Cognitive behavior therapy reduces crime and violence over 10 years: Experimental evidence*

Christopher Blattman   Sebastian Chaskel   Julian C. Jamison

Margaret Sheridan†

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Abstract

Several small, short-term, or non-experimental studies show that cognitive behavioral-informed trainings reduce antisocial behaviors for 1–2 years. But there is no large-scale, long-term research on persistence. We follow 999 high-risk men in Liberia 10 years after randomization into: 8 weeks of low-cost therapy, $200 cash, both, or a control group. A decade later, men receiving therapy or therapy with cash were about half as likely as the control group to engage in various antisocial behaviors, including robbery, drug-selling, and street fights—far exceeding expert predictions. Impacts are concentrated in the highest-risk men, and most robust from therapy with cash.

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1 Introduction

A range of antisocial behaviors plague many cities—homicides, theft, street fighting, and drug selling. Fragile states also confront election violence, rioting, and rebellion. All this makes urban crime and violence one of the most costly and divisive issues facing neighborhoods around the world. Policymakers are searching for preventative measures, especially alternatives to coercive tools such as policing and imprisonment. This is especially true of low-income countries, which have limited police forces and few resources to imprison offenders.

Policymakers are increasingly turning to cognitive behavioral-informed therapies, or CBT. In the United States, CBT-informed training is quickly becoming one of the principal direct non-police responses to gun violence (Clark, 2010; Feucht and Holt, 2016; Abt, 2019). This approach springs from the idea that much violence is the product of poor decision-making and distorted thinking: people react in haste, fail to consider the consequences of their actions, or overlook alternative solutions. They may hold on to exaggerated and negative beliefs about rivals. Or they may have difficulty managing their emotions. Trainings informed by CBT have long tried to help people become aware of these harmful thoughts, and learn to think and react differently (Lipsey et al., 2007; Wilson et al., 2005).

More recently, two large-scale randomized trials suggested that the effects of non-clinical CBT 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. Group sessions were led by social workers rather than clinical psychologists. The study found that criminal arrests fell by about half during the program period, but that the effects dissipated shortly afterwards.

Around the same time, Blattman et al. (2017) studied the 1-year effects of an 8-week CBT-informed intervention in West Africa, with nearly 1,000 criminally-involved young men. The *Sustainable Transformation of Youth in Liberia* (STYL) program provided non-clinical group training in managing antisocial behaviors and adopting non-criminal identities. Within

a month of the program, a wide range of criminal and violent behaviors fell by about half compared to a control group. Like BAM, however, 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. One year later, the men who received both therapy and cash engaged in crime and violence at half the rate of the control group. The grants did not affect incomes in a sustained way, however. Rather than enabling the men to adopt a new career, the cash may have 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 (The Economist, 2019). However, many questions remain unanswered. There is little evidence outside the United States. And, more importantly, there is no long-term experimental evidence on whether these approaches can reduce violence.[1]

This study returns to the STYL sample in Liberia roughly 10 years after subjects completed the program to collect long term evidence on impacts.

2   Context

Liberia is a coastal West African nation with roughly 5 million people. It emerged from 15 years of civil wars and instability in 2003. At the outset of the study, in 2009, the country had enjoyed a fragile order for 6 years. Among the threats to peace, the government and UN mission were particularly concerned with poorly integrated ex-fighters and other young men involved in crime and drugs. They also worried about political violence, as high-risk men had launched riots, were growing involved in election violence, and had been targets for mercenary recruitment into West African wars (Blattman and Annan, 2016; Hoffman,

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[1]Meta-analyses find that CBT-informed programs reduce criminal recidivism, but the underlying studies mainly have small numbers of subjects who are typically only followed for about a year, especially among experimental studies (Lipsey et al., 2007; Wilson et al., 2005). There is growing evidence that simplified, CBT-informed therapy led by non-professional counselors can reduce depressive symptoms over 5–7 years, but this is a very different population and behavior (Baranov et al., 2020; Bhat et al., 2022).

2011).

The largest concentration of high-risk men was in the capital, Monrovia. Roughly a few thousand young men had turned to drug dealing, pick-pocketing, and armed robbery for their incomes. Brawls and knifings were also common among them. Only a third of men were ex-fighters. Rather, with peace and normalcy, Monrovia was beginning to experience the same problem as so many other large cities around the world—poor, disenfranchised youth drifting into illicit and violent careers.

3   Interventions

We studied two interventions, cash grants and non-clinical, CBT-informed therapy and training sessions. We used a 2×2 factorial design, producing 4 treatment arms: Therapy Only; Cash Only; Therapy+Cash; and a Control condition.

The cost of delivering Therapy+Cash was \$530 per person, inclusive of all implementation costs: \$189 for delivering the therapy; \$216 for the grant and distribution costs; and \$125 for program registration and administration. This cost is equivalent to about 8 months of the sample's earnings at baseline.

3.1   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. In clinical settings the approach has been widely successful at reducing symptoms of depression, anxiety, phobias, traumatic stress, and hostility (Beck, 1979, 2011).

CBT-informed therapies share two 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 thoughts, allowing them to respond to everyday situations in more constructive ways.

Second, sustained changes in behavior or symptoms also come from 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, they gradually change their behavior and thinking.

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

Their approach combines non-clinical 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. On alternate days when groups did not meet, facilitators visited men at home or work to provide advising and encouragement.

NEPI trained its own facilitators. None were formally trained psychologists. They had typically been involved in armed groups or crime earlier in their lives, and were mainly past graduates of a past NEPI rehabilitation program.

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. These tasks increased in difficulty over time.

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

planful and goal-oriented in their daily activities. Second, the program strongly emphasized how to deal with anger, interpersonal violence, and threatening situations. Third, and more unusually, STYL tried to help men learn to behave and self-identify as normal society members rather than as an outcast or criminal. Appendix A.5 elaborates.

3.2 Cash grants

Winners of the cash lottery immediately received $200 in cash. Losers received $10 as a consolation. Subjects were explicitly told that the grant was unconditional and they were free to do what they wished. They were also 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).

Despite this minimal framing, the cash arm was intended to give the sample the option to engage in petty trade or some other legal earnings opportunity, 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).

3.3 Hypothesized effects

Primary outcome: Antisocial behavior. 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 (Saini, 2009; Pearson et al., 2002; Wilson et al., 2005; Del Vecchio and O'Leary, 2004). Accordingly, prior to the 10-year survey, we preregistered one primary outcome: a summary index of 7 violent and criminal behaviors and attitudes: drug selling; stealing; interpersonal fighting; carrying a weapon; arrests; hostile attitudes; and domestic abuse.

Secondary outcomes. The therapy could influence antisocial behaviors through a variety of channels, and so we measured and prespecified 7 secondary outcomes as mechanisms:

economic performance; forward-looking time preferences; self control; anticriminal identity and values; positive self-regard and mental health; substance abuse; and the quality of social networks.[2]

Note that, for the 10-year survey, we did not specify economic performance as a primary outcome because of the absence of short-term effects. In principle, any increase in legal earnings could reduce criminality by increasing the returns to legal enterprises and raising the opportunity cost of antisocial behavior (Becker, 1968; Blattman and Ralston, 2015). Nonetheless, one year after treatment we found no direct effect of cash on earnings or consumption (Blattman et al., 2017). Employment and incomes had risen in the month following the grant, as the men started small petty business. Within a year, however, these impacts had dissipated. 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.

