 103 had died in the intervening years. We successfully surveyed 833 of the original sample—93% of survivors.

We are reliant on survey data because, as one of the poorest countries in the world, Liberia has no systematic administrative data on crime or imprisonment. There are several

advantages of survey and behavioral game data over arrest data, however. The outcomes are more complete, and reporting is not mediated by the capacity or biases of the criminal justice system. Furthermore, survey and game data allows us to collect direct evidence on mechanisms. Few studies have measured lifestyle, skill, and time preference changes directly. Self-reported data have limitations, of course, and so we took several steps to validate responses and minimize the risk of response bias correlated with treatment. Our analysis suggest that our results are unlikely to be the product of unfaithful responses or measurement error.

What should we expect after 10 years? Experts were pessimistic. To assess the profession's prior beliefs, we sent an anonymous survey to 88 scholars who had cited the 1-year results, and 30 responded. Almost all respondents expected Therapy Only or Cash Only to have no effect whatsoever on antisocial behaviors after 10 years. For Therapy+Cash, a third predicted no effect at all. Two-thirds predicted steeply diminished impacts, for an average prediction of about one third the 1-year impact. Based on the apparent transience of Therapy Only after one year, we too hypothesized that the impacts of the interventions would diminish or even disappear over time.

After a decade, however, we find large, sustained impacts of Therapy+Cash. Therapy Only shows some evidence of impacts on changed behavior but, as before, these effects are smaller and more fragile.

Our main outcome, as before, is a standardized index of antisocial behaviors that includes self-reported drug selling, stealing, interpersonal fighting, weapons carrying, arrests, hostile attitudes, and domestic abuse. In the first year after treatment, this index declined in every arm, including the control group, implying a degree of mean reversion among our sample—to be expected, since we targeted men in the riskiest situations. But for the 10 years thereafter, antisocial behavior remained steady and reasonably high, suggesting that men did not age out of these criminal and violent activities over time, at least on average.

Comparing across treatment arms, these declines in crime and violence were steepest in

both CBT arms, and these impacts persisted over time. In fact, the long-term improvements in both therapy arms are extremely similar to their 1-month and 1-year impacts. We see no effect of Cash Only after 1 or 10 years. But Therapy+Cash leads to a 0.25 standard deviation decrease in antisocial behaviors after 10 years, significant at the 5 percent level. Therapy Only leads to a 0.2 standard deviation decrease, significant at the 10 percent level. In concrete terms, this implies more than 50% reductions in self-reported drug selling, thefts, and robberies.

The 10-year Therapy+Cash results are highly robust to various selective attrition scenarios, along with other sensitivity analysis. The 10-year impacts of Therapy Only, however, are somewhat fragile to specification and selective attrition scenarios. This is consistent with our earlier findings: cash enabled the men to start legitimate enterprises for a few months and avoid an immediate return to crime, before those businesses ultimately failed. CBT is fundamentally about practice, learning-by-doing, and habit formation. A plausible hypothesis is that cash enabled a few months of legitimate activity after the CBT ended, helping to cement men's new non-criminal identity and behavior change.

To give a crude sense of magnitude and cost-effectiveness, our estimates suggest that Therapy+Cash led to roughly 34 fewer thefts and robberies per year per subject at both the 1- and 10-year surveys. Interpolating would mean 338 fewer crimes per subject since STYL began. Given the per-person program expenses, this implies a cost of roughly $1.50 per crime avoided—ignoring any continued reductions in crime, any reduced drug sales or street violence, improved political stability, or any other positive behavioral changes or spillovers.

What's more, we show how these reductions appear to be concentrated in the men who reported the highest antisocial behaviors at baseline. This was the sole heterogeneity analysis we prespecified and performed. Thus, even though STYL was already a highly selective and targeted intervention, this suggests that there may be returns to even more selective targeting among such programs in the future.

Finally, beyond crime and violence, we also see sustained impacts on a range of (ex-

ploratory) secondary outcomes and potential mechanisms: men in the Therapy+Cash arm are less poor, more patient in game play, report improved mental health, and have less risky social networks. We do not see any evidence, however, that they are less impulsive, abuse fewer substances, or have acquired more anticriminal norms and values as a result of the therapy.[4]

Altogether, these findings are significant not just because they address an important policy need, but also because of what they tell us generally 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 (and the economic means to do live a noncriminal life). A large literature has shown that a broad set of such noncognitive skills predict long-run economic performance and criminal activity (Nagin and Pogarsky, 2004; Heckman et al., 2006; Borghans et al., 2008). This literature emphasizes how these skills respond to investment by families, schools, and communities, especially in childhood (Cunha et al., 2010). It is an open question, however, whether these skills can be easily shaped in adulthood. Absent any evidence, some scholars have been skeptical that self-investment or interventions can shape noncognitive skills and behavior in adulthood (Heckman and Kautz, 2014; Hill et al., 2011). The findings from STYL suggest that adults engaged in the most socially harmful behavior may be quite responsive to remedial investments, and that these interventions could have huge social returns.

Section 2 describes the intervention and experimental procedures, Section 3 the data and attrition, Section 4 the results, and Section 5 concludes.

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[4]The results are consistent with evidence from stigmatized Indian sex workers, where short courses of non-CBT psychological therapy increased self-worth, reduced shame, and increased savings and health-seeking behavior (Ghosal et al., 2020).

2 Context, intervention, and experiment

2.1 Liberia

Liberia is a small West African coastal nation with a current population of about 5 million. The country emerged from 15 years of civil wars and political instability in 2003. Those wars killed roughly 10% of the population, displaced a majority, and recruited tens of thousands into combat. By the outset of the study period, in 2009, the country had enjoyed a fragile order for 6 years. Nonetheless, by this date, most people aged 18–35 would have spent up to 15 years of their childhood or adolescence in an environment of conflict.

Among the many threats to stability in the country at the time, the government and a large United Nations peacekeeping mission were particularly concerned with poorly integrated ex-fighters and other young men involved in violent crime and the growing retail drug trade. They also worried about political violence, as high-risk men had launched riots and were involved in election violence in the past, and they were targets for mercenary recruitment into West African wars (Christensen and Utas, 2008; Hoffman, 2011; Blattman and Annan, 2016).

The largest concentration of these high-risk men was in the country's large seaside capital, Monrovia. Most young men in the city had limited employment opportunities and earned money through a mix of casual labor and petty business. A few turned to crime, especially drug dealing, pick-pocketing, mugging, and home invasion. Street violence was also common among this group, especially brawls and knifings. Importantly, however, these men were not necessarily ex-fighters from the war. 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 careers of crime and social relationships governed by violence.

2.2 Target population and recruitment

The study recruited 999 men actively involved in crime and interpersonal violence. On average these men were 25 years of age, had nearly eight years of schooling, earned about $68 in the month previous to the baseline survey, worked an average of 46 hours per week (mainly in low skill labor and illicit work), and had $34 informally saved. Only 38% were former members of armed militant groups.

We focused on five mixed-income residential neighborhoods in Monrovia with large populations of high-risk men. All recruitment was handled by NEPI, the nonprofit that designed and ran the therapy. NEPI recruited subjects on the street. In each neighborhood, certain places, groups, and professions had well-known reputations for crime and violence involvement, and recruiters targeted these locations and people. This included known drug selling areas, areas for the fencing and resale of stolen goods, informal settlements with high levels of interpersonal street violence, and so forth. Recruiters also approached men that were homeless, drug-using, or appeared disreputable in appearance.

When they approached a new subject, recruiters described the purpose of the program (transition to a less violent, noncriminal lifestyle), the therapy, the allocation by lottery, and the associated research, and asked subjects if they wanted to participate. Recruiters never mentioned cash grants.

About one third of those approached declined. Some refused because they felt they were the wrong target—poor but not engaged in criminal or violent activities. Others were mistrustful or were content with their current illicit activities. We were unable to collect survey data on or track these individuals that refused any engagement.

Note that these procedures also tried to minimize the possibility of spillovers between treatment and control subjects. To do so, we designed recruitment to be highly dispersed. Each of the five neighborhoods had a population of roughly 100,000, including at least a thousand high-risk young men. To maximize independence of the study subjects (and reduce potential for spillovers) recruiters approached just one out of every 7–10 high-risk men they

visually identified. Over several weeks, recruiters identified roughly 8,000 potentially high-risk men and approached 1,500. In our five neighborhoods, we estimate that our sample of 999 men represents 0.6% of all adult males and about 12% of all men aged 18–35 who are in the bottom decile of income.

When a subject agreed to participate in the study, NEPI immediately introduced them to a representative of a nonprofit research organization, Innovations for Poverty Action (IPA), for a baseline survey and random assignment to therapy. A week after therapy finished, NEPI recontacted all subjects and told them about a new opportunity: a public draw for cash grants.

2.3 Interventions and hypothesized effects

Therapy   Cognitive behavioral therapy is an approach for reducing self-destructive beliefs and behaviors, and promoting positive ones. Its methods can be applied to a wide range of problematic thoughts and behaviors, and CBT-informed therapies have been widely successful at reducing symptoms of depression, anxiety, phobias, traumatic stress, and hostility (Beck, 1979, 2011).

Most CBT-informed therapies have a few common elements. First, the counselor tries to help the patient become more conscious of their harmful automatic thoughts, especially inaccurate or negative thinking about themselves or others. They help subjects to recognize and respond differently to these automatic thoughts, allowing them to respond to everyday situations in a more constructive way. A second principle of CBT is that sustained changes in behavior or symptoms also come from actively practicing new behaviors. Thus, thoughts influence actions but actions also shape thoughts—a kind of "learning by doing". Often subjects begin practicing simple tasks and, through repetition, positive reinforcement, and gradually increasing the difficulty or complexity of the tasks, they gradually change both their behavior and thinking. This practice happens in therapy but also as "homework".

NEPI had worked with this population and in these neighborhoods for more than a

decade. STYL's specific activities and approach grew organically over the years, primarily through NEPI's trial and error. Most of the specific stories and activities are NEPI's own invention. Importantly, over the years NEPI acquired, tested, and adapted CBT techniques from Western manuals as well as formal and informal training from international organizations.

NEPI's program combined group therapy with one-on-one counseling over 8 weeks. Twenty men met in groups three times a week, four hours at a time, led by two NEPI facilitators. NEPI offered no incentives to attend except for lunch. On alternate days when groups did not meet, facilitators visited men at home or work to provide advising and encouragement.

The sessions employed a mix of lectures, group discussions, and practice, including: role playing in class, homework that requires practicing tasks, exposure to real situations, and in-class processing of experiences of executing these tasks. Like many CBT-informed programs, these tasks increased in difficulty over time.

There were virtually no formally-trained psychologists or counselors in the country of Liberia at the time, and so all STYL sessions were led by facilitators trained by NEPI. These facilitators had typically been involved in armed groups or crime earlier in their lives, and most were past graduates of a past NEPI rehabilitation program. Generally speaking, they had limited formal education or counseling experience. Their experiences, however, made them natural role models for the students, as they modelled the desired changes in behavior and identity.

The curriculum focused on three related kinds of behavior change.[5] First, STYL tried to help men learn to behave and self-identify as normal society members rather than as an outcast or criminal. For instance, in the early weeks, facilitators encouraged men to try to maintain some simple behaviors associated with their new social identity. This included getting a haircut, wearing shoes and pants instead of sandals and shorts, improving per-

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[5]The STYL manual is available online, including day-by-day breakdowns of the curriculum and homework. See http://chrisblattman.com/documents/policy/2015.STYL.Program.Manual.pdf

sonal hygiene and the cleanliness of their living area, and reducing substance abuse. As the men progressed, facilitators also encouraged men to engage with society in planned and unaccustomed ways, akin to exposure therapy. For instance, homework included reintroducing themselves to their family, joining community sports, and visiting banks, supermarkets, shops, and so forth. Men also studied successful people in their community and reached out to one as a mentor. Men then discussed their successes and failures as a group.

Second, 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 their daily activities. For example, the men practiced breaking down large goals into smaller ones, and then creating plans to accomplish them via concrete steps. For homework they would attempt some of these plans. Examples include how to feed their family the next day, starting a vegetable garden, making a savings plan, reconciling with estranged family, or starting a business. They began with easier assignments and increased in difficulty with time.

Finally, the program strongly emphasized how to deal with anger, interpersonal violence, and threatening situations. During the group session, the men discussed angry and hostile thoughts, emotions, and reactions. They learned to recognize and connect them to bad outcomes, like violence and exclusion. They also began to practice techniques to manage these automatic responses. For instance, they practiced social skills for managing threatening situations, and learned techniques to calm oneself, such as walking away, doing other activities, or breathing techniques.

While CBT-informed techniques had not been tested on such a high-risk population before, a wide body of evidence suggests these techniques reduce problematic behaviors in lower risk populations.[6] Accordingly, prior to the 10-year survey, we preregistered one primary outcome: a summary index of various antisocial behaviors, described below. The therapy could influence these antisocial behaviors through a variety of channels, however, and

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[6]e.g. Saini (2009); Pearson et al. (2002); Wilson et al. (2005); Del Vecchio and O'Leary (2004)

so we measured and prespecified 7 secondary outcomes to explore as mechanisms: economic performance; forward-looking time preferences; skills of self control; anticriminal identity and values; positive self-regard and mental health; substance abuse; and the quality of social networks.[7]

Cash grants Winners of the cash lottery immediately received $200 in cash, while losers received $10 as a consolation. This grant had minimal framing. Prior to the draw, the assembled subjects were given about 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 or home improvement). But subjects were explicitly told that the grant was unconditional and they were free to do what they wished.