Expert predictions Experts were pessimistic about the long term results. To assess 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.

4 Experimental procedures

4.1 Target population, recruitment, and randomization

The study recruited 999 men actively involved in crime and interpersonal violence, aged 25 on average, who posed a risk of future violence. We focused on five mixed-income residential

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[2]See Social Science Registry *AEARCTR-0006736*. In general, we follow the 1-year study for index construction, covariate adjustment, and heterogeneity analysis.

neighborhoods in Monrovia with large populations of high-risk men.

All recruitment was handled by NEPI. In each neighborhood, certain places, groups, and professions had reputations for crime and violence involvement, and recruiters targeted these locations and people. Appendix A.1 elaborates.

We tried to minimize general equilibrium effects and spillovers between treatment and control group members. We worked in neighborhoods with tens of thousands of residents, recruiting less than 1 percent of adult men. NEPI recruiters were also instructed to approach just 1 out of every 7–10 potentially high-risk subjects they identified on the street. This avoided more than 10 percent of high risk men being treated in a neighborhood. Appendix A.2 elaborates.

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

After the final week of therapy, we re-contacted all 999 men and asked them to return for a second, surprise draw for the grants. Of those assigned to the grant, 98% received one. Assignment to the four arms was largely balanced along covariates (Appendix A.2).

4.2   Data and attrition

As one of the world's poorest and fragile countries, 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 disadvantage is bias in self-reported results, discussed 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. We ran pairs of surveys to reduce noise in outcomes with low autocorrelation, such as earnings or criminal activity. We average these pairs into "1-month", "1-year", and

"10-year" outcomes.

Subjects typically had no fixed address, often went by many aliases, or lived clandestinely. By collecting social network and contact information, and through 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: 17 refused; 7 were imprisoned; and 6 we could not find. Therefore we have survey data on 833 (83%) of the original sample—93% of known survivors.

Response rates varied slightly by treatment group, though none of the differences are statistically significant. Compared to the control arm, for instance, Therapy Only subjects are 4 percentage points less likely to be found, and Therapy+Cash subjects are 4 percentage points more likely to be found. Appendix A.3 elaborates.

5 Results

5.1 Antisocial behavior

Figure 1 displays antisocial behavior index levels and average treatment effects over time. We adjust the index to have zero mean at baseline and unit standard deviation over all survey rounds. Table 1 reports means and average treatment effects for the overall index and each of the 7 components in the 10-year survey round only. We estimate intent-to-treat effects by regressing outcomes on indicators for each treatment arm, a vector of baseline characteristics, and randomization block fixed effects (Appendix A.4) Given that we specified only one primary outcome, we do not adjust p-values for multiple hypotheses. We have seven major findings:

Self-reported 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. The main reason is likely to be mean reversion that arises from recruiting

people at their most violent, criminal, or desperate point. A second, less likely possibility is life cycle effects. In many countries, crime rates peak in the late teens and early twenties, and decrease thereafter. In this instance, however, antisocial behaviors did not decline between the 1- and 10-year surveys. This suggests that life cycle effects may be weak for this population—Liberian high-risk men already in the 20s at the time of first recruitment.

Both Therapy Only and Therapy+Cash cause sizable and statistically significant reductions in antisocial behavior, exceeding expert predictions. After 10 years, Therapy Only reduces the index of all 7 antisocial behaviors by 0.20 standard deviations compared to the control group ($p = 0.058$), and Therapy+Cash reduces it by 0.25 standard deviations ($p = 0.019$). 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 ($p = 0.467$).

Also note that the 10-year impacts are remarkably similar to the 1-year impacts. In the Therapy+Cash arm, antisocial behaviors declined 0.308, 0.245, and 0.246 standard deviations after 1 month, 1 year, and 10 years. In the Therapy Only arm, they declined 0.249, 0.075, and 0.201 standard deviations after 1 month, 1 year, and 10 years. None of the differences between these rounds are statistically significant (see Table 2 below).

While the Therapy+Cash estimate is larger and more robust, we cannot reject that it is equivalent to the effects of Therapy Only. Unlike the 1-year results, the difference between Therapy+Cash and Therapy Only is just 0.045 standard deviations, and not statistically significant. That said, we should note that the treatment effect on Therapy Only has a p-value above the typical threshold ($p = 0.058$). Also, unlike the Therapy+Cash result, the robustness of Therapy Only varies with the specification.

Appendix B.2 reports various robustness tests. The Therapy+Cash results are highly robust to changes in the control vector, methods of index construction, censoring of extreme values, and extreme various attrition scenarios (such as imputing extremely low antisocial

behavior among missing treatment group members and extremely bad behavior for missing control group members). In the Therapy Only arm, the results gain statistical significance when we choose control variables using the double lasso method or when we treat each round as a different observation. We did not use these models or approaches in the 1-year results, and so we do not report them as out main results. Yet the Therapy Only results become less significant in other models: when we omit control variables, for instance, and under some (but not all) extreme attrition scenarios (such as trimming for excess attrition).

The impacts of Therapy Only and Therapy+Cash are greatest among the very highest-risk men. Following the 1-year paper, we conducted only one heterogeneity analysis—by baseline antisocial behavior. We reprise the original analysis using a continuous measure of baseline antisocial behavior in Appendix Table D.1, and find that impacts are concentrated in the most violent and criminal young men at baseline. Again, results are larger and more statistically significant for Therapy+Cash, but we cannot reject their equivalence.

To illustrate the degree of concentration, Figure 2 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 ($p < 0.01$), while effects in the lower three quartiles are close to zero.

Therapy Only and Therapy+Cash seem to lead to large declines in several components of the index. We have to take this analysis with caution—as merely exploratory—not only because of the number of hypothesis tests, but also because not all impacts are statistically significant. To give a sense of what is likely to be driving the overall index, however, we summarize 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 (including 8 kinds of crime, from pick-pocketing to armed robbery.) In the control group, the total fell from 5.7 acts at baseline to 1.8 after a year and 1.9 after 10 years. After 10 years, men receiving Therapy Only reported 1 fewer crime each 2 weeks (-54%, $p = 0.027$) than the control group. Those receiving Therapy+Cash reported a decrease of 0.92 crimes (-49%, $p = 0.050$). These are conservative effects, for if we do not top-code this variable at the 99th percentile, the control mean and the treatment effects are about a third larger (not shown).

- *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, leaders, or police. For instance, the control group reported 2.21 physical fights at baseline, compared to 0.79 after a year and 0.80 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 8% of the control group said they carried a weapon on their body, increasing to 15% after a year, and decreasing to 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 ($p = 0.453$).

These impacts imply more than 200 fewer crimes committed per participant over 10 years—surely cost-effective by any measure. We know of no figures on the social cost of crime in sub-Saharan Africa, but some simple calculations make the cost effectiveness of the program clear. If we annualize the 2-week theft and robbery figures, the therapy arms each result in about 25 fewer such crimes per year after 10 years. These are similar to the 1-month and 1-year reductions of about 32 and 19 crimes per year (Blattman et al., 2017). We do not have data for intervening years, but assuming the impacts were relatively stable, this suggests at least 200 fewer crimes per participant. Ignoring any of the other antisocial behaviors (from drug selling to aggression) this implies a cost of less than $2.50 per crime averted given the $530 program cost.