The cash arm was intended to stimulate legal self-employment and earnings, by relieving a liquidity or credit constraint. There is broad-based evidence across Africa that unemployed youths have high short-term returns to capital (Haushofer and Shapiro, 2018; Blattman et al., 2020). This evidence also suggests that the effects on earnings may be short-lived.

Consistent with these other studies, one year after treatment we found that there was no direct effect of cash on earnings or consumption (Blattman et al., 2017). Employment and incomes rose in the first month following the cash grant, as the men started small legal enterprises (such as market trading or shoe-shining). After a year, however, these impacts had dissipated, and those who received the grant had similar employment and incomes as the control group. Our qualitative and quantitative investigations 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.

As a result, we did not specify economic performance as a primary outcome prior to running the 10-year survey. Nonetheless, even a short term increase in legal earnings could reduce criminality by increasing the returns to legal enterprises and raising the opportunity cost of antisocial behavior (Becker, 1968; Blattman and Ralston, 2015). Furthermore,

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[7]See Social Science Registry *AEARCTR-0006736*.

conceivably the grant could also reduce antisocial behaviors in the long run by positively reinforcing the therapy's push towards a more mainstream, legal lifestyle, and providing increased opportunities to practice these behaviors in the months following therapy. Hence we hypothesized that the effects of Therapy+Cash would be more durable than those of Therapy Only, even if the effects of both declined with time.

2.4 Randomization and balance

Initially, recruited subjects were randomly assigned to therapy via a draw, where they drew colored chips from an opaque bag (with replacement). We did so partly for transparency and trust, and partly to eliminate the need to recontact these hard-to-find individuals post-randomization. Therapy began the week following randomization. Of those assigned to therapy, 95% attended at least the first week and two-thirds attended most sessions.

About one week after the final week of therapy, NEPI re-contacted all subjects and told them that another opportunity was available to them: a lottery for cash grants. Again, one by one, subjects selected colored chips from a bag. For safety, they drew their lot and received the funds in private, and were immediately transported to a location of their choice by motorbike taxi. For those who did not attend, a chip was drawn on their behalf, and NEPI attempted to track them down. Of those assigned to a grant, 98% received one.[8]

For logistical reasons we conducted this experiment in three phases of 100, 398, and 501 subjects between December 2010 and March 2012. Over the 3 phases, the experiment resulted in 28% assignment to therapy only, 25% to cash only, 25% to both, and 22% to neither.[9] The excess therapy assignments is in part due to chance, and is in part driven by two blocks where excess treatment chips were accidentally used. All regressions include block fixed effects to account for this.

Assignment to the four arms was largely balanced along covariates. Blattman et al.

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[8]The draws were conducted by an international nonprofit named Global Communities, in cooperation with NEPI.

[9]See Appendix Table A.1 for details.

(2017) reported tests of randomization balance for the full sample. That analysis found that, of 57 covariates over three treatments, 14 (8.2%) had a difference with $p < .05$, and within treatment arms the covariates were not jointly significant (see Appendix Table A.2).

Here, in Table 1, we reproduce these baseline summary statistics and tests of balance, but we limit the sample to the 833 subjects interviewed at the 10-year endline. We do so to confirm that there is no imbalance introduced by attrition. Column 1 reports the sample mean for each covariate, and Columns 2 to 7 report the coefficients and p values on treatment indicators from ordinary least squares (OLS) regressions of each baseline covariate on three treatment indicators (one for assignment to each treatment arm), controlling for block fixed effects. Column 8 reports the p-value from a joint test of significance of the three coefficients.

Overall, there is minor imbalance. Of 171 coefficients (57 covariates and 3 treatment arms), 12 (7%) have a $p < .05$, and other 7 (4.1%) have a $p < .1$. When we look at tests of joint significance across all arms, 7 of 57 covariates (12%) have a a $p < .1$, while 4 (7%) have a $p < .05$. We control for these baseline covariates in all treatment effects regressions in the paper to account for these modest differences.

2.5   Empirical strategy

We estimate intent-to-treat (ITT) effects via the ordinary least squares regression:

$$Y_{ij} = \tau_1 TherapyOnly_i + \tau_2 CashOnly_i + \tau_3 Therapy\&Cash_i + \mathbf{X}_i\lambda + \gamma_j + \varepsilon_{ij} \qquad (1)$$

where *TherapyOnly*, *CashOnly*, and *Therapy&Cash* are indicators for random assignment to the 3 treatment arms. We control for a vector of baseline characteristics, X, and fixed effects for each of the j randomization blocks, γ_j. Y_{ij} is the average outcome from the two proximate survey rounds. To reduce sensitivity to outliers, we top-code continuous variables at the 99th percentile. We estimate heteroskedastic robust standard errors.

To minimize the number of hypothesis tests, we collected survey variables into family

Table 1: Tests of randomization balance, 10-year surveyed sample only

	Sample Mean	Test of randomization balance(N=833)						F-Test
Baseline covariate		Assigned Therapy		Assigned Cash		Assigned Both		
		Coef.	p value	Coef.	p value	Coef.	p value	p value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	25.235	-0.136	0.788	0.310	0.540	-0.133	0.793	0.781
Married or partnered	0.149	-0.022	0.549	-0.038	0.300	-0.024	0.507	0.779
# of partners	0.531	0.057	0.412	0.131	0.058	-0.025	0.715	0.094
# of children<15 in household	2.212	-0.622	0.054	-0.768	**0.017**	-0.643	**0.047**	0.085
Sees family often	2.345	0.122	0.226	0.286	**0.005**	0.108	0.287	**0.040**
Muslim	0.080	0.001	0.965	0.006	0.838	0.018	0.529	0.910
Years of Schooling	7.892	0.011	0.974	0.272	0.411	-0.177	0.596	0.577
Currently in school	0.060	-0.018	0.473	-0.019	0.441	-0.010	0.679	0.862
Literacy index (0-2)	1.247	0.116	0.219	0.101	0.280	-0.043	0.648	0.215
Math score (0-5)	2.812	-0.069	0.676	0.055	0.738	-0.370	**0.025**	**0.038**
Health index (0-6)	4.852	-0.080	0.588	-0.191	0.194	0.023	0.877	0.435
Has any disabilities	0.068	0.033	0.210	-0.011	0.679	0.000	0.988	0.331
Depression index (0-17)	7.078	0.247	0.464	0.014	0.966	-0.028	0.934	0.813
Distress index (0-21)	7.414	-0.231	0.582	-0.202	0.629	-0.641	0.129	0.471
Relations to commanders index (0-4)	0.427	-0.014	0.864	0.085	0.289	0.047	0.562	0.562
Ex-combatant	0.372	0.068	0.178	0.075	0.141	0.090	0.078	0.313
War experiences index (0-12)	5.785	0.385	0.198	-0.017	0.956	-0.079	0.793	0.339
Weekly cash earnings (USD)	16.615	-4.747	**0.027**	-5.752	**0.007**	-3.687	0.087	**0.045**
Summary index of income, z-score	0.005	-0.196	0.064	-0.097	0.358	-0.107	0.312	0.328
Homeless in past two weeks	0.228	-0.019	0.665	0.020	0.643	-0.003	0.944	0.832
# of days slept hungry, last 7 days	1.248	0.220	0.126	0.328	**0.022**	0.159	0.269	0.141
Savings stock (USD)	32.661	-11.866	0.081	-13.101	0.054	-10.702	0.118	0.209
Can get loan of 50 USD	0.502	-0.033	0.524	-0.057	0.262	-0.037	0.469	0.733
Can get loan of 300 USD	0.103	-0.016	0.623	-0.002	0.949	0.006	0.854	0.906
Hours in illicit activities	13.185	-0.989	0.736	-1.252	0.669	0.168	0.955	0.946
Hours/week in agriculture	0.402	0.414	0.336	-0.152	0.723	0.337	0.436	0.487
Hours/week in low-skill wage labor	18.971	-1.299	0.659	-1.454	0.621	-0.031	0.992	0.928
Hours/week in low-skill business	12.229	0.930	0.711	-0.126	0.960	3.897	0.122	0.318
Hours/week in high-skill work	1.586	-0.081	0.923	0.653	0.434	0.989	0.239	0.472
Years of experience in agriculture	0.742	-0.023	0.913	-0.205	0.340	-0.217	0.316	0.606
Years experience in non-agricultural business	3.050	-0.175	0.704	-0.759	0.099	-0.338	0.465	0.379
Years experience in high-skill work	0.986	-0.422	0.135	-0.576	**0.041**	-0.037	0.895	0.093
Sells drugs	0.196	-0.009	0.827	-0.018	0.670	0.005	0.902	0.945
Drinks alcohol	0.760	0.073	0.106	0.054	0.226	0.047	0.300	0.426
Uses marijuana	0.582	0.104	**0.039**	0.077	0.124	0.037	0.466	0.166
Uses marijuana daily	0.431	0.057	0.254	0.029	0.565	0.006	0.909	0.627
Use hard drugs	0.242	-0.026	0.554	0.018	0.687	-0.013	0.779	0.769
Uses hard drugs daily	0.132	-0.074	**0.034**	0.030	0.389	-0.023	0.507	**0.018**
Committed theft/robbery in past 2 weeks	0.523	0.034	0.512	0.011	0.826	0.020	0.699	0.927
Number of nonviolent stealing incidents	4.823	-0.584	0.557	-0.812	0.413	-0.443	0.657	0.872
Number of felony stealing incidents	0.429	-0.111	0.616	0.050	0.819	0.009	0.968	0.886
Disputes and fights in past 2 weeks (0-9)	2.012	-0.291	0.511	0.210	0.636	-0.325	0.466	0.566
Aggressive behaviors (mean of 19), z-score	-0.017	-0.032	0.761	0.071	0.490	-0.163	0.117	0.128
Conscientiousness index (0-24)	15.369	-0.051	0.827	-0.255	0.280	-0.270	0.255	0.538
Neuroticism index (0-21)	12.006	-0.060	0.815	0.201	0.435	0.245	0.345	0.528
Grit index (0-21)	13.812	0.172	0.412	0.029	0.890	0.017	0.934	0.817
Reward responsiveness index (0-24)	14.675	-0.204	0.484	0.135	0.643	-0.384	0.189	0.284
Locus of control index (0-24)	14.465	-0.138	0.550	-0.456	**0.048**	-0.068	0.769	0.189
Impulsiveness index (0-21)	9.324	0.450	0.244	0.178	0.644	-0.288	0.459	0.225
Self esteem index (0-24)	13.541	-0.009	0.973	0.026	0.921	0.126	0.631	0.946
Patience in game play index (0-6)	4.185	0.060	0.768	-0.219	0.278	-0.154	0.450	0.440
Time inconsistency in game play index (0-6)	3.280	-0.211	**0.037**	-0.024	0.810	-0.119	0.240	0.134
Risk aversion index (0-3)	1.580	-0.050	0.679	-0.018	0.880	0.065	0.590	0.777
Self-reported patience (mean of 7), z-score	0.009	-0.013	0.903	-0.081	0.434	0.030	0.776	0.730
Declared Risk Appetite (mean of 6), z-score	-0.005	0.025	0.815	-0.012	0.909	-0.139	0.196	0.373
Cognitive ability (z-score)	0.046	0.095	0.356	0.135	0.187	-0.080	0.439	0.123
Executive function (z-score)	-0.002	0.067	0.526	0.078	0.458	-0.145	0.173	0.101

Notes: The table reports ordinary least squares regressions of each baseline covariate on treatment indicators, controlling for randomization block fixed effects. We limit the analysis to the 833 members of the sample interviewed at the 10-year endline. All p-values are heterosketastic-robust, with p<0.05 in bold.

indexes, such as the index of antisocial behaviors. Given that we specified only one primary outcome, we do not adjust p-values for multiple hypotheses.

There are two potential threats to causal identification. One is measurement error correlated with treatment, which we discuss below. The second is interference between units in dense urban networks. As we noted above, our recruiting strategy—working in large neighborhoods, recruiting less than 1% of adult men in those areas—was designed to minimize general equilibrium effects (such as a change in the local returns to crime). We also recruited 1 in every 7–10 subjects identified on the street to reduce the chances that subjects interact, causing treatment to spillover from treatment to control subjects (potentially leading us to understate therapy's impacts). We could not eliminate either risk, however and do not have social network data to assess equilibrium responses and potential spillovers.

3   Data and attrition

As one of the poorer and more fragile countries in the world, Liberia does not have administrative data on arrests, crimes, or other forms of violent behavior. Thus, we use self-reported outcomes from surveys to measure outcomes. The main advantage of this strategy is unusually rich data on outcomes, especially a wide range of antisocial behaviors and mechanisms. A potential drawback comes from concerns of bias in self-reported data. We address these concerns in Section 4.5 below.

We attempted to survey each subject 7 times: (i) at baseline; (ii & iii) 2 and 5 weeks after the grants; (iv & v) 12 and 13 months after grants, and roughly (vi & vii) 114 and 115 months after grants.[10] We ran pairs of surveys to reduce noise in outcomes with potentially low autocorrelation, such as earnings or criminal activity. We average these pairs into "1-month", "1-year", and "10-year" outcomes.

Each survey round was roughly 90 minutes long and delivered verbally by enumerators in

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[10]We conducted the 10-year survey round roughly 123 months after the completion of Phase 1 treatment, 117 months after Phase 2 treatment, and 109 months after Phase 3. The weighted average gives us a roughly 9.5-year follow-up, which we round to 10 years.

Liberian English on handheld tablets. To measure time and risk preferences the respondents also conducted 45 minutes of incentivized games and tests.

Subjects typically had no fixed address, often went by many aliases, or lived clandestinely. By collecting extensive social network and contact information, and through long and intensive tracking, we located most surviving respondents. Of the 999 members of the original sample, 103 died before this round of surveys. Of the remaining sample, 33 could not be surveyed: 7 were in prison at the time of the survey; 17 refused or were unable to respond; and 6 were unreachable because we were unable to find any information on their whereabouts (or survival). Therefore we have survey data on 833 (83%) of the original sample, including 93% of known survivors.

Response rates varied slightly by treatment group. Table 2 reports the correlates of attrition from a multivariate regression including indicators for each treatment arm and a selection of baseline covariates. Looking at all sources of attrition, in column 2, we see roughly 4 percentage point higher rates of attrition in Therapy Only arm due primarily to excess mortality (not statistically significant). We see 4 percentage point lower attrition in the Therapy and Cash arm (also not significant). This lower attrition was due to somewhat better success rate at finding survivors (also significant at the 10% level, in column 5). Below we will show how our treatment effects are robust to various selective attrition scenarios. Otherwise, attrition is not particularly systematic. Looking at Column 2 Table 2, for instance, few covariates have large or statistically significant effects on attrition.

4   Impacts on antisocial behaviors

Our family index of antisocial behaviors averages 7 measures of disruptive or harmful acts towards others: drug selling; stealing; interpersonal fighting; carrying a weapon; arrests; hostile attitudes; and domestic abuse. Figure 1 displays levels of this index and average treatment effects, adjusted to have zero mean at baseline and unit standard deviation over

Table 2: Attrition balance by treatment arm and baseline covariates

	Sample Mean (SD)	Did not survey Estimate (SE)	Deceased Estimate (SE)	Imprisoned Estimate (SE)	Did not survey (Excl. deceased & imprisoned) Estimate (SE)
	(1)	(2)	(3)	(4)	(5)
Therapy Only	0.280	0.044	0.040	0.003	0.001
	(0.449)	(0.035)	(0.029)	(0.008)	(0.022)
Cash Only	0.250	-0.017	0.018	-0.009	-0.027
	(0.433)	(0.035)	(0.029)	(0.008)	(0.022)
Therapy + Cash	0.249	-0.039	-0.004	0.002	-0.038*
	(0.433)	(0.036)	(0.030)	(0.008)	(0.022)
Age	25.400	0.005*	0.007***	0.000	-0.001
	(4.858)	(0.003)	(0.002)	(0.001)	(0.002)
Married or partnered, binary	0.158	0.011	0.011	-0.001	0.001
	(0.365)	(0.037)	(0.030)	(0.008)	(0.023)
# of children<15 in household	2.209	-0.002	0.003	-0.001	-0.004
	(3.174)	(0.004)	(0.003)	(0.001)	(0.003)
Years of schooling	7.719	-0.009*	-0.005	-0.001	-0.003
	(3.287)	(0.005)	(0.004)	(0.001)	(0.003)
Cognitive skill index, z-score	0.000	-0.006	-0.010	0.002	0.002
	(1.000)	(0.015)	(0.012)	(0.003)	(0.009)
Health index, z-score	-0.004	0.002	-0.003	0.002	0.003
	(1.002)	(0.012)	(0.010)	(0.003)	(0.008)
Depression and distress index, std.	0.000	-0.007	-0.008	0.001	-0.000
	(1.000)	(0.013)	(0.011)	(0.003)	(0.008)
War experiences index (0-12)	5.859	0.002	0.007*	0.000	-0.005*
	(2.873)	(0.005)	(0.004)	(0.001)	(0.003)
Index of economic success, z-score	-0.003	0.009	0.003	0.000	0.006
	(0.999)	(0.013)	(0.011)	(0.003)	(0.008)
Savings stock (USD)	33.753	0.000	0.000	0.000	0.000
	(67.388)	(0.000)	(0.000)	(0.000)	(0.000)
Hours/week in illicit activities	13.557	0.001	0.001**	0.000**	-0.001
	(27.253)	(0.001)	(0.000)	(0.000)	(0.000)
Hours/week working	45.974	-0.001*	-0.001**	-0.000	-0.000
	(43.231)	(0.000)	(0.000)	(0.000)	(0.000)
Index of antisocial behaviors, z-score	0.001	0.016	0.002	-0.001	0.014
	(1.003)	(0.016)	(0.013)	(0.004)	(0.010)
Index of personality measures, z-score	0.000	-0.004	-0.004	-0.002	0.003
	(0.949)	(0.014)	(0.012)	(0.003)	(0.009)
Index of patience, z-score	-0.004	-0.034***	-0.031***	-0.000	-0.003
	(1.002)	(0.012)	(0.010)	(0.003)	(0.008)
Dep. var. mean		0.166	0.103	0.007	0.056
N		999	999	999	999

Notes: This table regresses different attrition outcomes (not surveyed, deceased, imprisoned, adjusted not surveyed) on the different treatment arms and a subset of the baseline variables.

all survey rounds. Table 3 reports means and average treatment effects for the overall index and each component in the 10-year survey round only.[11]

The first thing to note from Figure 1, panel (a) is that antisocial behaviors fall steeply in all treatment arms in the first year after randomization, even in the control arm. Levels are relatively stable thereafter, on average. There are at least two potential reasons for this. One is mean reversion. If the program is successful at recruiting people at their most violent, criminal, or desperate point, we would expect a degree of regression to lower levels of antisocial behavior over time. The second is life cycle effects. In many countries, crime rates peak in the late teens and early twenties, and decrease thereafter. In this instance, however, the fact that we do not see a steady decline in antisocial behaviors between the 1- and 10-year surveys suggests that life cycle effects may be weak for this population, and the decline is due principally to mean reversion.

While crime and violence decrease across all arms, these declines are steepest and most sustained in the two therapy treatment arms. We can see these treatment effects in panels (b) to (d) of Figure 1, as well as in Table 3. After 10 years, Therapy Only reduces the index of all 7 antisocial behaviors by 0.20 standard deviations ($p = 0.055$), and Therapy+Cash reduces it by 0.25 standard deviations ($p = 0.016$). Although the Therapy+Cash estimate is larger and more robust, we cannot reject that it is equivalent to the effects of Therapy Only ($p = 0.61$). Cash Only is associated with a small and not statistically significant decrease in antisocial behaviors, which we can distinguish from the effect Therapy+Cash with some confidence ($p = 0.079$).

These 10-year impacts are remarkably similar to the 1-year impacts. In the Therapy+Cash arm, antisocial behaviors declined 0.308, 0.242, and 0.251 standard deviations after 1 month, 1 year, and 10 years. In the Therapy Only arm, they declined 0.249, 0.074,

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[11]With just one primary outcome, we do not adjust standard errors for multiple comparisons. Note that the 1-year study specified two primary outcomes: antisocial behaviors and economic performance (reflecting an interest in the effects of cash transfers alone). Since the cash grants showed no impact on poverty after one year, we focus on antisocial behaviors for the long run evaluation of Therapy Only and Therapy+Cash. Our results, if adjusted for two hypotheses, would not substantively change our conclusions.

Figure 1: Program impacts on a standardized index of antisocial behaviors over time —
Levels and average treatment effects (ATEs)

a) Levels, All Treatment Arms

b) ATE of Therapy Only

c) ATE of Cash Only

d) ATE of Therapy + Cash
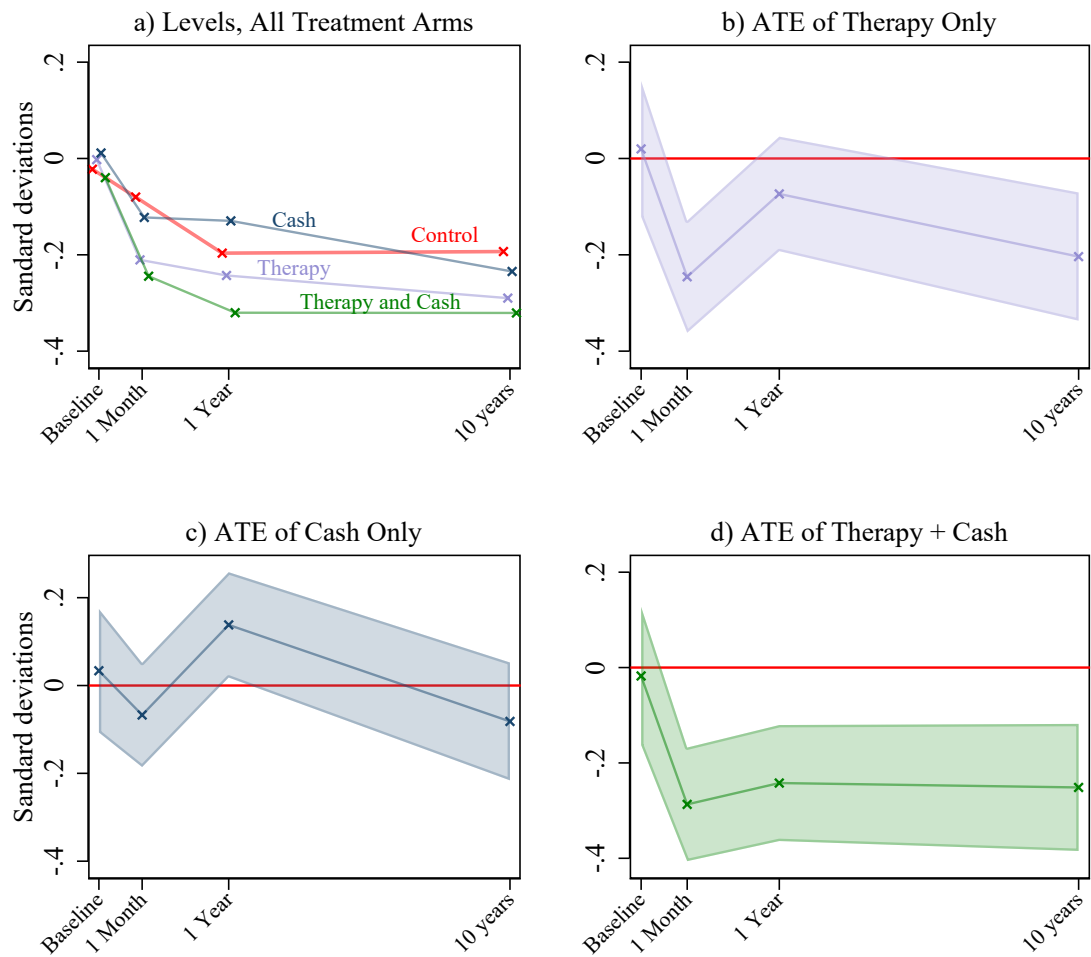
Notes: The estimates control for baseline covariates and randomization block fixed effects. The antisocial behaviors index is a composite of underlying survey variables, and here the index is standardized to have zero mean at baseline, and unit standard deviation across all survey rounds. The 95% confidence intervals use heterosketastic-robust standard errors.

Table 3: 10-year impacts on antisocial behaviors

	Control Mean	Average Treatment Effects			Differences	
		Therapy Only	Cash Only	Both	Therapy Only vs Both	Cash Only vs Both
		Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate [p-value]	Estimate [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)
Antisocial Behaviors (z-score)	0.122	-0.204*	-0.082	-0.251**	-0.048	-0.170*
	(1.156)	(0.106)	(0.104)	(0.104)	(0.614)	(0.079)
		[0.055]	[0.431]	[0.016]		
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]		
# of thefts/robberies in past two weeks (count)	2.275	-1.346**	-0.457	-1.266**	0.080	-0.809**
	(9.062)	(0.609)	(0.545)	(0.645)	(0.817)	(0.047)
		[0.027]	[0.402]	[0.050]		
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**	-0.006	-0.044	0.031	-0.039
	(0.339)	(0.033)	(0.036)	(0.035)	(0.329)	(0.282)
		[0.024]	[0.878]	[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]		
Aggresive 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]		

Notes: The table reports intent-to-treat estimates of each treatment arm after 10 years, controlling for baseline covariates and block fixed effects, as in Equation 1. Indexes are standardized to have zero mean and unit standard deviation. We re-scale all indexes to have mean zero for this 10-year round (rather than normalize to baseline, as in Figure 1.) Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

and 0.204 standard deviations after 1 month, 1 year, and 10 years. None of the differences between these rounds are statistically significant (see Table 8 below).

4.1 Index components

Next we turn to the components of the antisocial behavior index, in both Table 3 and Figure 2. We focus principally on the effects of Therapy Only and Therapy+Cash. In general, both are associated with substantively large declines in almost all components. While many of these component treatment effects are substantively large, however, not all are statistically significant. Moreover, examining the components increases the number of hypotheses, so we have to treat them with caution, as exploratory analysis. To give a sense of what is likely to be driving the overall index, the following discussion focuses on the magnitudes rather than the significance of the component treatment effects.

Drug Selling At baseline, 17% of the control group reported selling drugs often, falling to 13.5% after 1 year and 10.2% after 10. This decline is steeper in the therapy arms. After 10 years, drug selling is 1.6 percentage points lower with Therapy Only (a -16% change relative to the 10-year control mean $p = 0.542$) and 4.6 percentage points lower with Therapy+Cash (-45%, $p = 0.092$).