Finally, we see no impact of the intervention on violent death. Tragically, roughly 11 percent of the sample died over 10 years. We collected cause of death data from two

friends or relatives of every member of the sample. The vast majority due to illness or injury, but about a quarter of deaths were violent, mainly due to mob killings of suspected thieves. Mortality, including mob violence, far exceeded our expectations, and so were not part of our prespecified measure. Nonetheless, violent death is an important potential long term outcome. We see no evidence that either treatment decreased the risk of violent or non-violent death. Appendix B.1 elaborates.

5.2 Impacts on secondary outcomes and potential mechanisms

We registered 7 mechanisms as secondary outcomes. Table 2 reports 1- and 10-year program impacts on the family indexes for each mechanism. Appendix A.6 describes the components and measurement of each index in detail and Appendix Tables D.2 to D.8 report treatment effects on the components of each family index.

We do not adjust for multiple hypothesis tests because, with 7 mechanisms, any p-value adjustment would reduce statistical significance below conventional levels. Despite the unusually large size of this study, it is underpowered to distinguish between many competing mechanisms. Thus, we should take these estimates with caution, again as largely exploratory. Nonetheless, consistent with the 1-year results, we see evidence that therapy (especially Therapy+Cash) is associated with positive changes in forward-looking and prosocial behaviors.

- *Time preferences.* We construct an index of 4 measures of patience and 4 of present-biasedness, where one of each comes from incentivized games. 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, 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 robust for patience than present-biasedness.

- *Self control skills.* We measured skills of self control using standard psychometric questionnaires for impulsiveness, conscientiousness, grit, and reward responsiveness. 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.

- *Anticriminal identity and values.* The therapy also tried to foster a change in the men's identity and associated norms and values. The family index includes: an index self-reported attitudes towards the appropriate use of crime and violence; an index of prosocial behaviors and group activities; and whether their dress and appearance are consistent with mainstream social identity. 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 an admittedly wide-ranging set of 6 mental health-related outcomes: neuroticism; self-esteem; locus of control; subjective well being; depression; and distress. 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.

- *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 the quality of social networks—the traits of the men's five closest peers, as well as closeness to family members, former rebel commanders, and criminal bosses. A family index exhibits little change after 1 year. After 10 years, there is little significant change from either Therapy Only (-0.026 standard deviations, $p = 0.814$) or Therapy+Cash (0.085 standard deviations, $p = 0.405$).

- *Economic Performance.* We created an index of economic performance from several measures: earnings, consumption, homelessness, savings, investment, and employment levels. As with the 1-year results, we still see no evidence that Cash Only affects economic performance (a 0.024 standard deviation increase, $p = 0.835$). This' arm's enterprises failed in the first months after the intervention and there is no short or long term impact of cash.

  There is, however, suggestive evidence that therapy increased long run earnings and employment, especially Therapy+Cash, potentially because any impacts emotional regulation, planning, and conscientiousness raise productivity and earning potential.[3] 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 treat-

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[3]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.

ment effects, but are not individually statistically significant. We will return to this theme in the discussion, below.

5.3 Relative influence of mechanisms

As an illustrative exercise, Figure 3 plots two estimates for each family index: the impact of treatment on each of the 7 potential mechanisms; and the coefficient from 7 bivariate regressions of our primary outcome (10-year antisocial behavior) on each potential mechanism. Any mechanism, to be influential, must by definition have sizeable values of both. Most of the 7 have moderate values in both, suggesting that the intervention likely operates through many of the potential channels.

5.4 Measurement error concerns

The majority of our data come 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. One possibility is experimenter demand effects. Another is that the intervention might have changed the way the study participants view themselves and their activities (and thus how they answer surveys), without changing actual behaviors. cWe cannot rule out either form of measurement error correlated with treatment. Three analyses suggest that this may not be present in our data.

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.[4]

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[4]That said, this is not true of all non-survey outcomes. Included in the measure of anticriminal identity and values is the enumerator's subjective impression of the subject's dress, cleanliness, and appearance. We see no evidence of a treatment effect on this outcome (Appendix Table D.4).

Second, recipients of therapy do not report treatment effects in many of the outcomes most 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 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 Appendix C elaborates.

Our qualitative work suggests two explanations. The men have been members of a subculture 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.

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. 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 show that antisocial behavior changes can be lasting, and that simplified, nonexpert-led therapy can be cost effective. Just looking at thefts and robbery alone, we estimate that every therapy participant resulted in at least 200 fewer crimes over the following 10 years—a remarkable impact given the low program cost.

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

Finally, there are indications that impacts are larger and more sustained when therapy is combined with one-time economic assistance—although, to be clear, the difference between the two arms is not statistically significant after 10 years. What could account for this pattern, if true? It's important to recall that cash by itself only raised employment and incomes for a few months, before the businesses collapsed, and so the cash arm did not alter the opportunity cost of engaging in crime. 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).

The role of boosters and economic assistance are important areas for future research. Based on the present results alone, however, it is unclear that implementers should double the cost of the intervention merely in return for slightly larger and more more robust treatment effects. One alternative is to seek cheaper ways of reinforcing the lessons (such as booster sessions). An alternative is to seek other low-cost means of economic assistance that lead to more sustained enterprise and incomes. Mobile banking, gradual cash transfers, or the availability of credit and insurance products (to cope with shocks) are all promising possibilities. That said, we estimate that the treatment arms would need to be 3–4 times larger than present to be powered to detect differences between arms that are less than 0.1 standard deviations—making distinctions between treatment modalities inherently challenging.

STYL's 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.

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. A large literature has shown that investment in childhood noncognitive skills predict long-run economic performance and criminal activity (Nagin and Pogarsky, 2004; Heckman et al., 2006; Borghans et al., 2008; Cunha et al., 2010). Absent any evidence, some scholars have been skeptical that self-investment or interventions can shape noncognitive skills and behavior in adult-

hood (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.