Thefts and robberies We asked men the number of times they committed a crime in the previous 2 weeks. (We asked about 8 kinds of crime, from pick-pocketing to armed robbery.) In the control group, the total fell from 5.8 acts at baseline to 1.9 after a year and 2.3 after 10 years. After 10 years, men receiving Therapy Only reported 1.4 fewer crimes (-61%, $p = 0.027$) than the control group. Those receiving Therapy+Cash reported a decrease of 1.3 crimes (-57%, $p = 0.050$).

Disputes and fights We also asked about 9 types of verbal and physical altercations in the prior 2 weeks, including the frequency and severity of disputes with peers, neighbors,

Figure 2: Average treatment effects on the components of the antisocial behavior index over time

a) ATE of Therapy Only

b) ATE of Cash Only

c) ATE of Therapy + Cash
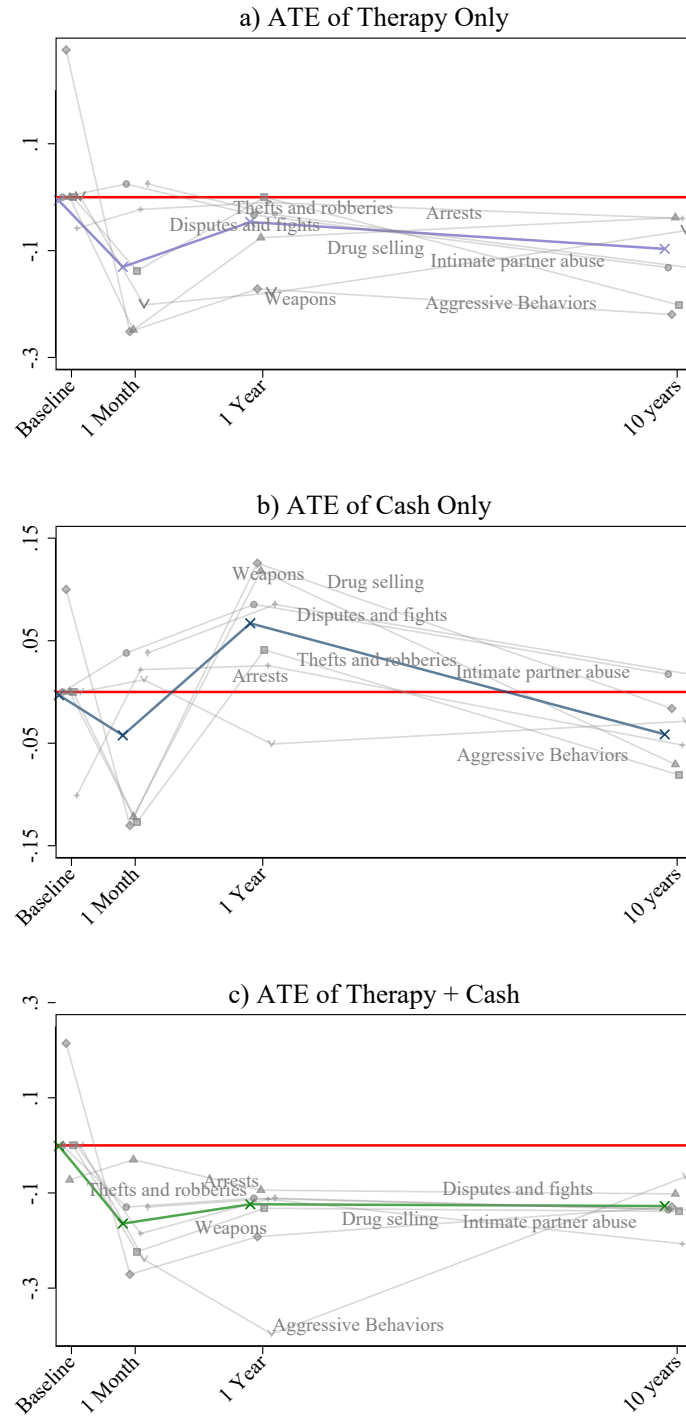
Notes: The figures display intent-to-treat estimates for each component of the antisocial behaviors family index at baseline as well as 1-month, 1-year, and 10-years following treatment. All measures are composites of underlying survey variables. Indexes are standardized to have zero mean at baseline and unit standard deviation over all rounds.

leaders, or police. For instance, the control group reported 2 physical fights at baseline, compared to 0.78 after a year and 0.76 after 10. After 10 years, a standardized index of these disputes was 0.14 standard deviations lower with Therapy Only ($p = 0.073$) than the control group and 0.13 standard deviations lower with Therapy+Cash ($p = 0.051$).

Weapons   At baseline, about 16% of the control group said they carried a weapon on their body, falling to 15% after a year, and 13.2% after 10. (Typically this was a knife, as guns are rare.) After 10 years, weapons-carrying was 7.5 percentage points lower with Therapy Only (-57%, $p = 0.024$) and 4.4 percentage points lower with Therapy+Cash (-33%, $p = 0.205$).

Arrests   14% of the control group reported an arrest in the 2 weeks before the 1-month survey, 12% before the 1-year survey, and 8% before the 10-year survey. This fell with therapy. After 10 years this was 1.2 percentage points lower in the Therapy Only arm (-15%, $p = 0.610$) and 2.9 percentage points lower in the Therapy+Cash arm (-36%, $p = 0.217$).

Aggressive behaviors   We asked 19 questions about reactive and proactive aggression, such as the frequency with which the subjects yell, curse, bully others, cheat, or lose their tempers. After 10 years, a standardized index declines: a fall of 0.060 standard deviations with Therapy Only ($p = 0.301$) and a decline of 0.062 with Therapy+Cash ($p = 0.316$).

Intimate partner abuse   Finally, we have a crude measure of intimate partner abuse among those with a partner—3 questions on verbal abuse and one on physical abuse in the past two weeks. A standardized index shows almost no improvement from Therapy Only (0.032 standard deviation increase, $p = 0.767$) and a moderate decrease of 0.082 standard deviations from Therapy+Cash ($p = 0.453$).

4.2 Heterogeneity analysis

Mechanically, we should expect the most opportunity for improvement among the men with the highest initial engagement in crime and violence. In line with our analysis of 1-year results, we analyze heterogeneity in treatment effects by baseline antisocial behavior. We find that the average treatment effect seems to be driven entirely by the most antisocial subset of this sample.

Table 4 estimates treatment effects with an interaction between each treatment arm and a continuous measure of baseline antisocial behavior (with zero mean and unit standard deviation). The coefficients on each of the interaction terms are large, negative, and statistically significant, indicating again that treatment effects are greatest in those who reported higher initial levels of crime and violence.

There are indications that treatment effects are concentrated in the most anti-social subset of this already relatively antisocial sample. Figure 3 plots average treatment effects on antisocial behavior for two subgroups: those above and below the 75th percentile of baseline crime and violence. In this highest quartile group, Therapy Only and Therapy+Cash reduce 10-year antisocial behavior by 0.710 and 0.819 standard deviations, $p < 0.01$. Effects in the lower three quartiles are close to zero, however. This was not our pre-specified split, however, and so this is merely suggestive.[12]

4.3 Impacts on violent death

None of these reductions in criminal and violent behavior translate into reduced chance of death, however. As we saw in Table 2 above, the Therapy+Cash arm had similar levels of mortality as the control arm, and the Therapy Only arm had slightly elevated levels of mortality (though not statistically significant).

To determine cause of death we interviewed two friends or relatives of every respondent

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[12]We test this formally in Appendix Table A.4. We report heterogeneity for all outcomes with a continuous measure of baseline anti-social behavior in Appendix Table A.3.

Table 4: Heterogeneity in program impacts by baseline antisocial behavior

	Sample Mean (SD)	Antisocial behavior Estimate (SE) [p-value]
	(1)	(2)
Therapy Only	0.266	-0.181*
	(0.442)	(0.099)
		[0.068]
Cash Only	0.251	-0.076
	(0.434)	(0.099)
		[0.440]
Therapy + Cash	0.264	-0.267***
	(0.441)	(0.099)
		[0.007]
Therapy Only × Baseline ASB	-0.008	-0.353***
	(0.476)	(0.101)
		[0.001]
Cash Only × Baseline ASB	-0.003	-0.165*
	(0.506)	(0.097)
		[0.090]
Therapy + Cash × Baseline ASB	-0.010	-0.408***
	(0.499)	(0.098)
		[0.000]
Baseline ASB	-0.024	0.477***
	(0.975)	(0.075)
		[0.000]
Baseline Controls		✓
Strata FE		✓

Notes: The table reports the same intent-to-treat estimates of each treatment arm after 10 years as in Table 3, interacting each treatment indicator with baseline antisocial behavior (ASB). Heteroskedastic-robust standard errors are reported in parentheses and p-values in brackets.

Figure 3: Heterogeneity in program impacts by baseline antisocial behavior

a) ATE of Therapy Only

b) ATE of Cash Only

c) ATE of Therapy + Cash
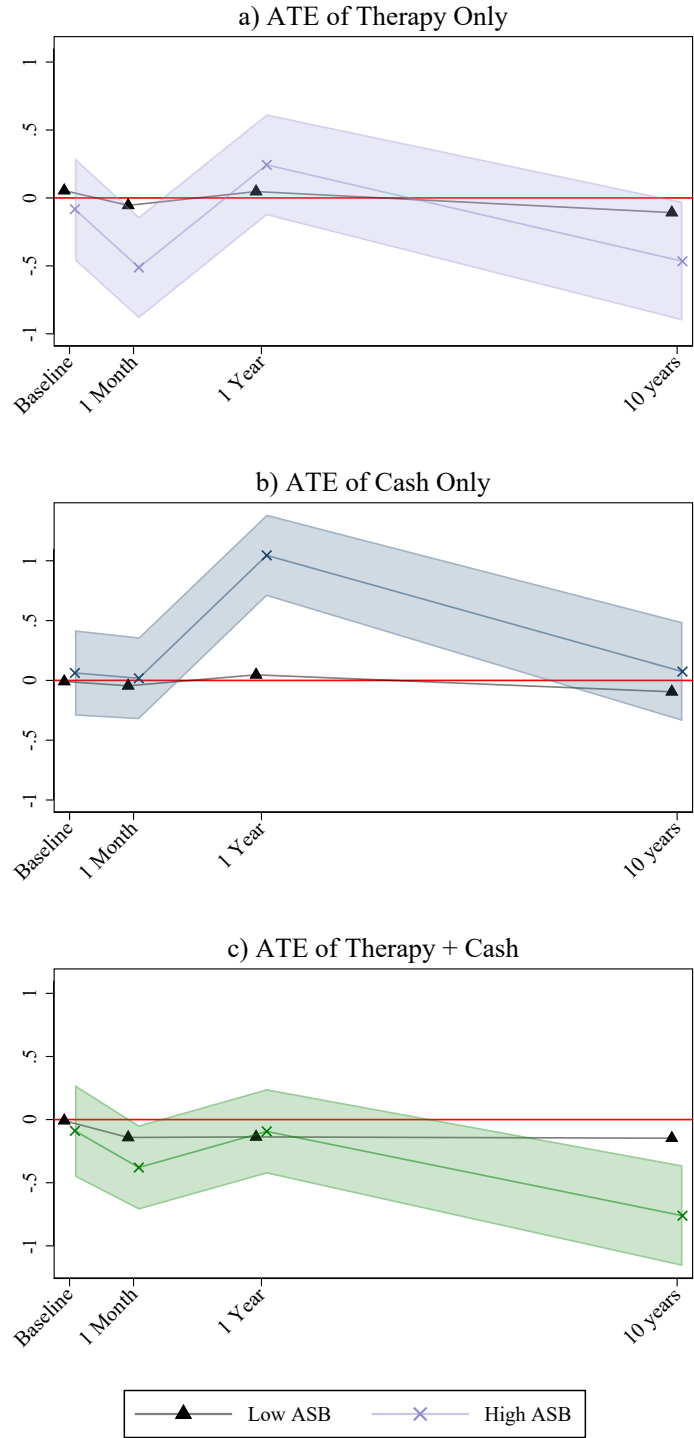
Low ASB   High ASB

Notes: The figure reports intent-to-treat estimates of each treatment arm after 10 years for two subgroups: those with baseline antisocial behavior above the 75th percentile (high ASB) and those below it (Low ASB). See Appendix Table A.4 for regression results.

reported as having died. We collected quantitative and lengthy qualitative explanations on the circumstances around the death, related events, and so on. If there were serious disagreements we sought out additional sources, but in practice this was rare.

The city, which sits in swampy terrain, has endemic malaria among other tropical diseases. Diseases like tuberculosis are also commonplace. The health system is rudimentary, even by regional standards, and the country has one of the lowest human development indexes in the world. Tragically, the city also experienced a major Ebola outbreak in the years between the 1- and 10-year surveys. Changes in antisocial behavior may not be protective under these extreme circumstances.

Table 5 reports counts of death by reported cause. The total number of deaths is 109—6 greater than described in the attrition analysis above—because tragically there were 6 new deaths in the sample reported after the 10-year endline surveys.[13] In all, 60% of the deaths are due to illnesses and other health problems—including Ebola, tuberculosis, and a variety of sudden deaths and unexplained illnesses that could be due to any number of (usually) undiagnosed conditions or diseases. In an additional 6% of cases, our informants suggested that drug use aggravated the illness, for a total of about two-thirds dying from illnesses. We had no reports of death by overdose—perhaps this population is seldom able to obtain or afford a sufficiently large and pure dose to overconsume. We see no statistically significant differences across treatment arms (see Table 6).