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Figure 1: Program impacts on a standardized index of antisocial behaviors over time — Levels and average treatment effects (ATEs)
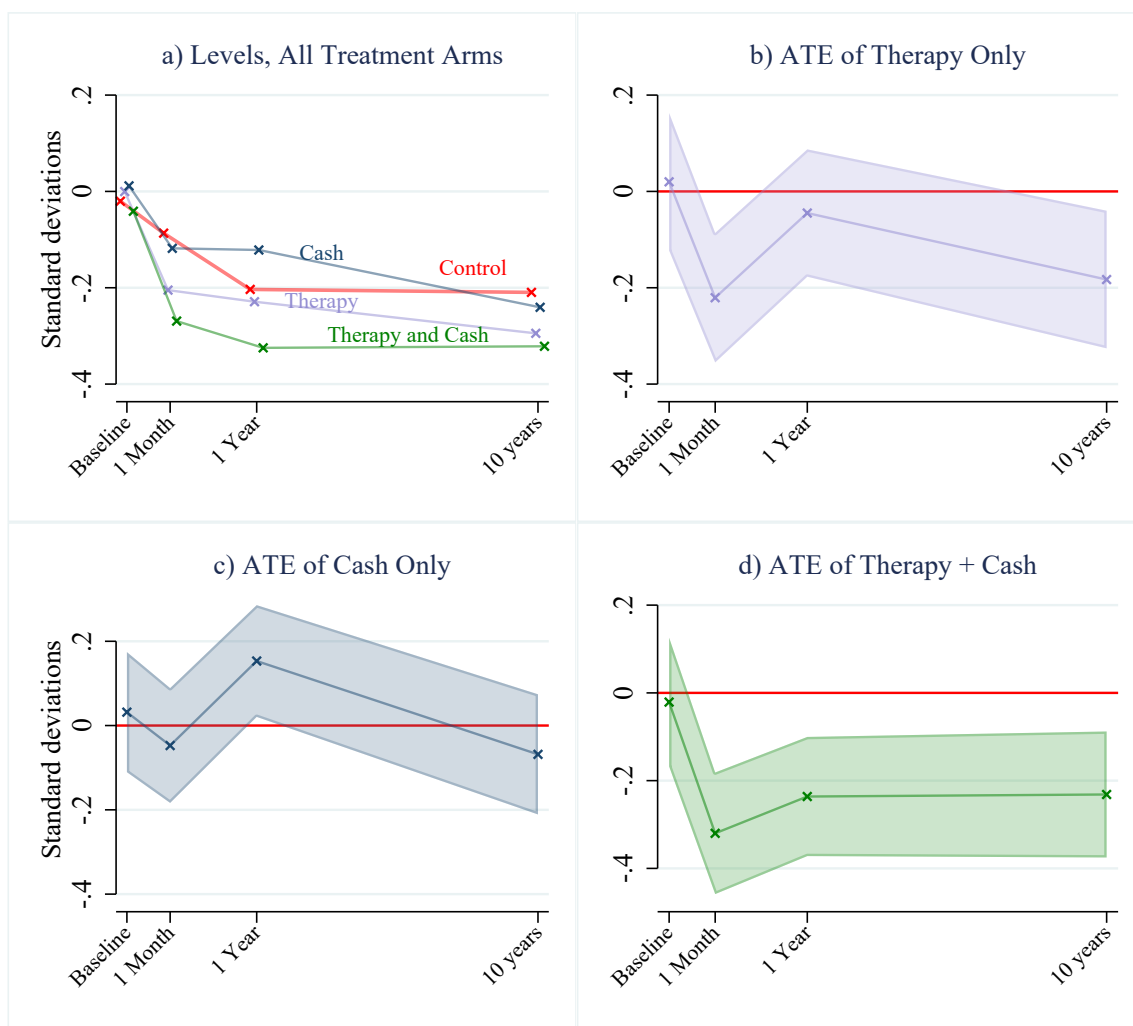
Notes: The estimates control for baseline covariates and randomization block fixed effects. The antisocial behaviors index is a composite of underlying survey variables, and here the index is standardized to have zero mean at baseline, and unit standard deviation across all survey rounds. Continuous and unbounded variables in each index have been top-coded at the 99th percentile to reduce the influence of extreme values. The 95% confidence intervals use heterosketastic-robust standard errors.

Table 1: 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)	0.116	-0.201*	-0.077	-0.246**	-0.045	-0.169*
	(1.122)	(0.106)	(0.105)	(0.104)	(0.635)	(0.081)
		[0.058]	[0.467]	[0.019]		
Usually sells drugs (indicator)	0.102	-0.016	-0.024	-0.046*	-0.029	-0.022
	(0.267)	(0.027)	(0.028)	(0.027)	(0.171)	(0.357)
		[0.542]	[0.390]	[0.092]		
# of thefts/robberies in past two weeks (count)	1.874	-1.006**	-0.274	-0.919**	0.086	-0.645**
	(5.147)	(0.416)	(0.452)	(0.452)	(0.782)	(0.049)
		[0.016]	[0.545]	[0.042]		
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 randomization block fixed effects, as in Equation 1 in Appendix A.4. Indexes are standardized to have zero mean and unit standard deviation. Continuous and unbounded variables in each index have been top-coded at the 99th percentile to reduce the influence of extreme values. 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.

Figure 2: Heterogeneity in program impacts by baseline antisocial behavior
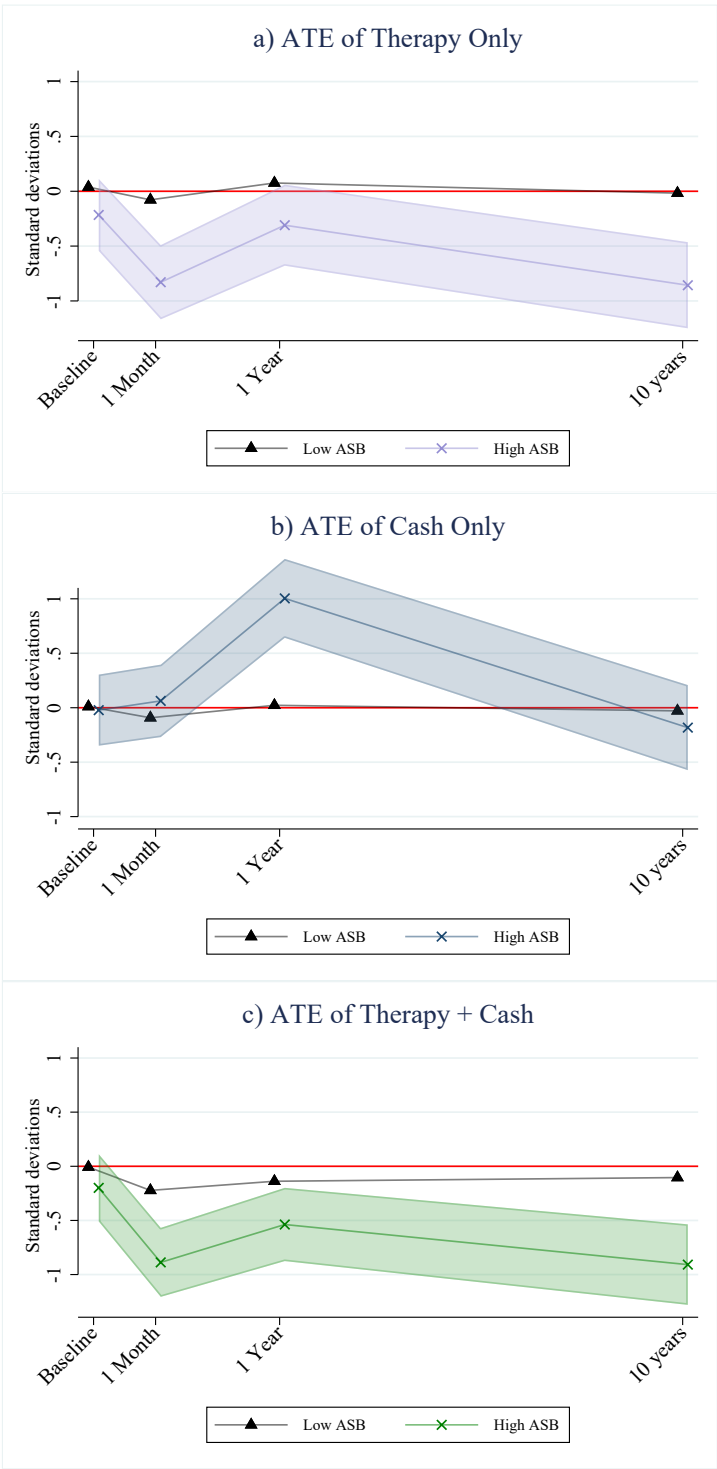
Notes: The figure reports intent-to-treat estimates of each treatment arm after 10 years for two subgroups: those with baseline antisocial behavior above the 75th percentile (high ASB) and those below it (Low ASB).