We identified 26 violent deaths. Most of these are associated with mob violence—the lynching and killing of a robber by the community. There were roughly equal numbers (4 to 5) in the control, Cash Only and Therapy+Cash arms—equal to about 1.5% of the population of the treatment arm. The highest number of deaths is in the Therapy Only group, with 13 violent deaths. None of these differences across arms are statistically significant, however (Table 6). The higher number of violent deaths in the Therapy Only group partly parallels the higher number of deaths in that treatment arm across most causes, including illness.

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[13]The analysis below is qualitatively the same if we focus only on the 103.

Table 5: Death type by treatment arm

Death type	Control	Therapy	Cash	TP + Cash	Total
Accidental death	0	0	4	1	5
Health complications, drug aggravated	1	3	0	2	6
Health complications, not drug aggravated	13	21	19	12	65
Violent death	4	13	4	5	26
Other	1	4	2	0	7
Total # of deaths	19	41	29	20	109

Notes: Simple tabulation of deaths over treatment arms. Note that the number of deaths is higher than the number of attrition cases due to death since 6 deaths were recorded after completing one or two surveys in this wave.

Table 6: Treatment effects on death types

	Sample Mean (SD)	Violent Death Estimate (SE)	Health complications Estimate (SE)	Health complications: aggravated by drugs Estimate (SE)
	(1)	(2)	(3)	(4)
Therapy Only	0.280	0.024	0.018	0.006
	(0.449)	(0.015)	(0.025)	(0.007)
Cash Only	0.250	-0.005	-0.001	-0.005
	(0.433)	(0.015)	(0.025)	(0.007)
Therapy + Cash	0.249	0.000	-0.004	0.003
	(0.433)	(0.016)	(0.025)	(0.008)
Dep. var. mean		0.026	0.071	0.006
N		999	999	999

Notes: The table reports the same intent-to-treat estimates of each treatment arm after 10 years as in Table 3. Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets. Each column comes from a separate regressions which also include the main regression controls.

Could excess attrition in the Therapy Only arm lead us to overstate the effects of the treatment, by only assessing the behavior of survivors? This is part of a more general concern that selective attrition could influence our results. Sensitivity analysis in the next section suggests that this is not likely to drive our main results.

4.4 Sensitivity of results to extreme attrition scenarios

Our treatment effect estimates are robust to several conservative attrition scenarios. Even though attrition appears to have been relatively unsystematic, mortality and other attrition

is large enough that unobserved selection could influence our conclusions. For example, recall that we are slightly more likely to find men in the Therapy+Cash arm at endline. If the men who died or whom we were unable to find had systematically lower rates of antisocial behavior than the ones we found, then our estimated treatment effects would overestimate the effects of Therapy+Cash.

Table 7 reproduces our main result in Column 2 and models three selective attrition scenarios in Columns 3 through 5. In Columns 3 and 4 we impute extreme values for missing subjects. That is, we impute systematically "good" outcomes for unfound control group members and "bad" outcomes for unfound treatment group members. For missing control group members, we impute the mean observed outcome plus a 0.1 or 0.2 standard deviation improvement. We do the opposite for missing treatment group members. Thus, we re-estimate treatment effects on the full baseline sample of 999, rather than the observed sample of 833. This is a rather extreme test, as it mechanically creates a 0.2 or 0.4 standard deviation gap in performance between missing treatment and control group members, attenuating our treatment effect estimates by construction.

For Therapy+Cash, we can see that the estimated treatment effect on antisocial behavior survives a gap of 0.4 (And indeed larger). This goes to show the extreme level of systematic attrition that would have to be present to change our conclusions. Impacts on the Therapy Only arm, however, are more sensitive to these attrition scenarios, partly because of the lower effect size but mostly because of the higher levels of attrition in that arm.

Finally, we report a trimming exercise in Column 4. This approach recalculates treatment effects after dropping (rather than adding and imputing) observations, to equalize imbalance across arms. For instance, the Therapy+Cash arm has roughly 4 percentage points higher response rates than the control arm. The trimming exercise drops the highest-performing members of the Therapy+Cash arm until the two arms have equivalent attrition levels, then recalculates treatment effects.[14]

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[14]Because we have more than one treatment, and because we have different levels of attrition in each arm (sometimes in opposite directions) we calculate the estimates in this column from three separate regressions.

Table 7: Attrition bound estimates for antisocial behaviors

	Sample Mean (SD) (1)	Main reg (N=833) Estimate (SE) [p-value] (2)	Impute .1 SD (N=999) Estimate (SE) [p-value] (3)	Impute .2 SD (N=999) Estimate (SE) [p-value] (4)	Trim (N = 393, 384, 383) Estimate (SE) [p-value] (5)
Therapy Only	0.280 (0.450)	-0.204** (0.100) [0.043]	-0.156* (0.084) [0.063]	-0.120 (0.084) [0.154]	0.027 (0.092) [0.772]
Cash Only	0.250 (0.433)	-0.082 (0.101) [0.419]	-0.055 (0.084) [0.516]	-0.021 (0.084) [0.800]	-0.054 (0.120) [0.652]
Therapy + Cash	0.249 (0.433)	-0.251** (0.100) [0.013]	-0.210** (0.086) [0.015]	-0.185** (0.086) [0.031]	-0.243** (0.122) [0.049]
Baseline Controls		✓	✓	✓	✓
Strata FE		✓	✓	✓	✓

Notes: The table reports the ITT estimates on antisocial behaviors for each treatment arm when using different sensitivity analyses to model systematic attrition. Column (2) displays the main results, column (3) shows a regression where the mean + 0.1 standard deviations (SD) is imputed for each missing outcome value in the control group, while a value equal to mean - 0.1 SD is imputed for the other arms. Column (4) repeats this process with a value of +/- 0.2 SD instead. Finally, column (5) trims (drops) values as to artificially reduce the imbalance in missing values relative to the control group. The column summarizes the estimates from 3 different regressions.

Our analysis echoes the extreme values result: the Therapy+Cash impacts are highly robust to dropping the most anti-social members of that arm (to bring attrition to the same levels as the control arm) but the Therapy Only arm is not at all robust to dropping the least antisocial members of the control arm (to bring attrition levels to equal levels).

Other robustness checks   Other approaches to estimation or index construction generally leave our conclusions unchanged. For example, recall that we survey each subject twice at each survey round. If, instead of taking the average response, we treat each response as a separate observation, with standard errors clustered by individual, our estimates are similar

In each regression, we restrict the analysis to the control group and the treatment arm in question (i.e., about half the sample), in each case trimming the arm with lower attrition in the direction that would diminish treatment effects.

but statistical significance increases. Alternatively, if each family index is the average of all underlying survey questions, rather than the average of component indexes, our results are unchanged. Our results are also unchanged if these indexes are covariance-weighted rather than simple averages of the component indexes. Appendix Table A.12 reports these results.

4.5   Measurement error concerns

Our data comes from self-reported surveys. An advantage of this is that it allows us to analyze direct evidence on outcomes and mechanisms that are not typically available in administrative data. At the same time, self-reported data raises concerns of experimenter demand. In particular, we would be concerned if treated subjects were less likely to report antisocial behaviors. There are several reasons, however, to suggest that experimenter demand is minimal.

First, note that some of our measures, especially measures of time preferences, are based on incentivized games where subjects are playing for real money, and our treatment effects are strong and persistent in these outcomes.

Second, we do not see large or significant treatment effects in some of the outcomes most directly relevant to the STYL therapy, including self-control skills, anti-criminal identity and values, and substance abuse. While each of these indexes moves in the expected direction, they are smaller and less statistically significant than many of our other outcomes. If men were simply repeating back their lessons to enumerators, we might expect these treatment effects to be larger than average.

Third, we attempted to validate a subset of questions using intensive qualitative observation. One year after treatment, we selected 7% of the endline surveys for qualitative validation. A Liberian qualitative researcher visited each of these respondents several times over several days shortly after the survey, interviewing them, building trust, and observing their behavior. Through this, the qualitative researcher assessed the answers to four potentially sensitive behaviors—marijuana use, thievery, gambling, and homelessness. A

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comparison of these responses to the survey questions finds no evidence of under-reporting correlated with treatment. Rather, the patterns suggest that, if anything, the control group *under-reported* sensitive behaviors such as stealing. If so, the treatment effects may actually underestimate therapy's impacts. (See Blattman et al. (2016) and Appendix B for details.)

Our qualitative work suggests two explanations. The men have been members of a sub-culture where drugs, crime, and gambling are commonplace, and admitting to the behaviors in a survey carries little stigma. Speculatively, therapy may have accustomed men to talking about these behaviors or reduced stigma. Another possibility is that, especially at a point one year after the program, members of the control group could have believed that reporting 'good' behavior would make them more likely to receive future treatment such as cash.

5 Impacts on secondary outcomes and potential mechanisms

We registered 7 mechanisms as secondary outcomes. Table 8 reports 1- and 10-year program impacts on the family indexes for each mechanism.[15] Most *p*-value adjustments for 7 secondary outcomes would reduce statistical significance below conventional levels, and so we should take these mechanism estimates with caution, again as largely exploratory. Nonetheless, consistent with the 1-year results, we see evidence that therapy (especially Therapy+Cash) is also associated with a host of positive changes in forward-looking and prosocial behaviors.

Time preferences The STYL curriculum focused on fostering forward-looking behavior and skills of self control. This includes short-term abilities to regulate emotions and to be resistant to impulse, as well as more sustained abilities to be planful, persevering, and patient. Becoming more self controlled and forward-looking are central components of many

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[15]Appendix Tables A.11 to A.10 report for treatment effects on the components of each family index.

Table 8: 1- versus 10-year impacts on antisocial behaviors and secondary outcomes (in standard deviations)

	1- year			10- year			1- vs 10-year difference		
	Therapy Only	Cash Only	Both	Therapy Only	Cash Only	Both	Therapy Only	Cash Only	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Antisocial Behaviors	-0.074	0.138	-0.242***	-0.204*	-0.082	-0.251**	-0.111	-0.204	0.012
	(0.093)	(0.097)	(0.088)	(0.106)	(0.104)	(0.104)	(0.121)	(0.128)	(0.119)
	[0.428]	[0.153]	[0.006]	[0.055]	[0.431]	[0.016]	[0.358]	[0.112]	[0.919]
Economic performance	0.073	0.002	0.057	0.083	0.024	0.213*	-0.044	0.003	0.168
	(0.104)	(0.099)	(0.095)	(0.113)	(0.113)	(0.110)	(0.134)	(0.138)	(0.133)
	[0.487]	[0.985]	[0.551]	[0.464]	[0.835]	[0.052]	[0.742]	[0.984]	[0.206]
Forward-looking time preferences	0.141	0.099	0.199**	0.131	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]	[0.172]	[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]
Anticriminal identity & values	0.028	-0.084	0.099	-0.037	-0.102	0.100	-0.070	-0.008	-0.002
	(0.093)	(0.092)	(0.092)	(0.101)	(0.099)	(0.099)	(0.125)	(0.123)	(0.120)
	[0.767]	[0.362]	[0.278]	[0.719]	[0.301]	[0.312]	[0.574]	[0.947]	[0.985]
Positive self-regard/mental health	0.022	-0.024	0.227**	0.088	-0.031	0.207**	0.112	-0.006	-0.020
	(0.091)	(0.091)	(0.090)	(0.104)	(0.102)	(0.101)	(0.125)	(0.126)	(0.124)
	[0.808]	[0.792]	[0.012]	[0.398]	[0.759]	[0.041]	[0.371]	[0.962]	[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]	[0.988]
Quality of social networks	0.035	-0.048	-0.006	-0.010	-0.033	0.103	-0.101	-0.060	0.046
	(0.096)	(0.097)	(0.097)	(0.115)	(0.114)	(0.106)	(0.132)	(0.133)	(0.129)
	[0.717]	[0.624]	[0.952]	[0.934]	[0.773]	[0.332]	[0.443]	[0.653]	[0.724]

Notes: The table reports intent-to-treat estimates of each treatment arm after 1 and 10 years, controlling for baseline covariates and block fixed effects, as in Equation 1. All indexes are standardized to have zero mean and unit standard deviation. Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

behavior change programs (Almlund et al., 2011; Borghans et al., 2008).

We first try to capture this through a family index of time preferences. This is an index of 4 measures of patience and 4 of present-biasedness.[16] Both therapy arms are associated with more forward-looking time preferences. Therapy Only increases the index by 0.141 standard deviations after 1 year ($p = 0.139$) and 0.131 after 10 years ($p = 0.172$), but these estimates are not statistically significant. Therapy+Cash is associated with larger and more significant increases: 0.199 standard deviations after 1 year ($p = 0.043$) and 0.247 after 10 years ($p = 0.008$). Looking within the family index, point estimates are larger and more precise for patience than present-biasedness (see Appendix Table A.5).

Self control skills   Related to the above, we also measured skills of self control using standard psychometric questionnaires for four constructs that psychologists associate with less impulsive and more planful behavior: impulsiveness (the inability to control thoughts and actions); conscientiousness (the tendency to be self-disciplined and purposeful); grit (the ability to press on in the face of difficulty); and reward responsiveness (whether people are motivated by immediate, typically emotional rewards).[17] We see some evidence of short-run improvements, but this diminishes in the long run. Specifically, after a year, Therapy Only and Therapy+Cash are associated with increased self control of 0.159 and 0.244 standard deviations ($p = 0.08$ and 0.011). After 10 years, 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). This is consistent with the absence of evident program impacts on present-biasedness, above.

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[16]Specifically, we first measured a set of incentivized tradeoffs between modest amounts of money now versus in two weeks, and again in two versus four weeks, that allow us to place men in seven ordered bins of patience and time-inconsistency (for an average payout of $3, about a day's wages). Note that time inconsistency or present bias captures the desire for immediate satisfaction. We also include a hypothetical (non-incentivized) version of the same tradeoffs, with higher stakes tradeoffs. We also collected a range of self-reported assessments of time preferences.

[17]We adapted the scales and questions to the context and Liberian English. Appendix Table A.6 lists all components.

Anticriminal identity and values   The therapy also tried to foster a change in the men's identity and associated norms and values. Research in both psychology and economics supports the idea that groups have well-defined norms of behavior, and that people receive emotional benefits from acting in accordance with the norms of their perceived group (Almlund et al., 2011; Shayo, 2020). To some extent people may also be able to change their perceived social category, and with it values that reward and penalize certain behaviors (Akerlof and Kranton, 2000). Relatedly, criminologists sometimes refer to a similar process of "knifing off" from old social rules and behaviors, and associate these changes with significant turning points in life, such as marriage, a move, or a life-threatening experience.(Maruna and Roy, 2007) This literature ties successful knifing off to having a new "script" for the future. The STYL program is intended to be that script.[18]

To assess this, we first attempted to measure values directly, using a set of 33 self-reported attitudes towards the appropriate use of crime and violence in the men's own lives—indicators of the degree to which they had internalized mainstream social norms. Second, we measured a range of prosocial behaviors, including group memberships, group and community leadership, and contributions to local public goods. Finally, the therapy encouraged men to change their appearance as part of the identity change, and we asked survey enumerators to record their subjective impressions: quality of dress, shoes, cleanliness, and smell. We see some evidence of long run increases in these values, but not significantly so. Therapy Only is associated with a 0.037 standard deviation decrease ($p = 0.719$) and Therapy+Cash with a 0.100 standard deviation increase ($p = 0.312$).

Positive self-regard / mental health   This family groups a set of 6 mental health-related outcomes in the interests of minimizing the number of families. Three are measures of positive self-regard: neuroticism, self-esteem, and locus of control. Another is a classic happiness

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[18]There are also parallels between STYL and socialization into military groups, street culture, gangs and armed groups. Such groups use similar techniques (appearance change, practice, modeling) to shape young men's social identity and behavior (Vigil, 2003; Wood, 2008; Maruna and Roy, 2007). NEPI designed STYL to reverse this process.

measures, asking men to rank their subjective well being in absolute terms and relative to others in their community. Finally we assess symptoms of two disorders: depression and distress.[19] Looking at the overall index, after 10 years we see a small and not significant effect of Therapy Only (0.088 standard deviations, $p = .398$) and a larger and significant increase in mental health from Therapy+Cash (0.207 standard deviations, $p = 0.041$). The largest driver of this appears to be the subjective well-being and self esteem components, although we see meaningful improvements in other components, such as symptoms of depression (see Appendix Table A.7).

Substance abuse   Although it was not a major focus of STYL, the therapy tried to equip participants with strategies to cut back substance abuse in order to achieve their goals of identity change and reducing antisocial behaviors. Generally, we do not see any significant effect of the intervention on the three components of the index: self-reported use of alcohol, marijuana, or hard drugs. An index declines 0.058 standard deviations with Therapy Only ($p = 0.542$) and 0.102 standard deviations with Therapy+Cash ($p = 0.288$).

Quality of social networks   Finally, we also assessed risky social networks. We measured the traits, positive and negative, of men's five closest peers.[20] We also asked about closeness to and support received from family members, former rebel commanders, and "big men" (intended to connote a criminal boss). A family index of positive social networks exhibits little change after 1 year. After 10 years, there is little significant change from either Therapy Only (-0.010 standard deviations, $p = 0.934$) or Therapy+Cash (0.103 standard deviations, $p = 0.332$).

Economic Performance   Finally, therapy could also affect antisocial behaviors through income. Skills such as improved emotional regulation, planning, and conscientiousness could

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[19]We assessed 6 symptoms of depression and 12 symptoms of post traumatic stress, based on a locally adapted instrument.

[20]We ask men who their five closest peers are, by name, and then ask whether they hold any of 20 qualities ranging from positive (they work hard, save, go to school) or negative (the steal, do drugs, get in fights).

improve productivity and earning potential, raising the returns to legitimate work and increasing the opportunity cost of criminal behavior. In Blattman et al. (2017), we developed a theoretical model of occupational choice between criminal and non-criminal careers that illustrated how each of these channels could affect criminal behavior in the short and long run.

To assess this, we create an index of economic performance from several measures: earnings, consumption, homelessness, savings, investment, and employment levels. There are some indications that that the therapy arms increased long run earnings and employment. Therapy Only is weakly associated with a 0.083 standard deviation increase in economic performance ($p = 0.464$) and Therapy+Cash with a larger and more significant 0.213 standard deviation improvement ($p = 0.052$). Looking within the index, most components (earnings, savings, assets, and hours of employment) have similar treatment effects, but are not individually statistically significant (Appendix Table A.11). Altogether, this suggests that the program's investment in non-cognitive skill development had moderate long term economic returns, even when it channelled individuals out of criminal occupations.

One note: thus far this discussion has focused on the therapy arms, and potential mechanisms for the long run impacts on antisocial behavior. At the outset of the study, however, we hypothesized direct effects of the cash arms on economic performance as well.

As discussed above, however, the reason the direct effects of Cash Only was not a focus of the 10-year study, however, is simple: after 1 year, neither cash treatment had a significant impact on economic performance. Specifically, in Blattman et al. (2017), we found that employment and incomes rose in the first month following the cash grant, as the men started small legal enterprises (such as market trading or shoe-shining). After one year, however, these impacts had dissipated, and men in the Cash and Therapy+Cash arms had similar employment and incomes as the control group. Our qualitative and quantitative investigations 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.

In line with this, 10 years after the grants, we continue to see no effect of Cash Only on economic performance (0.024 standard deviation increase, $p = 0.835$). Therefore, in general, this paper does not dwell on the long term direct effects of cash, except to the extent that the temporary income and employment boost seems to have interacted with therapy to make the Therapy+Cash arm more impactful than Therapy Only. We will return to this theme in the discussion, below.

6 Discussion and conclusions

Cognitive behavioral therapy-informed programs have emerged as one of the most promising alternatives to policing and incarceration. What has been been unclear, however, is whether the effects of therapy are lasting, especially therapy alone, without any other assistance transitioning to a new non-criminal lifestyle and career. The existing literature, which looks mostly over horizons of a few months to a year or two, has suggested the benefits of therapy alone dissipate within that time frame. CBT programs might still pass a cost-benefit test if this is the case, due to the costly crime and violence they temporarily deter, but this would still be a disappointing result for policymakers and practitioners looking for alternatives to incarceration.

The 10-year results from Liberia suggest instead that behavior changes can be lasting, especially when therapy is combined with even temporary economic assistance. The results also suggest that there are high returns to targeting the most violent and antisocial young men, at least among those who are willing to consider participating in any such program in the first place. Program impacts were almost entirely concentrated in the quarter of our sample most involved in crime and violence at baseline, and not purely for mechanical reasons. Finding ways to better identify and engage these youth is an important area for investment and experimentation.

Why do we see slightly larger, more statistically significant, and less sensitive impacts among those who received both the STYL therapy as well as cash? Recall that cash by itself only raised employment and incomes for a few weeks or months, before the businesses collapsed, and so the cash arm did not alter the opportunity cost of engaging in crime at either our 1- or 10-year endlines.

The psychological theory underlying CBT suggests a plausible 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. The therapy helped participants change their intentions, identity and behavior, and provided almost daily commitment and reinforcement. After eight weeks of therapy the grant provided some men with the cash they needed to maintain their new identity—to avoid homelessness, to feed themselves, and to continue to dress decently. Thus they had no immediate financial need to return to crime. The men could also do something consistent with their new identity and skills: execute plans for a business. This was a source of practice and reinforcement of their new skills and identity.

In this way, the grant may have parallels to "booster sessions" commonly used in therapy. A small body of 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).

This is an important hypothesis for future research. In particular, a comparison of extended therapy to shorter therapy plus cash would offer a more direct test of these approaches, as would a test of booster sessions.

This approach has promise beyond West Africa, and cities around the world have already begun to mimic STYL and the therapy—cash combination. STYL was adapted from U.S.-based CBT programs, suggesting that adaptability to even more contexts is feasible. More long term evidence from other settings and interventions is essential, however, to better understand what can lead CBT-induced behavior change to endure.

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Online Appendix

A   Additional results

This section includes supplementary tables referred to in the main body of the paper.

Table A.1: Study sample and treatment assignment by randomization block

	Round 1					Round 7				
	#	Therapy (%)	Cash (%)	Therapy cash (%)	Control (%)	#	Therapy (%)	Cash (%)	Therapy cash (%)	Control (%)
Red light	100	28.0	24.0	25.0	23.0	72	25.0	23.6	30.6	20.8
Red light, second phase	219	26.9	25.1	24.2	23.7	182	26.4	24.2	25.8	23.6
Central Monrovia	179	31.8	19.0	31.8	17.3	157	29.3	19.7	33.1	17.8
Clara Town	175	28.6	27.4	22.9	21.1	140	26.4	28.6	22.1	22.9
Logan Town	86	26.7	29.1	19.8	24.4	67	23.9	28.4	22.4	25.4
New Kru Town	240	26.3	26.7	23.8	23.3	215	26.0	27.0	25.1	21.9

Table A.2: Baseline statistics and balance test

	Sample	Test of randomization balance (N=999)						F-Test
		Assigned therapy		Assigned cash		Assigned both		
Baseline covariate	Mean	Coeff.	p value	Coeff.	p value	Coeff.	p value	p value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	25.40	-0.16	0.68	0.19	0.59	-0.18	0.68	0.18
Married or partnered	0.16	-0.03	0.65	-0.04	0.66	0.04	0.75	0.93
# of partners	0.53	0.05	0.45	0.12	0.18	-0.20	0.12	0.11
# of children<15 in household	2.21	-0.58	0.07	-0.51	0.19	0.64	0.29	0.32
Sees family often	2.37	0.12	0.00	0.24	0.01	-0.29	0.01	0.01
Muslim	0.10	0.02	0.64	0.00	0.96	0.01	0.87	0.18
Years of schooling	7.72	-0.20	0.68	0.03	0.95	-0.01	0.99	0.55
Currently in school	0.06	-0.03	0.08	-0.03	0.13	0.04	0.11	0.16
Literacy index (0-2)	1.23	0.14	0.12	0.12	0.30	-0.27	0.08	0.13
Math score (0-5)	2.79	-0.10	0.25	-0.03	0.85	-0.15	0.39	0.89
Health index (0-6)	4.87	-0.09	0.11	-0.19	0.17	0.31	0.15	0.28
Has any disabilities	0.08	0.04	0.29	0.00	1.00	-0.04	0.48	0.19
Depression index (0-17)	7.09	0.18	0.41	-0.01	0.97	-0.11	0.80	0.45
Distress index (0-21)	7.46	0.14	0.44	-0.01	0.98	-0.36	0.30	0.42
Relations to commanders index (0-4)	0.45	0.00	0.93	0.07	0.42	-0.06	0.55	0.72
Ex-combatant	0.38	0.06	0.09	0.08	0.08	-0.07	0.11	0.12
War experiences index (0-12)	5.86	0.34	0.25	0.15	0.54	-0.63	0.13	0.32
Weekly cash earnings (USD)	17.02	-1.89	0.03	-4.85	0.03	5.48	0.00	0.02
Summary index of income, z-score	0.00	-0.22	0.05	-0.12	0.48	0.26	0.21	0.07
Homeless in past two weeks	0.24	-0.01	0.80	0.00	0.93	-0.02	0.75	0.26
# of days slept hungry, last 7 days	1.26	0.25	0.10	0.28	0.05	-0.32	0.09	0.14
Savings stock (USD)	33.75	-10.08	0.26	-12.84	0.31	15.69	0.31	0.53
Can get loan of 50 USD	0.52	-0.03	0.59	-0.06	0.28	0.05	0.46	0.50
Can get loan of 300 USD	0.11	-0.03	0.27	-0.03	0.33	0.06	0.07	0.13
Hours in illicit activities	13.55	1.21	0.68	-0.86	0.67	0.06	0.99	0.14
Hours/week in agriculture	0.36	0.34	0.26	-0.10	0.35	0.13	0.84	0.01
Hours/week in low-skill wage labor	19.39	0.54	0.88	1.24	0.73	-0.43	0.90	0.94
Hours/week in low-skill business	11.53	0.16	0.92	-1.53	0.60	5.76	0.13	0.50
Hours/week in high-skill work	1.51	-0.05	0.91	0.94	0.03	0.11	0.85	0.01
Years of experience in agriculture	0.78	-0.21	0.29	-0.34	0.07	0.25	0.32	0.15
Years experience in non-agricultural business	2.96	-0.35	0.36	-0.80	0.05	0.97	0.08	0.04
Years experience in high-skill work	0.96	-0.29	0.13	-0.27	0.41	0.62	0.12	0.02
Sells drugs	0.20	0.01	0.69	0.00	0.92	0.00	0.93	0.92
Drinks alcohol	0.75	0.08	0.19	0.07	0.23	-0.07	0.23	0.31
Uses marijuana	0.59	0.12	0.02	0.08	0.02	-0.14	0.01	0.01
Uses marijuana daily	0.44	0.08	0.15	0.04	0.13	-0.09	0.22	0.34
Use hard drugs	0.26	-0.01	0.81	0.02	0.58	-0.01	0.81	0.83
Uses hard drugs daily	0.15	-0.04	0.21	0.02	0.52	0.01	0.90	0.37

Continued on following page.