Table 2: 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 (z-score)	-0.075	0.137	-0.245***	-0.201*	-0.077	-0.246**	-0.105	-0.196	0.021
	(0.093)	(0.097)	(0.088)	(0.106)	(0.105)	(0.104)	(0.120)	(0.128)	(0.117)
	[0.419]	[0.155]	[0.006]	[0.058]	[0.467]	[0.019]	[0.379]	[0.126]	[0.860]
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	-0.039	-0.093	0.100	-0.072	-0.001	-0.007
	(0.093)	(0.092)	(0.092)	(0.101)	(0.099)	(0.099)	(0.124)	(0.123)	(0.120)
	[0.767]	[0.362]	[0.278]	[0.700]	[0.348]	[0.310]	[0.564]	[0.995]	[0.957]
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	0.058	-0.037	0.015	-0.026	-0.071	0.085	-0.128	-0.078	0.031
	(0.096)	(0.098)	(0.098)	(0.112)	(0.109)	(0.102)	(0.130)	(0.131)	(0.127)
	[0.543]	[0.708]	[0.879]	[0.814]	[0.517]	[0.405]	[0.327]	[0.552]	[0.806]

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 in Appendix A.4. All indexes are standardized to have zero mean and unit standard deviation. Continuous and unbounded variables in each index have been top-coded at the 99th percentile to reduce the influence of extreme values. Note that the 1-year results differ slightly from Blattman et al. (2017) to account for the incomplete top-coding of some components in that analysis (using the prior coding makes no material difference). Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

Figure 3: Which mechanisms are likely to be influential?

(a) Mediation Analysis on 'Both' Treatment Arm

(b) Mediation Analysis on 'Therapy Only' Treatment Arm
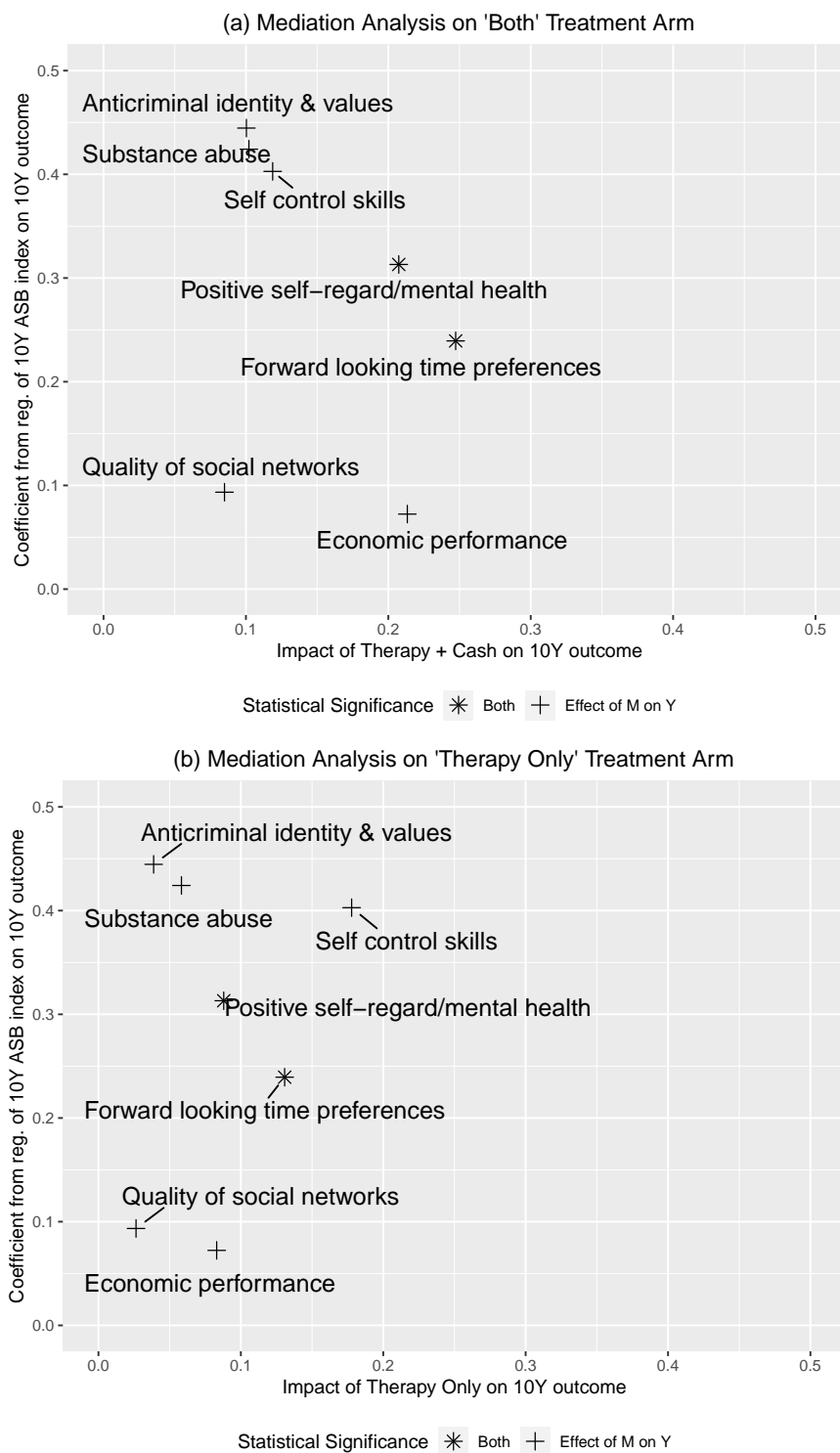
Notes: On the horizontal axis, the figure plots the average treatment effect of Therapy Only and Therapy+Cash on each potential mechanism—each one of the seven secondary outcome family indexes. On the vertical axis, the figure plots the relationship between each mechanism and 10-year antisocial behavior from a regression of the antisocial behavior index on the mechanism, baseline controls, and randomization block fixed effects. The estimates come from bivariate regression where each mediator variable is regressed individually (7 regressions by panel). For the sake of comparison, absolute values of the coefficients are displayed.

Online Appendix

A Experimental procedures

A.1 Target population and recruitment

We recruited subjects in five mixed-income residential neighborhoods in Monrovia with large populations of high-risk men. Within these neighborhoods, NEPI staff focused on areas known for criminal and antisocial behavior, including 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 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 the first intervention, therapy.

In the end, the study recruited 999 men. On average they 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.

A.2 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.

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

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 it.[5]

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 (see Table A.1). The excess therapy assignments are 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. (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. Here, in Table A.2, 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.

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

Table A.2: Summary statistics and randomization balance, 10-year surveyed sample only

	Sample Mean	Test of randomization balance(N=833)						F-Test
		Assigned Therapy		Assigned Cash		Assigned Both		
Baseline covariate		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.

A.3 Data and attrition

Each survey round was roughly 90 minutes long and delivered verbally by enumerators in Liberian English on handheld tablets. To measure time and risk preferences the respondents also conducted 45 minutes of incentivized games and tests.