Table A.2 (continued): Baseline statistics and balance test

	Sample	Test of randomization balance (N=999) (continued)						F-Test
		Assigned therapy		Assigned cash		Assigned both		
Baseline covariate	Mean	Coeff.	p value	Coeff.	p value	Coeff.	p value	p value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Committed theft/robbery in past 2 weeks	0.53	0.05	0.51	0.01	0.61	-0.02	0.62	0.77
Number of nonviolent stealing incidents	5.08	-0.36	0.58	-0.47	0.67	0.40	0.69	0.87
Number of felony stealing incidents	0.43	0.06	0.77	0.17	0.60	-0.17	0.67	0.86
Disputes and fights in past 2 weeks (0-9)	2.16	0.14	0.80	0.32	0.64	-0.66	0.25	0.69
Aggressive behaviors (mean of 19), z-score	0.00	0.05	0.66	0.13	0.22	-0.23	0.09	0.23
Conscientiousness index (0-24)	15.33	-0.05	0.70	-0.20	0.30	0.00	0.99	0.09
Neuroticism index (0-21)	12.08	-0.07	0.76	0.16	0.63	0.10	0.85	0.33
Grit index (0-21)	13.79	0.07	0.54	-0.07	0.82	0.00	0.99	0.20
Reward responsiveness index (0-24)	14.67	-0.16	0.37	-0.04	0.92	-0.21	0.70	0.92
Locus of control index (0-24)	14.45	-0.09	0.77	-0.43	0.15	0.45	0.29	0.00
Impulsiveness index (0-21)	9.35	0.35	0.39	0.16	0.65	-0.79	0.09	0.35
Self esteem index (0-24)	13.47	-0.08	0.78	-0.11	0.65	0.12	0.75	0.89
Patience in game play index (0-6)	4.12	0.05	0.62	-0.08	0.71	0.05	0.89	0.87
Time inconsistency in game play index (0-6)	3.27	-0.22	0.02	-0.05	0.62	0.13	0.33	0.01
Risk aversion index (0–3)	1.57	-0.01	0.90	-0.05	0.60	0.10	0.41	0.64
Self-reported patience (mean of 7), z-score	0.00	-0.08	0.62	-0.13	0.25	0.15	0.42	0.33
Declared Risk Appetite (mean of 6), z-score	0.00	0.01	0.94	-0.02	0.88	-0.10	0.65	0.94
Cognitive ability (z-score)	0.00	0.15	0.05	0.14	0.20	-0.29	0.01	0.04
Executive function (z-score)	0.00	0.07	0.18	0.10	0.45	-0.25	0.06	0.16
R-Squared		0.16		0.12		0.35		
p Value on F-statistics on all covariates		0.50		0.44		0.50		

Notes: Column (1) reports the sample mean. A small number of missing values are imputed at the median. Columns (2)-(7) report the coefficients and p values from ordinary least squares regressions of each baseline covariate on three indicators, one for assignment to each treatment arm, controlling for block fixed effects. Column (8) reports the p value from a joint test of statistical significance of all three treatment indicators.

Table A.3: Heterogeneity in program impacts by baseline antisocial behavior (standardized)

	Sample Mean (SD)	Antisocial behavior Estimate (SE) [p-value]	Economic variables Estimate (SE) [p-value]	Identity Estimate (SE) [p-value]	Time prefences Estimate (SE) [p-value]	Self-control Skills Estimate (SE) [p-value]	Mental health Estimate (SE) [p-value]	Social networks Estimate (SE) [p-value]	Substance abuse Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Therapy Only	0.266 (0.442)	-0.181* (0.099) [0.068]	0.045 (0.106) [0.675]	-0.037 (0.098) [0.703]	0.151 (0.094) [0.109]	0.134 (0.098) [0.174]	0.108 (0.100) [0.281]	0.018 (0.113) [0.872]	0.004 (0.087) [0.964]
Cash Only	0.251 (0.434)	-0.076 (0.099) [0.440]	0.009 (0.106) [0.934]	-0.106 (0.098) [0.282]	0.015 (0.094) [0.878]	0.007 (0.098) [0.947]	-0.069 (0.100) [0.493]	-0.002 (0.113) [0.985]	0.015 (0.087) [0.860]
Therapy + Cash	0.264 (0.441)	-0.267*** (0.099) [0.007]	0.224** (0.107) [0.037]	0.128 (0.099) [0.196]	0.266*** (0.095) [0.005]	0.104 (0.099) [0.296]	0.211** (0.101) [0.037]	0.122 (0.114) [0.284]	-0.066 (0.088) [0.453]
Therapy Only × Baseline ASB	-0.008 (0.476)	-0.353*** (0.101) [0.001]	0.156 (0.109) [0.152]	0.452*** (0.101) [0.000]	0.294*** (0.097) [0.002]	0.154 (0.101) [0.128]	0.220** (0.103) [0.033]	0.003 (0.116) [0.979]	-0.143 (0.089) [0.110]
Cash Only × Baseline ASB	-0.003 (0.506)	-0.165* (0.097) [0.090]	0.060 (0.104) [0.564]	0.211** (0.097) [0.029]	0.219** (0.093) [0.019]	-0.020 (0.097) [0.838]	0.034 (0.099) [0.730]	0.018 (0.111) [0.873]	-0.151* (0.086) [0.079]
Therapy + Cash × Baseline ASB	-0.010 (0.499)	-0.408*** (0.098) [0.000]	0.228** (0.106) [0.031]	0.309*** (0.098) [0.002]	0.202** (0.094) [0.032]	0.146 (0.098) [0.136]	0.212** (0.100) [0.035]	0.122 (0.112) [0.278]	-0.185** (0.087) [0.034]
Baseline ASB	-0.024 (0.975)	0.477*** (0.075) [0.000]	-0.136* (0.081) [0.092]	-0.409*** (0.075) [0.000]	-0.262*** (0.072) [0.000]	-0.140* (0.075) [0.062]	-0.177** (0.077) [0.021]	-0.058 (0.086) [0.498]	0.250*** (0.067) [0.000]
Baseline Controls		✓	✓	✓	✓	✓	✓	✓	✓
Strata FE		✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports the same intent-to-treat estimates of each treatment arm after 10 years as in Table 3, interacting each treatment indicator with a continuous, standardized measure of baseline antisocial behavior (ASB). Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

Table A.4: Heterogeneity in program impacts by highest quartile of baseline antisocial behavior

	Sample Mean (SD)	Antisocial behavior Estimate (SE) [p-value]	Economic variables Estimate (SE) [p-value]	Identity Estimate (SE) [p-value]	Time prefences Estimate (SE) [p-value]	Self-control Skills Estimate (SE) [p-value]	Mental health Estimate (SE) [p-value]	Social networks Estimate (SE) [p-value]	Substance abuse Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Therapy Only	0.266	-0.710***	0.277	0.529***	0.423**	0.375*	0.592***	0.163	-0.146
	(0.442)	(0.196)	(0.211)	(0.196)	(0.188)	(0.195)	(0.198)	(0.224)	(0.174)
		[0.000]	[0.189]	[0.007]	[0.025]	[0.055]	[0.003]	[0.467]	[0.401]
Cash Only	0.251	-0.194	0.127	0.089	0.219	-0.082	-0.104	0.084	-0.142
	(0.434)	(0.200)	(0.216)	(0.201)	(0.192)	(0.200)	(0.202)	(0.229)	(0.178)
		[0.334]	[0.557]	[0.657]	[0.254]	[0.682]	[0.608]	[0.712]	[0.425]
Therapy + Cash	0.264	-0.819***	0.536**	0.433**	0.523***	0.268	0.494**	0.359	-0.245
	(0.441)	(0.194)	(0.209)	(0.195)	(0.186)	(0.193)	(0.196)	(0.222)	(0.172)
		[0.000]	[0.010]	[0.026]	[0.005]	[0.167]	[0.012]	[0.106]	[0.155]
Therapy Only × Low Initial ASB	0.197	0.702***	-0.309	-0.761***	-0.360*	-0.321	-0.642***	-0.185	0.197
	(0.398)	(0.226)	(0.243)	(0.226)	(0.216)	(0.225)	(0.228)	(0.258)	(0.200)
		[0.002]	[0.203]	[0.001]	[0.096]	[0.153]	[0.005]	[0.472]	[0.325]
Cash Only × Low Initial ASB	0.192	0.164	-0.158	-0.261	-0.276	0.114	0.040	-0.118	0.209
	(0.394)	(0.229)	(0.247)	(0.230)	(0.220)	(0.229)	(0.232)	(0.262)	(0.203)
		[0.475]	[0.523]	[0.257]	[0.211]	[0.620]	[0.865]	[0.652]	[0.304]
Therapy + Cash × Low Initial ASB	0.197	0.734***	-0.420*	-0.404*	-0.335	-0.217	-0.371	-0.315	0.237
	(0.398)	(0.224)	(0.241)	(0.225)	(0.215)	(0.223)	(0.226)	(0.256)	(0.199)
		[0.001]	[0.082]	[0.072]	[0.119]	[0.332]	[0.101]	[0.219]	[0.232]
Low Initial ASB	0.751	-0.730***	0.293	0.459**	0.457**	0.275	0.563***	0.365	-0.273
	(0.433)	(0.205)	(0.221)	(0.206)	(0.197)	(0.204)	(0.207)	(0.234)	(0.182)
		[0.000]	[0.186]	[0.026]	[0.020]	[0.180]	[0.007]	[0.120]	[0.135]
Baseline Controls		✓	✓	✓	✓	✓	✓	✓	✓
Strata FE		✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports the same intent-to-treat estimates of each treatment arm after 10 years as in Table 3, inter-acting each treatment indicator with an indicator for being below the 75th percentile of baseline antisocial behavior (ASB). Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

Table A.5: Program impacts on components of the time preferences index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Forward-looking time preferences	832	-0.384	0.131	0.096	0.172	0.013	0.097	0.895	0.247***	0.093	0.008
Self-reported svy. questions on patience (3 Q's, z-score)	832	-0.226	0.081	0.095	0.394	-0.100	0.095	0.292	0.177*	0.095	0.062
Self-reported svy. questions on time inconsistency (3 Q's z-score)	832	-0.527	-0.079	0.112	0.481	-0.131	0.113	0.247	0.050	0.104	0.631
Variables obtained from patience games											
Incentivized trade-offs (0 to 6)	828	4.149	0.017	0.179	0.926	0.046	0.184	0.803	0.284	0.184	0.123
Hypothetical trade-offs (0 to 6)	832	3.703	0.495**	0.241	0.040	0.320	0.241	0.185	0.627***	0.237	0.008
Hypothetical discount rate (.9 to 4)	828	2.005	-0.039	0.120	0.748	-0.054	0.117	0.647	-0.212*	0.117	0.071
Variables obtained from time inconsistency games											
Incentivized trade-offs (-3 to 3)	828	0.243	-0.056	0.077	0.464	-0.073	0.075	0.334	-0.028	0.074	0.702
Hypothetical trade-offs (-3 to 3)	832	0.121	-0.179*	0.092	0.050	-0.053	0.096	0.580	-0.093	0.096	0.330
Hypothetical discount rate (-.3.1 to 3.1)	828	0.005	0.025	0.092	0.788	0.079	0.096	0.413	0.020	0.088	0.825

Table A.6: Program impacts on components of the self control index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Self-control skills	832	-0.110	0.178*	0.103	0.086	0.049	0.103	0.634	0.119	0.100	0.235
Impulsiveness (z-score)	829	0.072	-0.053	0.105	0.610	0.001	0.105	0.993	-0.068	0.099	0.491
Conscientiousness (z-score)	832	-0.054	0.110	0.114	0.336	-0.034	0.107	0.752	0.026	0.111	0.812
Perseverance / GRIT (z-score)	832	-0.058	0.145	0.106	0.173	-0.057	0.105	0.586	0.042	0.106	0.689
Reward responsiveness (z-score)	832	0.083	-0.118	0.108	0.275	-0.214**	0.109	0.049	-0.150	0.106	0.158

Table A.7: Program impacts on components of the identity and values index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Anticriminal identity & values	832	-0.017	-0.037	0.101	0.719	-0.102	0.099	0.301	0.100	0.099	0.312
Attitudes toward use of violence (sum of 11 indicator Q's.)	832	1.060	0.017	0.152	0.908	-0.031	0.149	0.832	0.012	0.151	0.935
Attitudes toward criminality, (sum of 12 indicator Q's.)	832	2.984	-0.180	0.181	0.321	-0.118	0.165	0.474	-0.232	0.172	0.178
Attitudes toward political violence (sum of 6 indicator Q's.)*	794	0.121	0.014	0.060	0.810	0.141**	0.065	0.031	-0.007	0.056	0.894
Index of appearance (z-score)	828	0.161	-0.079	0.085	0.356	-0.112	0.091	0.220	0.026	0.084	0.754
Prosocial behavior (z-score)	832	0.187	-0.068	0.115	0.552	-0.216*	0.114	0.060	-0.035	0.113	0.760