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.

Table A.3 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). Otherwise, attrition is not particularly systematic. Looking at Column 2 Table A.3, for instance, few covariates have large or statistically significant effects on attrition.

Table A.4 reports the results of a single regression across four columns. The regression interacts each covariate with indicators for each treatment arm, to test whether the determinants of attrition vary by arm. Attrition in the control arm is greater among men with more war experiences and lower baseline economic performance, but these are generally reversed in the treatment arms where there is little evidence of selective attrition.

A.4 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.

A.5 Further intervention details

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

There were virtually no formally-trained psychologists or counselors in the country of Liberia at the time, and so all sessions were led by facilitators trained by NEPI. 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 main forms of change:

Table A.3: 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.

Table A.4: Attrition balance interacting treatment arm and baseline covariates

	Sample Mean	Control	Interaction Coeff. Therapy Only	Interaction Coeff. Cash Only	Interaction Coeff. Both	Differences Therapy Only vs Both	Differences Cash Only vs Both
		Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate [p-value]	Estimate [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Levels			-0.012 (0.169) [0.944]	-0.043 (0.168) [0.800]	-0.139 (0.155) [0.373]	-0.127 (0.387)	-0.096 (0.506)
Married or partnered, binary	0.158 (0.365)	0.072 (0.090) [0.426]	-0.039 (0.121) [0.744]	-0.014 (0.121) [0.909]	-0.079 (0.117) [0.501]	-0.039 (0.387)	-0.065 (0.506)
# of children<15 in household	2.209 (3.174)	-0.005 (0.007) [0.513]	-0.003 (0.011) [0.774]	0.009 (0.011) [0.415]	0.002 (0.011) [0.874]	0.005 (0.387)	-0.007 (0.506)
Years of schooling	7.719 (3.287)	-0.009 (0.011) [0.418]	-0.004 (0.015) [0.813]	-0.014 (0.015) [0.337]	-0.003 (0.014) [0.844]	0.001 (0.387)	0.011 (0.506)
Cognitive skill index, z-score	0.000 (1.000)	-0.018 (0.036) [0.612]	0.032 (0.049) [0.514]	0.013 (0.047) [0.781]	0.046 (0.048) [0.341]	0.014 (0.387)	0.033 (0.506)
Health index, z-score	-0.004 (1.002)	0.041 (0.029) [0.161]	-0.053 (0.041) [0.198]	-0.041 (0.036) [0.252]	-0.019 (0.036) [0.598]	0.034 (0.387)	0.022 (0.506)
Depression and distress index, std.	0.000 (1.000)	-0.037 (0.031) [0.235]	0.044 (0.045) [0.333]	0.039 (0.039) [0.315]	0.080* (0.044) [0.071]	0.036 (0.387)	0.041 (0.506)
Index of war experiences	-0.004 (1.000)	0.064** (0.028) [0.025]	-0.080** (0.040) [0.047]	-0.024 (0.040) [0.552]	-0.073** (0.037) [0.049]	0.007 (0.387)	-0.050 (0.506)
Index of economic success, z-score	-0.003 (0.999)	-0.053* (0.027) [0.051]	0.081* (0.045) [0.075]	0.006 (0.040) [0.871]	0.112*** (0.039) [0.005]	0.032 (0.387)	0.106 (0.506)
Savings stock (USD)	33.753 (67.388)	0.000 (0.000) [0.286]	-0.000 (0.001) [0.649]	0.000 (0.001) [0.841]	-0.000 (0.001) [0.474]	-0.000 (0.387)	-0.001 (0.506)
Hours/week in illicit activities	13.557 (27.253)	0.003 (0.002) [0.128]	-0.000 (0.002) [0.921]	-0.002 (0.002) [0.361]	-0.002 (0.002) [0.229]	-0.002 (0.387)	-0.001 (0.506)
Hours/week working	45.974 (43.231)	-0.002** (0.001) [0.020]	0.001 (0.001) [0.593]	0.001 (0.001) [0.369]	0.002* (0.001) [0.068]	0.001 (0.387)	0.001 (0.506)
Index of antisocial behaviors, z-score	0.001 (1.003)	-0.020 (0.039) [0.601]	0.027 (0.053) [0.618]	0.047 (0.050) [0.343]	0.027 (0.048) [0.580]	0.000 (0.387)	-0.021 (0.506)
Index of personality measures, z-score	0.000 (0.949)	-0.011 (0.031) [0.709]	0.016 (0.044) [0.715]	0.038 (0.040) [0.349]	0.006 (0.041) [0.880]	-0.010 (0.387)	-0.032 (0.506)
Patience total index (IBM) (0-6)	4.115 (1.979)	-0.024 (0.016) [0.134]	0.016 (0.023) [0.483]	0.021 (0.020) [0.306]	0.017 (0.018) [0.354]	0.001 (0.387)	-0.004 (0.506)
Joint F-Test		.149	.649	.931	.194		

Notes: This table regresses the main attrition outcomes (not surveyed) on a subset of the baseline variables, while also interacting them with each treatment arm. This allows the baseline covariate to affect attrition differently on each treatment arm. Column (2) displays the coefficients from the non-interacted control variable, wile Columns (3) - (5) display the interacted term for with the Therapy Only, Cash Only and Therapy+Cash treatment arm, respectively. The first row does not display an interaction estimates, but rather the estimates from the corresponding treatment indicator variable. Columns (6) and (7) test the statistical significance of the difference between the Therapy+Cash treatment arm and the Therapy Only and Cash Only, respectively. Finally, the last row presents a joint F-test of the column estimates, excluding the level estimates.

Changing social identity   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 personal 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.

Future orientation   In the middle and final weeks, 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.

NEPI offered no incentives to attend except for lunch, and the subjects were unaware of the cash grants program until after the therapy was complete.

Self control   Throughout the nine weeks, men worked on emotional regulation and anger management. During the group sessions, 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.

A.6   Measuring mechanisms

Time preferences   Becoming more self controlled and forward-looking are central components of many behavior change programs (Almlund et al., 2011; Borghans et al., 2008). We attempted to measure discount rates and time inconsistency (corresponding to β and δ preferences) in four ways:

- *Incentivized trade-offs.* Following the survey, subjects were asked to play a set of "real money games" where they had to make a series of intertemporal choices between money at one point in time versus more money later in time, with some probability of a payout. The average payout was about $3, roughly a day's wages. Based on game play, we assigned present and future patience scores for each respondent.

- *Hypothetical trade-offs.* During the survey questionnaire, well before the incentivized games, we asked respondents to make the exact same series of tradeoffs as above, but in a purely hypothetical setting. We constructed the patience and time inconsistency proxies in exactly the same manner.

- *Hypothetical discount rate.* We also attempted to measure the discount rate in a second way. As in Holt and Laury (2002), we asked respondents a series of hypothetical inter-temporal choices for larger amounts of money (on the order of US$10-30, about a week's wages) and calculated discount rates based on each respondent's first switch from a present preference to a future preference.

- *Self-reported survey questions.* We asked respondents six qualitative questions to gauge their self-reported levels of patience and time inconsistency. For example, respondents were asked to place themselves on a ladder from 0 (least patient) to 5 (most patient) as one measure of self-reported patience, and how much they agree with statements such as "When I get money, I spend it quickly".

Self control   We also measured skills of self control by adapting to the local context several standard psychometric questionnaires for four constructs that psychologists associate with less impulsive and more planful behavior:

- *Impulsiveness*, the inability to control thoughts and actions, using 9 questions from the Barrett Impulsiveness Scale;

- *Conscientiousness*, the tendency to be self-disciplined and purposeful, using 8 questions from the NEO-five factor personality inventory (Costa Jr. and McCrae, 1997);

- *Grit*, the ability to press on in the face of difficulty, using 7 questions on perseverance from the GRIT scale (Duckworth and Quinn, 2009); and

- *Reward responsiveness*, whether people are motivated by immediate (typically emotional) rewards, from the Behavioral Inhibition/ Behavioral Activation Scale (Robinson and Berridge, 2000).

Anticriminal identity 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.[6]

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

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[6]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.

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.

Mental health This family groups a set of 6 admittedly wide-ranging mental health-related outcomes in the interests of minimizing the number of families.

Half our mental health family index is realted to positive self-regard. Poor self-regard has been linked with many aspects of negative behavior and counterproductive or extreme risk-seeking behavior (Coopersmith, 1967). Some research suggests self-regard is captured by an interrelated set of psychological scales, including: (i) neuroticism, the tendency to experience emotional instability or anxiety, assessed with eight questions from the NEO-5 factor personality inventory; (ii) self-esteem; and (iii) locus of control, the extent to which individuals believe they versus fate control events affecting them, measured using eight questions from a standard questionnaire (Judge and Bono, 2001; Sapp and Harrod, 1993). Arguably related to positive self-regard, we also collected a classic happiness measure, asking men to rank their subjective well-being in absolute terms and relative to others in their community.

A second element of the mental health index is depression and distress. We assessed 6 symptoms of depression and 12 symptoms of posttraumatic stress (distress), based on a locally adapted instrument used previously with ex-combatant populations in Liberia (Blattman and Annan, 2016). We group this with positive self-regard as a mental health family in the interests of minimizing the number of families.

Social networks We measured the traits, positive and negative, of men's five closest peers.[7] We also asked about closeness to and support received from family members, former rebel commanders, and "big men" (intended to connote a criminal boss).

B Additional results

B.1 Violent death

As we saw in Table A.3 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 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

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[7]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).

Table B.1: 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.

1- and 10-year surveys. Changes in antisocial behavior may not be protective under these extreme circumstances.

Table B.1 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.[8] 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 B.2).

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 B.2). 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.

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 following section suggests that this is not likely to drive our main results.

B.2 Robustness

Appendix Table B.3 reports treatment effects from alternative models and outcomes in 7 columns:

1. The main results on from Table 1

2. Removing the top-coding of extreme values in the outcome components

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

Table B.2: Treatment effects on death types

	Sample Mean (SD) (1)	Violent Death Estimate (SE) (2)	Health complications Estimate (SE) (3)	Health complications: aggravated by drugs Estimate (SE) (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 1. 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.

3. Using an alternative, briefer vector of controls

4. Removing all control variables except for randomization strata fixed effects

5. Using a control vector selected using the double lasso (machine learning) method

6. Instead of one observation per respondent, averaged across the two 10-year survey rounds, we use two observations per respondent and cluster standard errors at the individual level

7. The antisocial behaviors index is constructed using covariance weights rather than equally weighting the component measures

Table B.3: Program impacts on antisocial behaviors index after 10 years: Robustness to different covariates and alternative index construction

	Main	No top-coding of ASB components	Subset of main control vars.	Fixed-Effects Only	Double lasso control vars.	2 obs. per individual	Covariance weighted index
	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Therapy Only	-0.201*	-0.204*	-0.176*	-0.183	-0.215**	-0.159**	-0.188*
	(0.106)	(0.106)	(0.103)	(0.115)	(0.104)	(0.077)	(0.106)
Cash Only	-0.077	-0.081	-0.071	-0.068	-0.119	-0.064	-0.089
	(0.105)	(0.104)	(0.103)	(0.114)	(0.103)	(0.079)	(0.105)
Therapy + Cash	-0.246**	-0.252**	-0.257**	-0.232**	-0.253**	-0.207***	-0.250**
	(0.104)	(0.104)	(0.110)	(0.115)	(0.103)	(0.076)	(0.107)

Notes: The table reports different robustness checks to the intent-to-treat estimates. Column (1) reports the treatment estimates of the main specification. Column (2) runs the main specification with a subset of 15 out of the 57 baseline control variables. Column (3) shows regress a modified version of the outcome variable where the ASB index components that are unbounded have not been top-coded at the 99th percentile. Column (4) shows robustness to eliminating randomization strata fixed-effects. Column (5) shows robustness to including exclusively such fixed-effects. Column (6) allows a double lasso method to choose a subset of the basline covariates. Column (7) treats each subject response as a separate unit of analysis, while clustering standard errors at such level. Column (8) construct the ASB index according to the covariance of the index component.

Extreme 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 B.4 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 (Lee bounds) in Column 5. 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

Table B.4: Attrition bound estimates for antisocial behaviors

	Sample Mean (SD)	Main reg (N=833) Estimate (SE) [p-value]	Impute .1 SD (N=999) Estimate (SE) [p-value]	Impute .2 SD (N=999) Estimate (SE) [p-value]	Trim (N = 393, 384, 383) Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)
Therapy Only	0.280	-0.201*	-0.155*	-0.119	0.021
	(0.450)	(0.106)	(0.090)	(0.090)	(0.092)
		[0.058]	[0.083]	[0.186]	[0.823]
Cash Only	0.250	-0.077	-0.049	-0.016	-0.050
	(0.433)	(0.105)	(0.088)	(0.089)	(0.122)
		[0.467]	[0.579]	[0.857]	[0.679]
Therapy + Cash	0.249	-0.246**	-0.207**	-0.182*	-0.241*
	(0.433)	(0.104)	(0.093)	(0.094)	(0.123)
		[0.019]	[0.027]	[0.052]	[0.051]

Notes: The table reports the intent-to-trest 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) implements Lee bounds—trimming (dropping) values as to artificially reduce the imbalance in missing values relative to the control group. The column summarizes the estimates from 3 different regressions.

highest-performing members of the Therapy+Cash arm until the two arms have equivalent attrition levels, then recalculates treatment effects.[9]

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).

C   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).

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[9]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. 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.

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.[10]

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

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[10]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.

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. 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 C.1, including block fixed effects.[11] 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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[11]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 C.1: Estimates of the correlation between treatment and measurement error

Covariate	$y^s - y^v$, sensitive behaviors. (N=239)				
	All (0 - 4)	Marijuana	Gambling	Homeless	Stealing
	(1)	(2)	(3)	(4)	(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 intent-to-trest estimates.