Table A.8: Program impacts on components of the mental health index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Positive self-regard/mental health	828	-0.089	0.088	0.104	0.398	-0.031	0.102	0.759	0.207**	0.101	0.041
Neuroticism (z-score)	806	0.054	-0.101	0.112	0.365	0.013	0.107	0.907	-0.107	0.112	0.339
Locus of control (z-score)	806	0.069	-0.032	0.114	0.779	-0.248**	0.114	0.030	-0.094	0.112	0.400
Self esteem (z-score)	806	-0.027	-0.067	0.110	0.541	-0.041	0.106	0.698	0.144	0.107	0.177
Summary index of subjective well being (z-score)	828	-0.055	0.101	0.083	0.223	-0.009	0.084	0.911	0.348***	0.086	0.000
Depression, 6 Q's. (0 to 17)*	802	7.989	-0.384	0.357	0.283	-0.254	0.360	0.481	-0.596*	0.360	0.098
Distress (z-score)	802	0.355	-0.155	0.109	0.157	0.010	0.108	0.923	-0.094	0.104	0.368

Table A.9: Program impacts on components of the substance abuse index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Substance abuse	832	0.141	-0.052	0.083	0.533	-0.041	0.082	0.619	-0.096	0.083	0.244
Usually drinks (0-1)	832	0.690	-0.065	0.041	0.114	-0.043	0.042	0.311	-0.023	0.041	0.571
Usually uses marijuana (0-1)	832	0.462	-0.038	0.042	0.368	-0.034	0.042	0.418	-0.075*	0.041	0.067
Usually takes hard drugs (0-1)	832	0.220	0.054	0.038	0.159	0.038	0.038	0.316	0.012	0.038	0.761

Table A.10: Program impacts on components of the social networks index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Quality of social networks	832	0.123	-0.010	0.115	0.934	-0.033	0.114	0.773	0.103	0.106	0.332
Peers quality (z-score)	802	0.371	-0.191**	0.092	0.038	-0.139	0.087	0.113	0.027	0.082	0.743
Quality of family relations (z-score)	802	0.042	0.059	0.106	0.575	-0.037	0.104	0.723	0.101	0.103	0.325
Ex-commanders ties (z-score)*	832	-0.116	0.141**	0.071	0.046	0.063	0.067	0.345	0.062	0.062	0.320
Big men ties (z-score)	802	0.042	-0.102	0.109	0.350	-0.024	0.109	0.828	-0.047	0.112	0.672

Table A.11: Program impacts on components of the economic performance index

			Average Treatment Effects								
	N	Control Mean	Therapy Only			Cash Only			Both		
			β	SE	p-values	β	SE	p-values	β	SE	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Economic performance	832	0.061	0.083	0.113	0.464	0.024	0.113	0.835	0.213*	0.110	0.052
Profit 7d avg (USD, capped 99th)	802	26.583	2.516	4.113	0.541	2.035	4.386	0.643	5.181	4.180	0.216
Index of wealth: housing quality and assetss (z-score)	832	0.054	-0.005	0.107	0.961	-0.123	0.108	0.254	0.190	0.116	0.102
Total consumption last 2w (USD)	832	109.600	1.474	10.992	0.893	9.155	12.102	0.450	1.552	12.599	0.902
Savings stock (USD, capped 99th)	832	82.319	-1.700	15.762	0.914	-10.771	15.918	0.499	10.106	16.566	0.542
Business stock (USD, capped 99th)	832	135.963	-11.929	39.555	0.763	21.753	36.730	0.554	25.163	38.387	0.512
# Hours worked past 7d	832	27.091	7.119***	2.639	0.007	3.703	2.491	0.138	5.049**	2.558	0.049
Is sleeping in the strees now (indicator)	832	0.132	-0.010	0.029	0.727	0.017	0.029	0.553	-0.032	0.029	0.266

Table A.12: Robustness to alternative index construction

	Main Specification			Pooled composite			Pooled individual questions			Covariance-weighted		
	Therapy only	Cash only	Both	Therapy only	Cash only	Both	Therapy only	Cash only	Both	Therapy only	Cash only	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Antisocial behaviors	-0.204*	-0.082	-0.251**	-0.171**	-0.072	-0.220***	-0.203**	-0.079	-0.200**	-0.199*	-0.098	-0.265**
	(0.106)	(0.104)	(0.104)	(0.080)	(0.079)	(0.078)	(0.083)	(0.080)	(0.084)	(0.107)	(0.104)	(0.107)
Economic Performance	0.027	0.028	0.190*	0.058	0.054	0.179*	0.058	0.054	0.179*	0.104	0.055	0.174*
	(0.098)	(0.097)	(0.098)	(0.095)	(0.096)	(0.094)	(0.095)	(0.096)	(0.094)	(0.097)	(0.098)	(0.099)
Identity and values	-0.037	-0.102	0.100	-0.031	-0.047	0.093*	-0.044	-0.027	0.018	-0.063	-0.132	0.081
	(0.101)	(0.099)	(0.099)	(0.060)	(0.059)	(0.056)	(0.058)	(0.058)	(0.058)	(0.101)	(0.100)	(0.097)
Forward-looking time preferences	0.206**	0.000	0.174*	0.152*	-0.037	0.164**	0.152*	-0.037	0.164**	0.218**	-0.002	0.139
	(0.096)	(0.098)	(0.094)	(0.086)	(0.086)	(0.082)	(0.086)	(0.086)	(0.082)	(0.094)	(0.098)	(0.093)
Self-control skills	0.178*	0.049	0.119	0.173*	0.048	0.116	-0.061	-0.028	0.025	0.198*	0.100	0.142
	(0.103)	(0.103)	(0.100)	(0.101)	(0.100)	(0.097)	(0.078)	(0.078)	(0.078)	(0.106)	(0.104)	(0.103)
Positive self-regard/mental health	0.088	-0.031	0.207**	0.097	-0.039	0.317***	0.040	-0.028	0.262***	0.063	-0.067	0.188*
	(0.104)	(0.102)	(0.101)	(0.078)	(0.078)	(0.079)	(0.065)	(0.068)	(0.066)	(0.104)	(0.102)	(0.102)
Quality of social networks, (z-score)	-0.036	-0.013	0.109	-0.006	0.007	0.064	-0.132*	-0.104	0.019	-0.051	-0.003	0.127
	(0.098)	(0.096)	(0.094)	(0.050)	(0.049)	(0.048)	(0.075)	(0.072)	(0.067)	(0.098)	(0.094)	(0.094)
Substance Abuse	-0.058	-0.046	-0.102	-0.029	-0.033	-0.087	-0.029	-0.033	-0.087	-0.066	-0.047	-0.078
	(0.096)	(0.094)	(0.096)	(0.077)	(0.075)	(0.077)	(0.077)	(0.075)	(0.077)	(0.099)	(0.098)	(0.100)

Notes: We surveyed each subject twice at each survey round, averaging their composite subcomponent indexes for our main specification (columns 1–3). Columns 4–6 treat each survey round as a separate observation (up to 2 per respondent), with standard errors clustered by individual. Columns 7–8 return to aveaing the two survey rounds, but now instead of averaging the composite subcomponent indexes, we take a simple averae of all the survey variables within all the composite subcomponent indexes. Finally, Columns 10–12 covariance-weight the composite subcomponent indexes rather than take a simple average indexes are covariance-weighted rather than simple averages of the component indexes.

B Qualitative data validation

Out of concerns that concerned that our survey outcome measures, y^s, may be biased, we set out to validate some measures through trust-building and intensive observation, obtaining a validated measure y^v. Assuming y^v captures the true behavior, y^*, this allows us to estimate the degree and direction of bias. We summarize the approach, empirical strategy, and results here, with details in Blattman et al. (2016).

Approach to validation   Of more than 4,000 endline surveys in the first year following treatment, we randomly selected 7.3% and re-tested answers to six survey-based measures with two-week recall periods. We chose four potentially sensitive behaviors—marijuana use, thievery, gambling, and homelessness.

We used intense qualitative work—in-depth participant observation, open-ended questioning, and efforts to build relationships and trust—to try to elicit more truthful answers. Over several days of trust-building and conversation, plus direct observation, we tried to elicit a direct admission or discussion of the behavior.

We selected and trained eight of the study's most talented qualitative research staff as validators, all Liberians. In the ten days following the survey, a validator visited the respondent over four days, spending several hours each day in conversation and observation. Validators shadowed respondents as they went about their day, rather than conduct formal interviews. They raised target topics through indirect questions while chatting.

Validators developed techniques to foster trusting relationships and to build rapport: becoming close to street leaders; eating meals with subjects; sharing personal information (including similar acts they or their friends engaged in); and mirroring participants' appearance and vernacular as appropriate. Validators would also observe the respondent's behavior from afar, as well as converse with peers and family. The goal was to attain insider status, and thus reduce the chance of misreporting. The premise was that time, a focus on a small number of behaviors, and trust/rapport building would mean that respondents were less

willing, or feel less able, to deceive a more familiar person, who also knows them better. Validator also had the opportunity to clear up misunderstandings and get a more accurate assessment of the behaviors. By discussing sensitive behaviors openly, relating their own experiences and that of friends, validators sought to dispel any notion that certain answers are more desirable, or would result in any strategic gains.

Without knowing the respondent's survey response, y^s, the validators coded an indicator of whether or not the respondent engaged in the behaviors in the two weeks prior to the survey, y^v. The authors reviewed the evidence and the coding for every case. In general, we used a relatively high standard of evidence, only coding $y^v = 1$ for a direct admission of the behavior or persuasive statements that they did not engage in the behavior.[21]

If this technique simply reproduced the errors in the survey data, then the validation is little help. The key assumption is that four days of building trust and gathering extensive information, regarding just six behaviors, reduced experimenter demand and other biases correlated with treatment compared to responses during a 300-question, 90-minute questionnaire.

Nonetheless, y^v is not free from error. For instance, the requirement of a direct admission, the disruption in people's lives, errors in recall periods, or increased social desirability bias from scrutiny all undoubtedly led to systematic errors in y^v. These errors, however, are not necessarily correlated with treatment. This is possible, for example, because validators could have learned men's treatment status in conversation, and this could have biased their coding. Nonetheless, we designed the trust-building and evidentiary standards to minimize this risk.

Survey-validation differences   Of the 297 men we selected for validation, we found and validated 240 (81%). y^s and y^v are identical about 80% of the time for sensitive measures.

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[21]The validators only witnessed or received third-party evidence of the behavior in a fifth of cases, but neither was considered sufficient evidence for a final coding. Both had to be followed by questions confirming that the respondent also engaged in the behavior in the two weeks prior to the survey. In general, we used a relatively high standard of evidence, only coding $y^v = 1$ if the validator directly observed the behavior or the respondent directly admitted it.

As expected, however, $\bar{y}^s < \bar{y}^v$: The average person reported 1.21 sensitive behaviors in validation, and 1.12 sensitive behaviors in the survey.

Empirical strategy If we believe that the validation measure is closer to the true behavior, then one way to test for bias in the survey-based treatment effects is to take the difference $y_i^s - y_i^v$, our proxy of measurement error for person i, and regress it on treatment:

$$y_i^s - y_i^v = \beta_0 + \beta_1 T_i + \mu_i. \tag{2}$$

If $\beta_1 < 0$ for sensitive measures, then treated men were more likely to under-report bad behaviors, and our survey-based treatment effects may overestimate the decline in anti-social behaviors.

With a sample of 240, we estimate we are powered to detect average under- or over-reporting of at least 14%, and error correlated with treatment of 28%. Because of power concerns, we pay close attention to the sign, magnitude, and confidence interval for β_1.

Of course, the crucial assumption is that y^v is closer to the true behavior. This parallels the "no liars" and "no design effects" assumptions in list experiments. The assumption cannot be tested directly, but can only be argued on context and the quality of the approach.

Results for sensitive behaviors We estimate equation 2 in Table B.1, including block fixed effects.[22] For sensitive behaviors, almost none of the coefficients on treatment indicators or interactions are statistically significant. We see little evidence of the therapy inducing a desirability bias, and indeed the effects run in the opposite direction.

Indeed, looking at the index of four sensitive measures (Column 1), β_1 is actually greater than zero for therapy plus cash, implying that the impacts of therapy plus cash are, if anything, larger than the survey data imply.

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[22]That is, in equation 2 we actually estimate β_{0j} and $\tilde{\beta}_{0j}$, which is necessary to identify treatment effects when the probability of treatment assignment varies by block. The results without block fixed effects (not shown) are qualitatively similar.

Table B.1: Estimates of the correlation between treatment and measurement error

Covariate	$y^s - y^v$, sensitive behaviors. (N=239)				
	All (0 - 4) (1)	Marijuana (2)	Gambling (3)	Homeless (4)	Stealing (5)
β_o (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: The table reports the degree and direction of bias in our treatment effects. We assume that our measurement error does not vary by whether or not the individual engages in the behavior, which allows for a simple way to use β_1 to adjust our ITT estimates.