D Additional treatment effects analysis

Figure D.1: Average treatment effects on the components of the antisocial behavior index over time
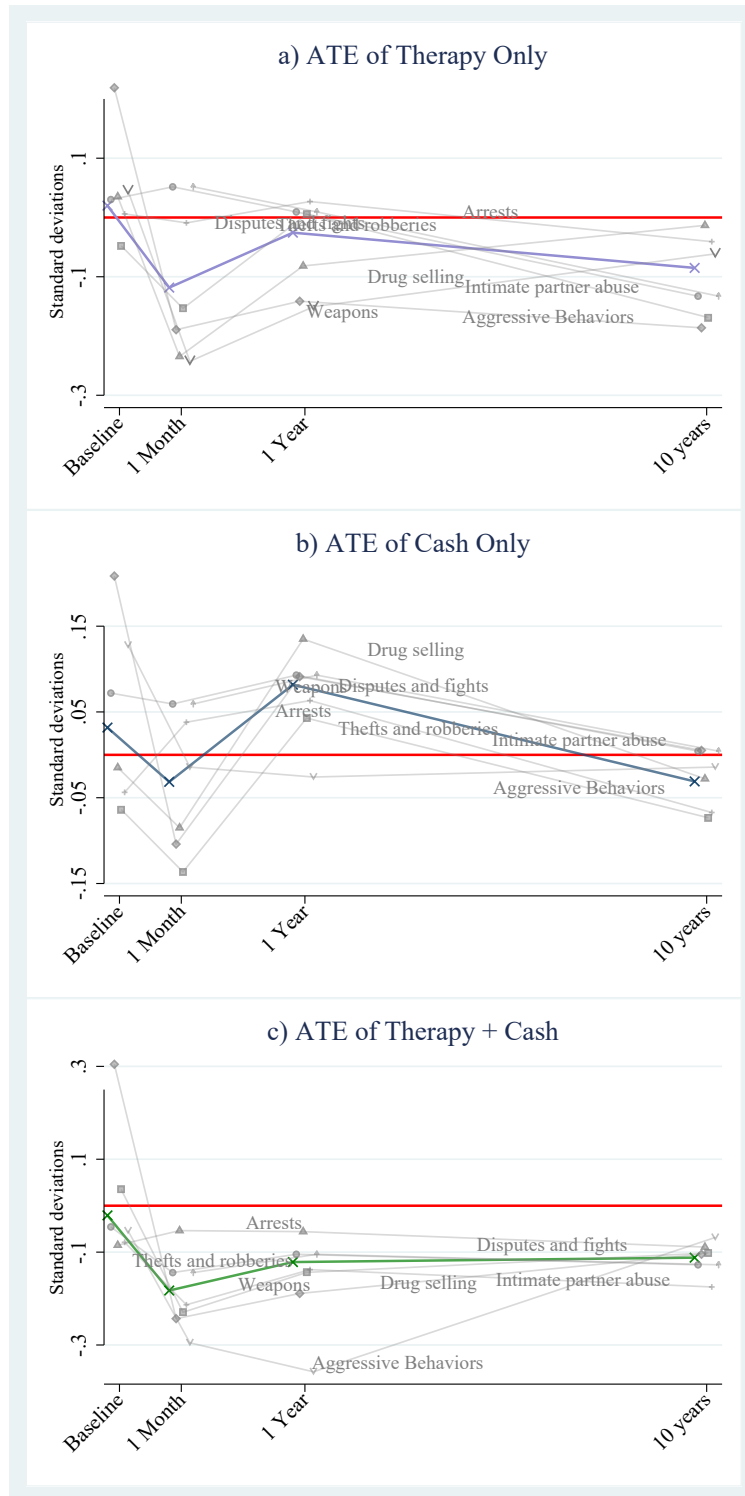
Table D.1: Heterogeneity in program impacts by baseline antisocial behavior (continuous, standardized)

	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)	-0.177* (0.102) [0.082]	0.045 (0.110) [0.687]	-0.041 (0.101) [0.687]	0.151 (0.092) [0.102]	0.134 (0.100) [0.180]	0.108 (0.100) [0.279]	-0.011 (0.110) [0.917]	0.004 (0.087) [0.963]
Cash Only	0.251 (0.434)	-0.064 (0.102) [0.530]	0.009 (0.110) [0.937]	-0.101 (0.097) [0.295]	0.015 (0.091) [0.873]	0.007 (0.100) [0.948]	-0.069 (0.099) [0.487]	-0.039 (0.104) [0.706]	0.015 (0.087) [0.859]
Therapy + Cash	0.264 (0.441)	-0.259** (0.107) [0.016]	0.224** (0.111) [0.045]	0.128 (0.102) [0.212]	0.266*** (0.091) [0.004]	0.104 (0.099) [0.295]	0.211** (0.100) [0.036]	0.110 (0.101) [0.278]	-0.066 (0.092) [0.473]
Therapy Only × Baseline ASB	-0.008 (0.476)	-0.343*** (0.099) [0.001]	0.156 (0.107) [0.145]	0.449*** (0.120) [0.000]	0.294*** (0.094) [0.002]	0.154 (0.094) [0.103]	0.220** (0.098) [0.026]	0.048 (0.121) [0.689]	-0.143* (0.085) [0.091]
Cash Only × Baseline ASB	-0.003 (0.506)	-0.141 (0.146) [0.333]	0.060 (0.101) [0.553]	0.198 (0.127) [0.119]	0.219** (0.091) [0.016]	-0.020 (0.096) [0.836]	0.034 (0.088) [0.698]	0.029 (0.121) [0.811]	-0.151 (0.095) [0.110]
Therapy + Cash × Baseline ASB	-0.010 (0.499)	-0.391*** (0.102) [0.000]	0.228** (0.106) [0.032]	0.304** (0.119) [0.011]	0.202** (0.092) [0.029]	0.146 (0.090) [0.106]	0.212** (0.090) [0.019]	0.145 (0.121) [0.230]	-0.185** (0.089) [0.037]
Baseline ASB	-0.024 (0.975)	0.462*** (0.075) [0.000]	-0.136* (0.081) [0.092]	-0.399*** (0.104) [0.000]	-0.262*** (0.061) [0.000]	-0.140** (0.069) [0.041]	-0.177*** (0.062) [0.004]	-0.083 (0.101) [0.413]	0.250*** (0.061) [0.000]

Notes: The table reports the same intent-to-treat estimates of each treatment arm after 10 years as in Table 1, interacting each treatment indicator with a continuous, standardized measure of baseline antisocial behavior (ASB) with zero mean and unit standard deviation. Heterosketastic-robust standard errors are reported in parentheses and p-values in brackets.

Table D.2: 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	0.321	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 D.3: 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	0.609	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 D.4: 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)
Identity and values	832	-0.017	-0.039	0.101	0.700	-0.093	0.099	0.348	0.100	0.099	0.310
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 4 indicator Q's.)	794	0.178	0.029	0.086	0.738	0.183*	0.095	0.054	-0.015	0.081	0.853
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 D.5: 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 18)	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 D.6: 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 D.7: Program impacts on components of the social networks index

	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	802	0.011	-0.026	0.112	0.814	-0.071	0.109	0.517	0.085	0.102	0.405
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)	791	-0.219	0.229**	0.106	0.031	0.085	0.097	0.382	0.098	0.091	0.286
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

Average Treatment Effects (spanning header over Therapy Only, Cash Only, Both columns)

Table D.8: Program impacts on components of the economic performance index

	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	2.034	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	9.156	12.102	0.450	1.552	12.599	0.902
Savings stock (USD, capped 99th)	832	82.319	-1.699	15.762	0.914	-10.770	15.918	0.499	10.106	16.566	0.542
Business stock (USD, capped 99th)	832	135.963	-11.928	39.556	0.763	21.757	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

Average Treatment Effects (spanning header over Therapy Only, Cash Only, Both columns)